Response to Editor Comments

Thank you to the editor for the constructive assessment of our manuscript and our response to the reviewers. In response to the remaining queries from the editor:

- Line 316: you mention that you repeat the calculations of fig. 2d-f using a range of grid resolutions, but I found it difficult to identify the resolution used to produce the original results shown in figure 2

Thank you for spotting this. We have clarified in the manuscript on line 321 and the figure 2 captions that the results on fig. 2 are “performed on a grid resolution of 1.9 km”.

- Line 425: “where the edges of the boxes represent” (also check figure captions)

We have updated the text which is now on Line 455, and the figure captions, to reflect this suggestion.

- Line 493: “past the grounding line” – specify whether this refers to locations upstream or downstream of the grounding line. In this paragraph, it may also be useful to mention that deformation offshore of the grounding line can be important for ice sheet stabilization if it causes regrounding of an ice shelf

Thank you for the useful suggestion. We have clarified on line 528 that this refers to locations “predominantly downstream of the grounding line”, and add on Line 530 that “However, it is important to note that deformation downstream of the grounding line can be important for ice sheet stabilization if it causes re-grounding of an ice shelf.”

(Below, we append our past response to reviewer comments for ease of reference)

Appendix: Response to Reviewer Comments

We thank the reviewers for their thoughtful and constructive comments, which helped us to improve our revised manuscript. We provide our response to reviewers' comments in the following section, leaving the reviewers' original comments in black text and our response in blue text.

Reviewer 1 (Dr. Samuel Kachuck) General Comments
The manuscript, "Resolving GIA in response to modern and future ice loss at marine grounding lines in West Antarctica" by Wan et al., addresses what computational grid is required to resolve the local sea level changes caused by solid-earth responses to changes in cryospheric loading, and how those compare to uncertainties in the solid-earth mechanisms (e.g., elastic vs. viscoelastic, poorly constrained viscosities). These questions are particularly salient for those engaged in describing and forecasting the sea level effects of changes in the cryosphere, particularly in areas, like West Antarctica, where such solid-earth feedbacks on the future dynamics of the ice are a significant source of uncertainty.

The authors find that the limiting factor for spatial resolution is the representation of the load, rather than the smoother solid-earth response, and provide a useful rule of thumb for determining what resolution is necessary to compute the elastic response of certain loads. They additionally show how uncertainties in the behavior of the mantle (in particular, the possibility of a low viscosity zone underlying parts of West Antarctica) can generate signal differences far exceeding errors caused by resolution, emphasizing how critical it is to include these processes in modeling and to encourage observational constraints of the mantle rheology.

The manuscript is well presented and I have only a handful of rhetorical suggestions to help with cohesion and flow, and one requested additional model run to resolve an apparent inconsistency in the comparison of the 3D rheologies with the 1D average rheology.

Finally, I would like to encourage the authors to release their model code source publicly, in accordance with the journal's data policy, to "guarantee integrity, transparency, reuse, and reproducibility."

Thank you for the summary of our study and findings. In regards to the final sentence, we agree that this is an important issue and appreciate the reviewer raising it. In our Data/Model Accessibility Statement, we note that Konstantin Latychev, the author and maintainer of the 3D model, can be contacted with questions about the code and requests for additional output, and he is also available to work with other groups to run whatever GIA simulation they are interested in. However, since this code has been developed over decades and applied by a range of scientists in numerous previous studies, our study is not the appropriate venue for sharing it publicly for the first time. Note that the coauthors on this study and other users and contributors to the code are actively engaged in community discussions and efforts to make GIA modeling and results increasingly accessible in future. For example, Gomez and colleagues organized an online seminar series over the last year on the theme of ice sheets, sea level and GIA in which GIA model benchmarking and accessibility was a central theme. Discussions on this topic continued at the PALSEA meeting and follow up action will be an ongoing community effort.

Specific comments
Comment on the 1D Viscosity: The vertically averaged GIA model you use to compare with the 3D viscosity models is for the whole of Antarctica and therefore not reflective of the viscosity underlying your region of interest (the Amundsen Sea). It makes it difficult for me to evaluate claims like the one made in Line 378 that there is a dependence on the 3D structure within the Amundsen Sea domain or in Line 404 that a 1D viscosity, rather than one with simply a long relaxation time, is closer to the purely elastic model. I appreciate that you quantify the importance of GIA from outside the Amundsen Sea (L313 and Supplemental S1) as being about 6% of the central response, and so might be hesitant to apply a lower viscosity 1D average to the globe. However, if you were to focus only on load changes in the Amundsen Sea, is there not any 1D viscosity profile or relaxation spectrum that, within other uncertainties, reproduces? This would greatly strengthen the cited arguments and the comparison you make in Figure 8.

We agree with the reviewer on the importance of distinguishing the impact of adding lateral variations in Earth structure from the impact of the lower viscosity upper mantle and thinner lithosphere of the 3D model in the Amundsen Sea compared to the globally averaged 1D model we considered. We note that the question of whether a 1D Earth model can represent the response of the Earth in a zone of complex Earth structure is explored in detail in a study in review by Evelyn Powell et al. titled "The robustness of geodetically-derived 1-D Antarctic viscosity models in the presence of complex 3-D viscoelastic Earth structure". To address this comment, we are performing an additional simulation adopting a 1D viscosity profile from the Powell et al. study that is representative of the low viscosity structure in the West Antarctic, and will add these results to Fig. 8. In the revised manuscript, we will revise the sentences mentioned by the reviewer to clarify this issue and refer to the new results.

Comment on ice dynamics and grounding lines: Though the paper's technical scope is focused on modeling local sea level changes due to solid-earth processes, part of the justification is that these sea level processes will affect the dynamics of the ice's grounding line, and are therefore necessary to model in tandem. I think a good place for this discussion could be surrounding the location of the grounding line under different resolutions and physical assumptions (e.g., elastic or viscoelastic mantle). And yet the final grounding lines in Figure 2, 4, 6, and 7 doesn't appear to change at all from case to case - the variation in the sea level (almost 20 m near the grounding line in Figures 2i,f) doesn't seem to affect where ice is floating. Is that right? In contrast, when we included the ice dynamics, we found a lag in a grounding line position of about 30 km after 100 years for a similar amount of sea level fall at Pine Island (Figure 3b in Kachuck et al., 2020). Considering this will significantly affect the resolution error near the grounding line that you show in Figure 9, and I think is worth discussing.

The goal of this study is to isolate the influence of grid resolution on GIA model predictions, and our experiments are therefore based on a stand alone GIA model with a prescribed ice model input at varying resolutions, rather than a dynamic ice model that responds to the ongoing sea level changes. Hence, we do not incorporate a coupled ice sheet-sea level model run and analysis of how the modelled grounding line is affected by the solid earth response from GIA predictions is outside the scope of this paper.

We further clarify this by adding the following to the methods section of the revised manuscript: *"Note that we adopt a stand alone GIA model throughout this study with the purpose to inform the set up of coupled ice sheet - sea level modeling studies, but we do not model the feedback of GIA on ice sheet dynamics explicitly. However, other work (e.g. Kachuck et al., 2020; Gomez et al., 2015) suggests that the scale of differences in GIA simulated at different resolutions and adopting different Earth structure models here will be large enough to alter the timing and magnitude of grounding line migration in a coupled modeling context."*

Line-by-line
L35: Instead of "consists of… and…", you might indicate that a viscoelastic material's response begins as that of an instantaneous elastic solid, and transitions to a longer-timescale viscous-like relaxation. For heuristic and analytic purposes we sometimes treat them as separable and additive.

Thank you for this suggestion, we have incorporated it into the text.

L60: "Viscous effects due to ongoing ice loss … have not been included in recent high resolution coupled projections": You could cite Kachuck et al (2020), Coulon et al. (2021), or De Conto et al. (2021) as recent examples of coupling ice dynamics to viscous vertical land motion in projections. The first has high spatial resolution, with 500 m grid spacing near the grounding line, for projections with viscoelastic feedback of Pine Island Glacier in the Amundsen Sea Embayment. The others are Antarctic-wide and determine that the ELRA model, and modifications thereof, are satisfactory for constraining large-scale sea levels.

Thank you for suggesting these very relevant papers, we have included this in the text to reflect recent developments in high resolution coupled modelling.

L246: An image of the ice load and topography would really help in understanding this description.

We will incorporate a figure of this in the supplementary material

L255: The Scaling Factor used in Figure 3b is not introduced in the main body text and doesn't appear again in the results focused on the Amundsen Sea. I would recommend integrating it more (see next note). Additionally, "scaling factor" is used previously to refer to the construction of viscosity profiles from seismic data (e.g., L179), muddling the usage. Another term might make this clearer.

We are grateful that the reviewer has pointed out the overlap in usage of the "scaling factor" terminology, we have renamed the scaling factor used in figure 3b to "mass factor". We have also incorporated further discussion of the Fig 3b results in the concluding paragraphs of section 3.1.

L322: I could use more elaboration here on the differences between representing the ice load and computing the elastic response, and maybe some tie-in with the Idealized Experiments results. Is the idea that we need finer resolution for better representation of the grounding line (i.e, where the ice load disappears), but not necessarily for the **smoother earth response,** which is consistent with your findings in the idealized experiment? If so, could you use the Scaling Factor from Figure 3b to explain this? In those earlier experiments, though, am I right in understanding that you didn't have any ice-ocean interaction? Does the ocean load smooth that boundary?

In response to the latter half of this comment, that is correct, as further emphasized above, the experiments run in this study are stand-alone GIA model simulations with no ice-sheet interaction. But the reviewer is correct in pointing out that high resolution is necessary for better representation of where ice loading is occuring, and less for capturing the smoother response of the solid earth. We appreciate the way the reviewer has articulated the difference and have adjusted the text to adopt this wording.

L353: It is hard for me to visually evaluate this statement across the different scales of the colorbars.

We have adopted a consistent colour bar for ICE-GOL and ICE-SH respectively for the elastic and viscoelastic sea-level pattern Fig. 2. Unfortunately for ICE-RD a consistent colour bar would mean that some of the detail behind the sea level pattern would be lost, so we have decided to keep the different scale for Fig. 2f vs 2i. The difference in magnitude of the sea level fall between 2f and 2i is evident in the large difference in colour bar range.

L369: "A viscous process" is vague. Could you specify that this is a result of assuming viscous incompressibility, or whatever other viscous consequence you are referring to?

Thank you for pointing this out, we have incorporated the suggested clarification.

L423: This would lead me to conclude that high resolution is not important for coupling viscoelastic deformation to grounding line motion. Do you believe that to be so? Are there conditions you can foresee in which resolution would play a larger role?

Yes we believe this would be the case unless there is a very significant and spatially isolated ice loss. Otherwise, where resolution would play a larger role would be when we have achieved GIA model prediction of such high accuracy from incorporating other factors mentioned that the error due to grid resolution becomes comparatively significant. We also want to emphasise that while analysis of the effect of high resolution on ice dynamic models is outside the scope of this paper, we expect resolution to play an important role in ice dynamic models in some contexts based on the existing literature. We discuss this in the introduction.

L465: Although this feedback on grounding line dynamics is not addressed in this study.
Yes, we agree that future studies with coupled ice sheet - sea level models can further elaborate on this.

L489: see comment to L322. Clarification there will also clarify here.
Thank you, we clarify this point in and adopt the suggested phrasing in the comment to L322 as well.

Technical corrections
L136: "structure. which"

L475: “reflecting” -> “reflect”
L481: missing word in “over time our simulations”

We are grateful that the reviewer caught these technical errors.

Comments on Supplemental material
L36: The referenced plot is titled “ASE_10km-ASE_1km” rather than “ANT_10km-ASE_ANT.”
Thank you for pointing this out, we have edited the text.

Caption to Figure S1. The grounding line appears to be a shade of green rather than the red as described in the text (which will probably be very difficult for individuals with red-green color-blindness).
Thank you for pointing out this error, we have addressed it in the caption.

Figure S2.2a) It looks like the recorded minimum of -3.21 m sea level occurs outside the domain, given the scalebar.

It falls just outside the domain of the plot.

Figure S2.2b) max and min are written as negatives, but look on the scalebar as positive.
Thank you for pointing this error out. We have addressed it in the figure.

Citation: https://doi.org/10.5194/tc-2021-232-RC1

Reviewer 2 (Dr. Grace Nield) Comments:

Summary
“Resolving GIA in response to modern and future ice loss at marine grounding lines in West Antarctica” by Wan et al.
In this study the authors seek to find out what GIA model resolution is required to accurately capture viscoelastic deformation in response to present-day and future ice loss in the Amundsen Sea Embayment of West Antarctica. They conduct several experiments to determine this: 1) the elastic response to an idealised cylindrical load; 2) the elastic response to realistic ice loss; 3) viscoelastic response to realistic ice loss with a 1D earth structure and three 3D earth models. The results are given in terms of percentage error for each grid resolution tested and the authors find that errors converge at a resolution of 3.75 km, or three times the radius for the idealised experiment. Furthermore, the results show that the error from neglecting viscoelastic deformation over short time scales, or from adopting different 3D earth models, is far larger than the error from grid resolution. This topic is timely given the efforts within the GIA community to improve models. High-resolution models are very computationally expensive, prohibitively so for most, and quantifying the error for different grid resolutions is very valuable. The paper is well written and clearly organised but would benefit from some small amendments to the text as suggested below.

General Comments
1) The main conclusions from this study state that the GIA model grid resolution needed for an isolated cylindrical load is three times its radius, and that for realistic ice loading in the ASE a grid resolution of 3.75 km is required to achieve an error of 2%. I think the paper could be improved by adding a qualitative discussion of how these findings might apply in other areas. Line 344 states “for most applications, errors of less than 5% can be achieved with a 7.5 km grid, and errors of less than 2% with a 3.75 km grid”. It would be beneficial to have a short section in the discussion expanding on this, discussing whether this rule of thumb is limited to ASE, or marine grounding lines, whether it might apply in other areas of Antarctica where present-day ice change is occurring, or how different spatial scale of ice loading change might affect this general rule.

Thank you for the insightful suggestion. To address this, we added to the Discussion section that “*Whilst we focus on the ASE where there is both ongoing and expected future ice loading and complex Earth structure, since the resolution error is primarily associated with the representation of the ice load, the general conclusions on the resolution required and results of the sensitivity* *experiments can be applied to any area of active, localised ice loading, for example in other parts of Antarctica, in Greenland or in the vicinity of smaller glaciers."*

2) In this study the authors find that representation of the ice load on the GIA model grid leads to higher error than the grid resolution itself, however, there is no discussion of how representation of the Earth structure within the model grid might impact the results. Particularly in Section 2.2 and 2.3, what is the resolution of the seismic tomography data that is used? How does this compare with the vertical resolution of the grid with depth, how much is it being down sampled? Perhaps the results from the tests mentioned on line 158/159 could be included in the supplementary information.

This is an important consideration. We have added the following text to the methods section to provide further detail on the resolution of the Earth models used: *"ANT-20 is provided on a 25 km grid at the surface, with distance between the points decreasing with depth, and provided at depth slices in 5 km intervals from 0 to 800 km depth. S362ANI is a 3-D global anisotropic shear wave velocity model for the whole mantle, extending from 25 km depth to the core-mantle-boundary defined at 2891 km depth, provided at a 2° lateral grid resolution at the surface. We note, however, that the spacing of the seismic model grid is distinct from the scale of the Earth structure variations captured by the model. While quantitatively assessing the latter remains an outstanding challenge in seismic tomography, we expect the resolution of the ANT-20 model to be O(>100 km) in the upper mantle and coarser at greater depths, and thus well represented by the GIA model grid."*

We have also added a discussion of this issue in section 5.2: *"Finally, we note that the required resolution of the GIA model grid will depend on the resolution of the seismic model used to construct the 3D Earth structure model. Earth structure is currently resolved in seismic tomography models in Antarctica at length scales of O(100 km) or greater (Lloyd et al., 2020; Lucas et al., 2020), but as further improvements in the resolution of seismic tomography emerge, variations in earth properties at even shorter spatial scales may be revealed and need to be represented in GIA models. However, given the smooth nature of the GIA response, we expect that the wavelength of ice loading variations will remain the determining factor of surface GIA model grid resolution requirements."*

Specific Comments

Line 21: The authors repeatedly refer to a "spatially isolated load". The use of this term is particularly important since once of the main conclusions of the paper – a grid resolution needed of 3 times the radius of the load – only holds for cylindrical loads. For clarity, consider changing this term to "spatially isolated disc/cylindrical load".
Thank you for bringing this point up - In discussion of the rule of thumb we pointed out again that we consider only cylindrical loads as follows: *"The results <u>from this analysis of spatially isolated cylindrical loads</u> provide a rough estimate of the magnitude of error one can expect from a given model resolution and loading scenario, and can serve as a guide for selecting the appropriate grid resolution for a given problem."*

Line 57: please quantify short spatial scales, e.g. 10s of km, or 100s of km.
We have quantified short spatial scales as less than 100s of km.

Figure 1c: is the white area saturated? Please add to colour bar
The white area is not an area of saturation. Instead, it is a region with no data as the thick lithosphere in East Antarctica extends down to > 200 km depths. We have clarified this in the figure caption.

Figure 1d: scale bar in the figure is not clear. The choice of colour bar makes it difficult to see the regional variation in viscosity.
Recognising these constraints, we chose to retain this colour bar to remain consistent with Fig. 1c. Each discrete colour represents ~0.2 log(n2d/n1d) difference.

Line 151: 70 radial layers – Are these radial layers where properties are assigned, or is this the vertical resolution of the grid? On line 153 it says layer boundaries at 12, 25, and 43km, again are these the boundaries of material properties or grid resolution?
We designed the grid to ensure there are radial layers on every layer where material properties are assigned in the 1D reference model STW105. We have clarified this further in the text, stating *"The*

radial layers of the grid are defined to respect the unconformities in material properties of the radially varying (1-D) seismic reference model STW105 (Kustowski et al., 2008), with the shallowest layers at 12, 25 and 43 km depth".

Line 157: 29 million nodes is a lot! It would be interesting to add here what the run time of this model is.

Our simulations take ~ 65 CPU hours (30 minutes on 134 nodes) to run per time step on a high performance computing cluster. We have added this information to the text here.

Figure 3a: consider adding solid/dotted/dashed lines to the key for the 2/5/10km ice, or perhaps label.
Figure 3b: x-axis label - Grid Resolution km, not m?
Figure 3b-e: y-axis would look better labelled at 2km intervals rather than 2.5 km intervals.
Thank you for the great suggestion and for pointing out the error in the figures. We have made the suggested changes for Fig. 3b

Line 260: "grid points" do you mean nodes? Is the load applied at the nodes or the centre of the elements?
Yes, we are referring to grid nodes and we have clarified this point in the text. The centre of the cylindrical load on an arbitrary model grid node in the ASE (76°S 150°W).

Line 292: "serve as a guide…. appropriate grid resolution for a given problem" I think its important to include here that this guide is restricted to isolated cylindrical loads.
As mentioned above, we have highlighted further here in the text that these insights are based on experiments with isolated cylindrical loads.

Line 335-344: There is reference to Figure 5(a) and 5(b) in this paragraph but there are no corresponding panels in figure 5. Fig 5 caption also only states (a).
Thank you for pointing this out. We had a Fig. 5b in a previous iteration and neglected to edit it out in the text, this has been addressed.

Figure 6: (and 2/4/7) it would be useful to include the acronyms ICE_SH etc referred to in the text in the column headings of these figures.
Thank you for this suggestion, we've incorporated it to increase the ease of interpreting our figures 2,4,6,7.

Line 397: This reference is missing from the list.

Figure 8: panels b and c need y-axis labels; the grey colour showing ice thickness in the lower panels is washed out and hard to see. In the caption for panel a) this is not identical to figure 3c, perhaps figure 2 instead.
Thank you for spotting these mistakes, we've addressed these in Fig. 8.

Line 402: It is confusing to label these two sites A/B since the panels in the figure are labelled b and c. Perhaps change to 1/2 or X/Y, since I think this is the only time they are referred to anyway.
Thank you for the suggestion, we have incorporated it in the text.

Line 403: it is not clear what the authors mean by "spin up time".

This is further clarified with added reference in the revised manuscript: "... represents a time of hindcast spin up to the year 2000 in the ice sheet model simulation (see DeConto et al., 2021) rather than…"

Figure 9: the hollow diamonds don't show up very well on this figure.

Thank you for pointing this out, unfortunately due to the density of the points there is inevitable overlap which makes the hollow diamonds hard to discern. They only occur outside the "whiskers" of the plot.

Line 560: add "in the ASE" to the first sentence to clarify the region the conclusions apply to.

We’ve addressed this in the text.

Line 564: would be useful to quote the percentage error in conclusion (1)
We’ve addressed this in the text.

The following paper is relevant and worth citing:

Blank, B., Barletta, V., Hu, H., Pappa, F., & van der Wal, W. (2021). Effect of lateral and stress-dependent viscosity variations on GIA induced uplift rates in the Amundsen Sea Embayment. Geochemistry, Geophysics, Geosystems. https://doi.org/10.1029/2021GC009807

Technical Comments
Line 158: missing word “limited to the surface (and?) a few layers down to 10km”
Line 211: incorrect spelling of adopted
Line 475: reflect rather than reflecting
We are grateful that the reviewer caught these technical errors.

Citation: https://doi.org/10.5194/tc-2021-232-RC2