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Importance of ice elasticity in simulating tide-induced grounding line variations along prograde bed slopes
by Natalya Ross, Pietro Milillo, Kalyana Nakshatrala, Roberto Ballarini, Aaron Stubblefield, Luigi Dini

Response to Reviewer 1

Blue font: comments from Reviewer 1
Black bold font: Authors' responses to the Reviewer's comments

Note: The line numbers referenced here correspond to the revised manuscript **with** highlighted corrections.

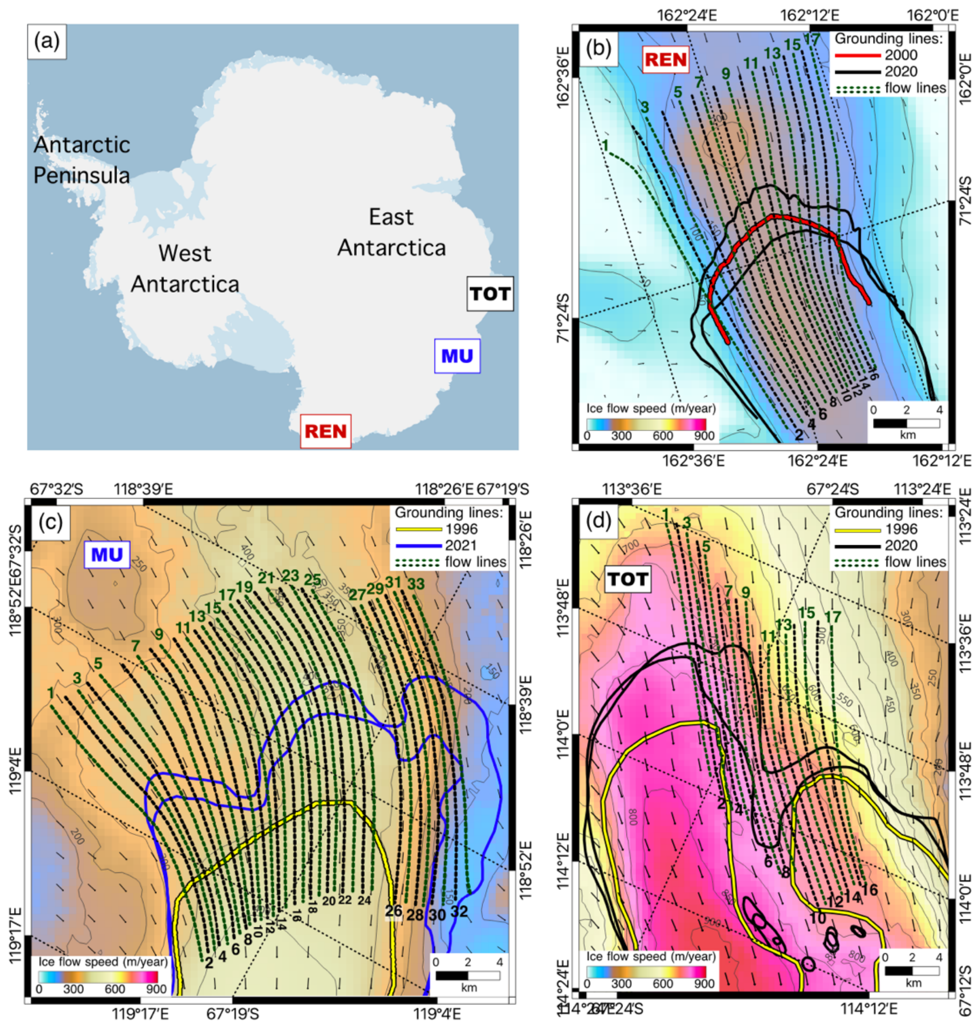
GENERAL COMMENT:
I would like to begin by thanking the authors for addressing the comments and feedback I provided during the first review cycle. I recognize that this likely required considerable effort, and I appreciate their responsiveness. The manuscript has shown significant improvement since its initial submission. The message is now much clearer, and the reading experience is considerably smoother. The manuscript presents a satellite-derived estimate of grounding zones along the Sabrina Coast in East Antarctica, utilizing DInSAR techniques. The authors also compare their results with a numerical model, examining the elastic and viscoelastic components of ice flow dynamics. I have no major concerns at this stage, but I do have a few minor comments on the figures. Assuming these are addressed, I would recommend the manuscript for publication in The Cryosphere.

We sincerely appreciate the Reviewer's valuable feedback. We have made every effort to thoroughly address the previous comments and firmly believe that incorporating the current suggestions has further strengthened our manuscript. Below, we provide a detailed response to each comment, along with a description of the corresponding revisions.

Figure 1: I find the geo-referenced figures a bit confusing. While I understand that the coordinates are expressed in polar stereographic, I wonder if these numbers may be difficult to interpret for readers who are not familiar with this reference system. Would it be possible to present the coordinates in latitude and longitude instead, as this might be a clearer choice? Additionally, I have some concerns about the rectangles in panel (a). I think they should be meant to represent the zoomed-in areas shown in the subsequent panels? It doesn't seem that they do.
Figure 2,3: Although the authors appear to be showing the same zoomed-in area as in Figure 1, the geometry seems distorted due to inconsistencies in the size of the inset boxes. For instance, panel (c) in Figure 1 is noticeably larger than panels (b) and (d), but this size disparity changes again in Figures 2 and 3. In my opinion, maintaining consistent sizing across all figures would help readers more easily recognize that the same area is being depicted throughout. Additionally, the comment I made regarding the coordinate labels in Figure 1 is also relevant to Figures 2 and 3.

We address the comments regarding the three figures collectively:

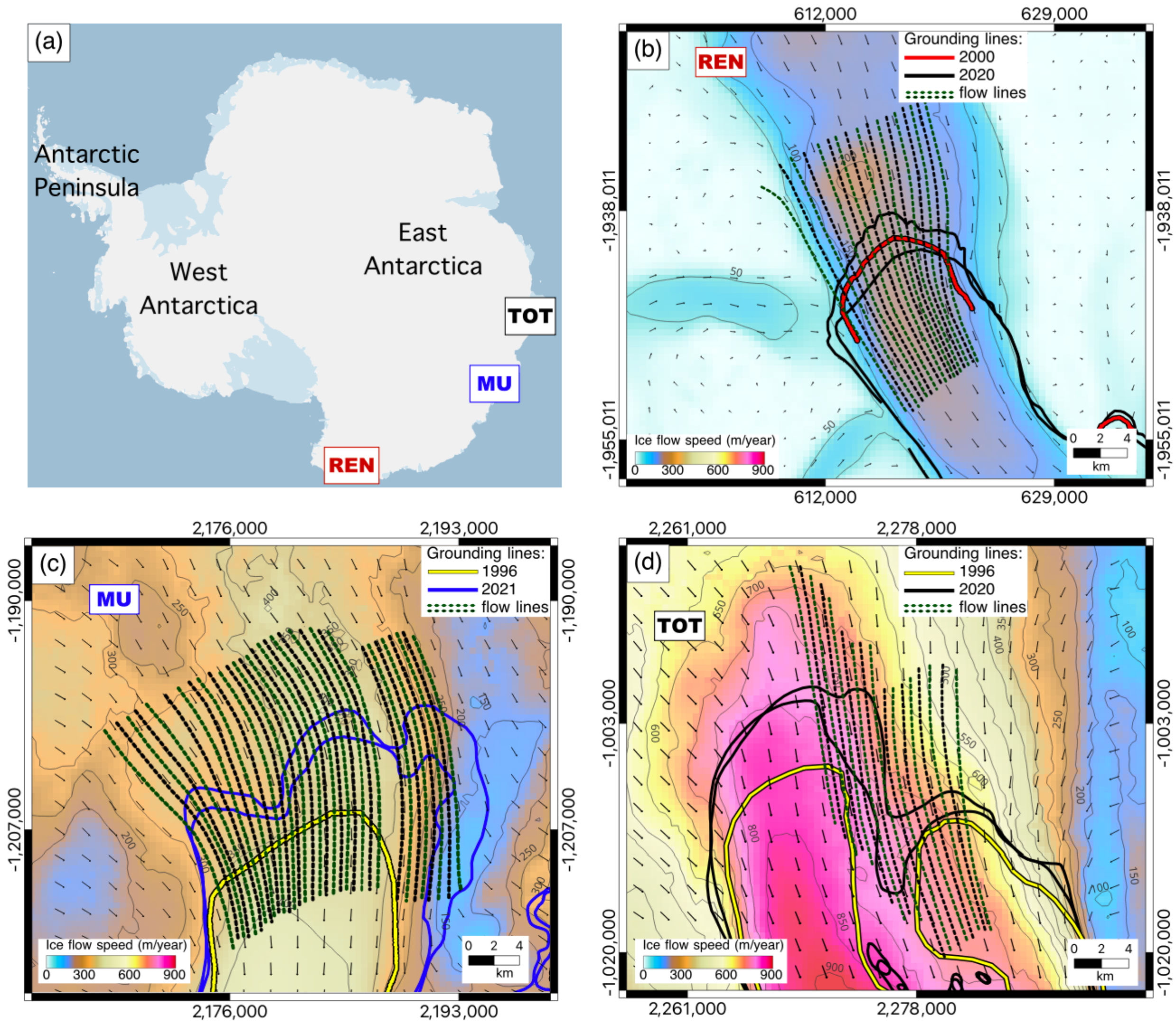
1) **The projection used in all three figures is EPSG:3031, which defaults to meters as the unit. We believe that changing the default units from meters to degrees would be confusing for those familiar with this projection. However, we attempted to convert the coordinates from meters to latitude/longitude and found that it degraded the figure quality (see the modified Figure 1 below). In latitude/longitude coordinates, the grid becomes tilted, making the figure more difficult to interpret.**



Modified Figure 2 (formerly Figure 1) with lat/lon coordinates.

2) **Panel (a) in Figure 2 (formerly Figure 1) displays the locations of the glaciers, as specified in the text (line 80), rather than the contours of the zoomed-in areas.**

3) **Due to the significant differences in glacier sizes, standardizing the figures to the same size and scale reduces their clarity and overall readability (see the modified Figure 1 below). For example, the Rennick Glacier is the smallest of the three, and resizing its subplot would introduce excessive irrelevant surrounding area while shrinking the main trunk. This also makes it significantly harder to label and number the profile lines, as they become too densely packed.**



Modified Figure 2 (formerly Figure 1) to the same size and scale of the subplots.

For the reasons listed above, we have opted not to modify the figures in the revised version of the manuscript.

Figure 5: I believe this figure is much clearer now. Thank you for your effort in addressing my concerns with its initial version.

Thank you, we appreciate your comment!

We thank the Reviewer for the valuable feedback and look forward for receiving further comments.

Response to Reviewer 2

Blue font: comments from Reviewer 2
Black bold font: Authors' responses to the reviewer's comments

Note: The line numbers referenced here correspond to the revised manuscript **with** highlighted corrections.

Review of "Importance of ice elasticity in simulating tide-induced grounding line variations along prograde bed slopes" by Ross et al.
This revision has greatly improved the manuscript's structure, helping readers understand the importance of your work in the context of tidal grounding line variations. There are still a few major structural points that we think would strengthen the manuscript. Below, we list these points along with minor corrections to consider as you revise.

We appreciate the Reviewer's thoughtful feedback and have made every effort to address the concerns raised. Below, we present a detailed explanation of the revisions made in response to each comment.

Major Comments

1. The current organization of §2 and §3 makes your workflow opaque. For example, we expect the DInSAR measurements from §2.2.3 to be used as model inputs rather than validation because the section is squeezed between a section describing model inputs and a section describing the model. We think that a flowchart or schematic describing what data is used as input into the model, what outputs the model produces, and how DInSAR measurements are used to validate the model will help readers parse the manuscript.

Agreed, section 2.2.3 has been renumbered as section 2.3. As requested, we have added schematics of the modeling process as Figure 1.

2. Your revision to Figure 5 looks great and helps us visualize what is important between the models as grounding zone width changes. You do a great job summarizing these points in your reply to both reviewers, but the description of how glacier thickness, velocity, and bed slope impact the grounding zone in §4.2 is confusing as written. We think it would be beneficial to include the main points in your reply to reviewers in addition to the specific examples given with numbers.

The main points from section 4.2 are:

1. *In both the viscous and viscoelastic models, the grounding zone width (GZ) exhibits a linear relationship with glacier thickness (H). The main difference between the two models lies in the magnitude of the modeled grounding zone width as a function of glacier thickness over varying bed slopes.* **(lines 427-428 and 432-433)**
2. *In both models, shallower bed slopes increase the sensitivity of grounding zone width to changes in glacier thickness. The viscous model is more sensitive to ice thickening compared to the viscoelastic mode.* **(lines 436-437)**
3. *The most pronounced effect of velocity changes on grounding zone width occurs at shallower slopes of 0.1% and 0.05%. For these slopes, an increase in ice velocity from 100 m/year to 800 m/year can result in up to a 60% increase in grounding zone width in both the viscous and viscoelastic models.* **(lines 447-450)**
4. *In both models, grounding zone width increases as the bedrock slope decreases, indicating that the relationship between glacier bed slope and grounding zone width follows an inverse power law.* **(lines 466-468)**
5. *For any combination of bedrock slope, glacier thickness, and ice inflow speed, the grounding zone width obtained from the viscoelastic model is nearly half that of the grounding zone width calculated by the viscous model on shorter time scales.* **(lines 473-475)**

Minor Comments

[Line number of manuscript v3 given in brackets if applicable]

General comments:

- Unit abbreviations are inconsistent; please choose m/yr or m/year

We made the abbreviations consistent: now, we use m/year everywhere

- Supplement and figures should be cited in the order of first appearance (e.g., Table S6 is first mentioned on line 162, before Tables S2-S5 and Figure 6 is first referenced on line 185, before Figures 3-5)

We arranged the supplementary tables and figures in the order in which they appear in the manuscript.

[13] What type of tides does each region have? Does having more time for viscous components of the model to equilibrate matter (e.g., if tides are primarily diurnal versus semidiurnal)?

In all our tests, we found that a two-month run with a stationary ocean and no tides, followed by a seven-day period with tides incorporated, allows the model to reach stability. Our results align with Stubblefield et al. (2021). We have tested a single semidiurnal sinusoidal tide and acknowledge that results may change if constructive or destructive interference occurs between diurnal and semidiurnal tides. Therefore, we reserve the pleasure of this analyzing different tidal scenarios for a follow up study.

[19-20] Word choice: what error is considered a match of the model to measurements? ~74% more accurate is not meaningful without context from §4.3.

Agreed we have modified this sentence with *'We establish the dependence of the grounding zone width on glacier thickness, bed slope, and glacier flow speed and find that grounding zone predictions using a viscoelastic model significantly outperform those of a purely viscous model.'*

[21] Underscore the critical role played by ice elasticity in continuum mechanics-based glacier models *on daily tidal time scales*.

Thank you for the advice! We added 'on daily time scales' into the sentence (line 22)

[37] This sentence does not explain the cool science you have done to make your short-term model include viscous components and better match data! It just seems like you are following the status quo of short term models.

Thank you, in the updated manuscript (now lines 37-39), we clarify that our study focuses on short-term models and highlight the 'status quo' – we disregard long-term glacier evolution. The physical aspects of our model are introduced later (lines 59–61) to maintain the logical flow of the text.

[56] full Navier-Stokes

We removed the word 'full'

[60-64] This line seems overly specific for the intro — these are methods and should be moved to the appropriate location.

Agreed, we removed the sentences the Reviewer is referring to

[71] "determine" is a strong word here that might imply the development of closed form parameterizations. A word like "assess" or similar is probably better here.

We replaced 'determine' with 'assess'

[76] Figure 1 shows locations, not relative locations

We removed the word 'relative' from the figure description

[82-84] This context on mass balance cites a paper that is over a decade old. There are much more up to date assessments of mass balance that should be provided.

We replaced (Rignot et al. 2013) with (Li et al. 2022) – (line 88)

[84-85] Just being grounded below sea level does *not* make a glacier potentially susceptible to collapse. It requires a reverse slope bed and TOT has ~ 40 km of prograde slope ("This means that the main trunk of TG will not be retreating on a retrograde bed and be prone to a marine instability in the coming years" from Li et al., 2015). Please revise accordingly

We replaced this sentence with 'Moreover, between 2010 and 2018, both glaciers experienced higher basal melt rates than the neighboring glaciers in East Antarctica due to the intrusion of warm ocean water into their subglacial cavities', which adds two recent publications into the reference list: Adusumilli et al., 2020, and Nitsche et al., 2017 (lines 88-91)

[89-92] Again, Pritchard et al. (2012) is 13 years old and multiple new, higher fidelity, longer-term estimates of basal melt have been published. It is important to look to the full body of literature when appropriate, but for specific values and assessments of stability, we should be using the most up-to-date perspectives.
Line 96: we replace Pritchard et al., 2012 with Adusumilli et al., 2020
Line 98: we replace Pritchard et al., 2012 with Baumhoeret al., 2021

[95] How do you choose the 69 profiles? They obviously do not matter much if your results are similar to when you used the nonphysical profiles in the first draft, so maybe mention that exact profile choice only saw ± x% error? You do not choose areas with negative or near-zero grounding zone widths (e.g., the gap between contours 25 and 26 on MU). Why? This looks like an interesting region as no GZ migration is observed. (This issue comes up again on line 102 where it says flow lines "were spaces 500 to 600 m apart" when there is a larger gap on MU)
Our InSAR-based grounding line has an accuracy of 200 m, resulting in a grounding zone accuracy of 400 m. We did not consider areas where the grounding zone width is less than 400 m, as this would result in an error exceeding the observed value. This explains the gap between profiles 25 and 26. Therefore, the 69 profiles were selected along the main trunks where the grounding zone width exceeds 400 m. We clarified that in the text: *'However, due to the 400 m grounding zone mapping error, we did not position the profiles in areas where the grounding zone width is less than 400 m, which resulted in a larger spacing between MU's profiles 25 and 26.'* (lines 111-113).

[98-99] Rignot et al. (2017) has been superceded by the Mouginot et al. (2019) phase-based velocity map, which is considerably higher fidelity. Is there a reason here to use a somewhat out-dated velocity product?
The reviewer's observation is valid, as the term 'phase-based' map may be misleading. Below, we explain why using Mouginot et al. (2019) would not change our results.
The 'phase-based' map from Mouginot et al. (2019) relies solely on InSAR phase data for the slow-moving interior of Antarctica. However, for fast-moving regions, such as the grounding zone we analyzed, it still uses the same pixel-offset-based dataset from Rignot et al. (2017). The Mouginot et al. (2019) map is more accurate only in slow-moving regions. The results remain unchanged in areas where only the pixel-offset technique has been applied in both the Mouginot and Rignot maps. The launch of NISAR or the recently deployed Sentinel-1A/C with a one-day revisit configuration will improve this situation, as it may allow phase-based measurements over grounding line regions of these glaciers.

[101] The expression "arctan(v_x/v_y)" can only provide direction across 180°, so we hope this was calculated not directly as the quotient, but rather as a two-argument function.
We used the $\mathbf{np.arctan2}(v_y, v_x)$ function for the direction calculation and have clarified the calculation process in the text (lines 108-110).

[101-102] The phrasing here is a little confusing. "Flow lines were selected along the flow direction" makes it sound like each flow line is selected along flow (which does not make sense since it would just select the same flow line over and over again). Did you mean they were selected across the direction of flow and generated for XX km along flow, centered on the grounding line (or something similar)?
Corrected, thank you (lines 110-111)

[104] Parallel --> perpendicular. Also, some of the lines on, e.g., MU are indeed parallel to the grounding line. Crossflow heterogeneity is mentioned as impacting your analysis in lines 103-104, but there is no further mention of what possible errors it may introduce or how you might address those errors in future work.
Thank you! We replaced 'Parallel' with 'perpendicular'
We extended the discussion on crossflow heterogeneity: 'Since the model does not rely on individual measurements but rather on the range of observations to ensure comprehensive coverage of the considered glaciers, crossflow heterogeneity does not impact the results of our analysis' (lines 360-362)

[109] Figures 1 and 2 still have confusing colorbars. For example, the Figure 1 colorbar has two yellows and two pinks that look nearly the same. We had a hard time determining that Totten is up to 400 m/y faster than Moscow University from a quick look at the figure (until we saw the contour lines).
We have modified the colorbar in Figure 2 (formerly Figure 1) and removed repeating colors. We did not modify Figure 3 (formerly Figure 2) as it does not have repeating colors. Moreover, we consulted with a

colorblind person, who confirmed readability of both figures. We acknowledge that color blindness exists in different forms and have made our best effort to accommodate the reviewer's request.

[122-123] "linearly approximating extracted bed elevation values": does this mean you fit a linear polynomial to the full profile and positive indicates prograde (i.e., higher upflow)? This is a little counterintuitive as we would reflexively think in an upflow-to-downflow frame, so a prograde slope should be a negative number), which just highlights that the reference frame should be defined and how this bedrock slope estimate is calculated should have a bit more clarity. Moreover, there seems to be a bit of a mismatch between the Figure 2 and Table S1 in that there appear to be some retrograde bed slopes (i.e., negative), particularly along the right side of MU, yet Table S1 indicates there are no negative bed slopes. This also plays back into the comment on lines 84-85, where the majority of these grounding line bed slopes are prograde, not retrograde, indicating there is *not* a substantial concern (at least in the chosen locations) of instability.

We thoroughly rechecked the slope values for profiles 30–33 over MU (we believe the reviewer is referring to these). Considering the uncertainties associated with BedMachine data and the use of a linear approximation, these profiles exhibit very shallow but positive slopes (please refer to Figure 7, where the reviewer can see four very shallow slopes of 0.02% with grounding zones extending over 5.5 km). Moreover, even if we exclude these four profiles from consideration, it will not change the conclusions we make.

We understand that reviewers may find the classification of these positive slopes as prograde counterintuitive. However, assigning positive and negative slope values to prograde and retrograde terrains, respectively (or higher/lower bed elevations upflow), aligns with the Latin origins of the terms 'pro' and 'retro,' meaning 'in front of' and 'behind.' Positive numbers are greater than negative numbers, meaning they come 'in front of,' while negative numbers stay 'behind.' In other words, our convention follows a downflow-to-upflow framework, which is widely adopted in the cryosphere community:

1. **Rignot, E., 2022. Sea level rise from melting glaciers and ice sheets caused by climate warming above pre-industrial levels. Phys.–Uspekhi, 65(1).**
2. **Rignot, E., Ciracì, E., Scheuchl, B., Tolpekin, V., Wollersheim, M. and Dow, C., 2024. Widespread seawater intrusions beneath the grounded ice of Thwaites Glacier, West Antarctica. *Proceedings of the National Academy of Sciences*, *121*(22), p.e2404766121.**
3. **Li, X., Rignot, E., Morlighem, M., Mouginot, J. and Scheuchl, B., 2015. Grounding line retreat of Totten glacier, East Antarctica, 1996 to 2013. *Geophysical Research Letters*, *42*(19), pp.8049-8056.**
4. **Sergienko, O.V. and Wingham, D.J., 2022. Bed topography and marine ice-sheet stability. *Journal of Glaciology*, *68*(267), pp.124-138.**
5. **Schoof, C., 2012. Marine ice sheet stability. *Journal of Fluid Mechanics*, *698*, pp.62-72.**
6. **Milillo, P., Rignot, E., Rizzoli, P., Scheuchl, B., Mouginot, J., Bueso-Bello, J.L., Prats-Iraola, P. and Dini, L.J.N.G., 2022. Rapid glacier retreat rates observed in West Antarctica. Nature Geoscience, 15(1), pp.48-53.**
7. **Batchelor, C.L., Christie, F.D., Ottesen, D., Montelli, A., Evans, J., Dowdeswell, E.K., Bjarnadóttir, L.R. and Dowdeswell, J.A., 2023. Rapid, buoyancy-driven ice-sheet retreat of hundreds of metres per day. *Nature*, *617*(7959), pp.105-110.**
8. **Chen, H., Rignot, E., Scheuchl, B. and Ehrenfeucht, S., 2023. Grounding zone of Amery ice shelf, Antarctica, from differential synthetic-aperture radar interferometry. *Geophysical Research Letters*, *50*(6), p.e2022GL102430.**

[126] Remove "relief"
Removed

[145-146] Horizontal flow velocity is also not uniform even on tidal timescales – what error is introduced by assuming the double difference interferogram cancels out the horizontal deformation (e.g., Rack et al., 2017; Wild et al., 2019)?
In our double-difference interferograms, if there were uncompensated horizontal deformation, it would appear as a distinct fringe pattern in our data. We do not observe any characteristic pattern associated with residual horizontal deformation, indicating that the potential error introduced is negligible. No changes are needed in response to this comment.

[158-161] Why is the IBE correction applied here? Doesn't IBE contribute to grounding line motion as well?
The IBE is used to calculate the effective tidal height in our DInSAR interferograms to ensure that when comparing DInSAR grounding zone measurements with tidal model outputs, we select the corresponding tidal height in the models. This is specified in the manuscript at lines 189-192: *'To ensure a valid comparison between the DInSAR-derived and modeled grounding zones, the IBE-corrected tidal levels listed in column H of Table S6 are used to evaluate the grounding zone width in the model. The modeled grounding zone widths are measured by calculating the difference between the grounding line position at maximum H and minimum H.'* **The IBE is not included in our model, and we plan to analyze this aspect in future studies.**

[166] Why can you assume that the grounding line position observed in the interferogram corresponds to the largest tidal level among the acquisitions? Does this mean you cannot image low tide very well and are missing important information about GL migration near low tide?
High tides facilitate water infiltration upstream (Rignot et al., 2024); therefore, the highest tide in our double-difference InSAR acquisitions represents the mapped grounding line. The reviewer is correct that, under this assumption and with this approach, we will never be able to capture the grounding line at the lowest tidal level. We have added this clarification on lines 180-181: *'Following this assumption our approach will never be able to map the grounding line at its lowest tidal position.'*
Rignot, E., Ciracì, E., Scheuchl, B., Tolpekin, V., Wollersheim, M., & Dow, C. (2024). Widespread seawater intrusions beneath the grounded ice of Thwaites Glacier, West Antarctica. Proceedings of the National Academy of Sciences, 121(22), e2404766121.

[170-172] Thank you for including the table of height changes from DInSAR measurements for each glacier (though we note these are results, not methods, so should probably be moved).
We noticed there is only one set of measurements (high and low tide) for each glacier (as a result of tasking constraints we presume), which do not always span a representative range of tides. Because this is tasked and there does not seem to be more available data, some discussion of the limitations of the analysis given the limited dataset is critical later in the manuscript.
Agreed, we have added the following clarification on lines 523-527: '*As shown in equation (44), a one-meter amplitude semidiurnal tide (12-hour period) was used to simulate the tidal behavior of the studied glaciers. It should be noted that our DInSAR grounding line measurements rely on a single set of tidal observations (high and low tide) for each glacier. While these measurements do not always cover the full tidal range, they consistently capture a differential tidal variation of approximately one meter (Table S2).***'**

[202] You do an excellent job explaining model variables as they appear, so we think having Table 1 in the main text now detracts from the read-through. We would put it in an appendix or the supplement for easy reference.
We moved Table 1 to the Supplement.

[205] Do you want to use (x,z) to be consistent with Stubblefield et al., (2021)?
We find the (x,y) coordinate system more appropriate in this context, as the (x,z) system may give some readers the impression of a three-dimensional representation. Therefore, while we appreciate the Reviewer's suggestion, we have chosen to retain the (x,y) system without modifications.

[211] Line 211 and equations 3,4,5 should replace "f(x)" with "b(x)" to be consistent with Figure 4 and Equation 41 (or use β(x) as in Stubblefield et al., 2021).
We replaced "f(x)" with "b(x)"

[234] We might use η(D) rather than η(ν) in equations 8 and 10 (local η should only depend on invariants of D). This is also consistent with using η(τ) in equation 13.
We replaced η(ν) with η(D) in equations 8 and 10.

[250] Shouldn't this definition come after eq. 12 and not eq. 13?
We moved the definition to come after eq.12.

[279.5] Why do you jump around from indicial to tensor notation in equation 30?
Depending on the mathematical operations performed, indicial notation may be more advantageous than tensor notation, and vice versa. We believe that our derivation approach is both efficient and accessible to readers with the relevant background. Therefore, we respectfully choose not to modify the derivation process.

[319] Does adding additional tidal constituents change the results?
We have not analyzed additional tidal constituents or IBE effects as we believe this could be an excellent starting point for future studies.

[322] Thank you for clarifying that the data-model comparison is not one-to-one. We interpret the comparison as selecting points from the model at the same tidal level as the interferogram, but the wording is confusing. We suggest revising this section with an emphasis on the precise comparison made. Also, if you are modeling 1 m tides, then are subselecting, is this a fair comparison if the tidal range is different from 2 m here? In other words, the model is missing the maximum stresses if the modeled tide range is different from the actual tide range at these glaciers (which is important for the viscous component).
We understand the reviewer's confusion; however, we are not missing the maximum stresses in the model, as the tidal amplitude remains constant for each run. We simply sample the model grounding line position at the DInSAR-measured tidal heights. We have revised our wording for clarity and now state on lines 339-344: '*To ensure a meaningful comparison between the measurements and the model results, the modeled grounding zones were calculated using a 1-meter tidal amplitude while considering the model grounding line position at the DInSAR tidal heights (Column H in Table S2). In other words, the high- and low-tide sea levels derived from the interferograms were used to extract the modeled grounding line positions and subsequently determine the corresponding grounding zone.***'**

[376] "%" --> "m/yr"
Replaced '%' with 'm/year'

[382-383] The statement, "MU introduces some variability, particularly due to its wide grounding zones exceeding 6km", is confusing. On Figure S1, there does not appear to be any MU grounding zones that exceed 6 km in width at all. In fact only Totten has grounding zone widths that exceed 6 km.
Thank you for the observation! We replaced 'MU introduces some variability, particularly due to its wide grounding zones exceeding 6km' with 'MU introduces some variability, particularly due to presence of narrow grounding zones under 1 km.' (lines 401-402)

[393-394] Why is this assumption being made? The data collection time from 1996 should be available and therefore the tide stand of that grounding line can be explicitly estimated
Indeed, the acquisition time of the 1996 data should be available but is not accessible to the authors. Furthermore, the cross-comparison of C-band and X-band grounding lines, along with their respective accuracies, is limited and beyond the scope of this study. Therefore, we do not find it appropriate to extend our analysis to include the 1996 tidal levels. We clarified this aspect in the text (line 412)

[438] Y-axis does not show evolution, but rather the size of GZ for different chosen ice speeds.
Changed 'while the y-axis shows the evolution of the grounding zone as the glacier becomes thicker' to 'while the y-axis shows the grounding zone width.' (lines 458-459)

[442] What does "where the results for different inflow speeds are averaged by glacier thickness" mean?
We clarified the sentence by replacing 'where the results for different inflow speeds are averaged by glacier thickness' with 'where the grounding zone values, indicated for different glacier thicknesses, are obtained by averaging the grounding zones for the same thickness across varying inflow speeds' (lines 462-463)

[501] "confirms" is not a great word choice here. It is certainly consistent with your observations, but other processes can account for this apparent sensitivity.
Replaced 'confirm' with 'support' (line 523)

[502] Figure 7: What justification do you have for the glacier thickness/flow speed/GZ length dependence? We could draw a line with a positive or negative slope within the error bars presented in Figure 5c.

We acknowledge that we do not have extensive data to unambiguously fit a linear relationship. However, our justifications are based on literature studies, as discussed in Section 5.1 of our manuscript. Our findings can be summarized by the following key points:

1. **GZ Wider where glaciers are thicker (lines 517-520):**
   This observation can be associated with the increase of the flexural wavelength of ice when its thickness increases (Freer et al., 2023). For thicker ice, the same tidal amplitude affects a larger horizontal distance, leading to a broader grounding zone. This effect is more pronounced on shallow slopes, where the tidal amplitude influences a larger area.
2. **GZ Width for Shallow slopes and glacier velocity (lines 520-523):**
   Glacier velocity significantly impacts grounding zone width for bed slopes below 0.1% due to the increase in elastic stresses with faster glacier flow (Christmann et al., 2021). As a result, the elastic stress of fast-flowing glaciers on shallow slopes is higher than that of slower-moving glaciers, making the former more sensitive to thickness changes and supporting our observations.

[530-532] This line is off the mark. Plenty of people have used pure elastic models successfully to model flexure over tidal timescales and compare to observations. Yes, you will not fully capture all glaciological processes, but you can certainly investigate individual processes with elastic models.

Agreed, we removed the sentence the Reviewer is referring to.

Table S1: Including standard deviation would be helpful for a reader to understand variability across the glacier.

Table S1 does include the standard deviations (please see the screenshot from the Supplement previously submitted for the first round of review).

Table S1. Minimum, maximum, and average values of the grounding zone width, ice thicknesses, bed slopes, and ice flow speed of TOT, MU, and REN glaciers calculated along the profiles of the corresponding glaciers.

Glacier characteristics		TOT	MU	REN	Data source
Grounding zone, km	Min	1.2 ±0.4	0.6 ±0.4	1.3±0.4	Pairs of DInSAR interferograms
	Mean	4.1±0.4	2.1±0.4	2.3±0.4	
	Max	14.9±0.4	5.1±0.4	3.4±0.4	
Ice thickness, km	Min	1.9±0.2	2.0±0.2	0.9±0.3	BedMachine2 (Morlighem et al., 2017)
	Mean	2.2±0.1	2.2±0.1	1.1±0.2	
	Max	2.4±0.2	2.4±0.1	1.2±0.1	
Bed slope, %	Min	0.01±0.01	0.2±0.2	0.3±0.2	BedMachine2 (Morlighem et al., 2017)
	Mean	1.2±0.1	2.2±0.2	1.4±0.2	
	Max	4.0±0.2	5.9±0.3	3.5±0.3	
Ice flow speed, m / year	Min	492 ±113	171±41	117±51	InSAR-based ice velocity data provided by MEaSUREs program (Rignot et al., 2017).
	Mean	647±65	335 ±20	172±24	
	Max	754±49	381±18	192±12	

Figure S2: Why is only the lower boundary the smaller mesh size? Did you try a run with all small mesh sizes to see if there are different results? The monotonic decrease in GZ width with decrease in mesh size in Figure S1 is interesting. How do you know it evens out in the viscoelastic model? Wouldn't you expect scatter in GZ width once it is not mesh size dependent?

Yes, we did run the model with small mesh sizes on the upper boundary and we did not find beneficial for two reasons:

1. **Reducing the upper boundary mesh size does not improve model performance, which is expected since our primary focus is on the lower boundary. Adding more nodes to the upper boundary only increases the number of computed values in the upper part of the glacier, which are not relevant to our analysis of grounding lines.**
2. **Increasing the number of nodes on the upper boundary significantly increases computational time. This excessive use of computational resources is not justified, as it does not enhance accuracy.**

We now mention our findings regarding the decreased upper-boundary mesh size (Supplement, lines 38-39)

The monotonic decrease in grounding zone (GZ) width with decreasing mesh size, as shown in Figure S1, is an expected outcome—finer mesh resolution improves precision. This trend of decreasing an error when decreasing the mesh size has been well-documented in multiple finite element method (FEM) studies. Below, I list some FEM studies as an example:

1. **Nakshatrala, K.B., Turner, D.Z., Hjelmstad, K.D. and Masud, A., 2006. A stabilized mixed finite element method for Darcy flow based on a multiscale decomposition of the solution. Computer Methods in Applied Mechanics and Engineering, 195(33-36), pp.4036-4049.**
2. **Joodat, S.H.S., Nakshatrala, K.B. and Ballarini, R., 2018. Modeling flow in porous media with double porosity/permeability: A stabilized mixed formulation, error analysis, and numerical solutions. *Computer Methods in Applied Mechanics and Engineering*, *337*, pp.632-676.**
3. **More, S.T. and Bindu, R.S., 2015. Effect of mesh size on finite element analysis of plate structure. *International Journal of Engineering Science and Innovative Technology*, *4*(3), pp.181-185.**
4. **Dutt, A., 2015. Effect of mesh size on finite element analysis of beam. *International Journal of Mechanical Engineering*, *2*(12), pp.8-10.**
5. **Strouboulis, T., Copps, K. and Babuška, I., 2001. The generalized finite element method. *Computer methods in applied mechanics and engineering*, *190*(32-33), pp.4081-4193.**
6. **Szabó, B.A., 1986. Mesh design for the p-version of the finite element method. *Computer Methods in Applied Mechanics and Engineering*, *55*(1-2), pp.181-197.**
7. **Strouboulis, T., Zhang, L. and Babuška, I., 2003. Generalized finite element method using mesh-based handbooks: application to problems in domains with many voids. *Computer Methods in Applied Mechanics and Engineering*, *192*(28-30), pp.3109-3161.**
8. **Strouboulis, T., Babuška, I. and Copps, K., 2000. The design and analysis of the generalized finite element method. *Computer methods in applied mechanics and engineering*, *181*(1-3), pp.43-69.**

Table S2: The r squared values of 1.0 are suspicious. How many data points are in the regression?
For each bed slope and inflow speed, we approximate four modeled grounding zones, corresponding to the investigated glacier thicknesses of 1 km, 1.5 km, 2 km, and 2.5 km. Since we are approximating modeling results rather than observations, an R^2 value of 1 is not unrealistic; it simply indicates that, for the four considered thicknesses, the modeled relationship between grounding zone position and glacier thickness (for a given bed slope and inflow speed) is linear.

Figure S4: Clarify if these are bar plots or histograms. If they are histograms, the bar width should encompass the full range of values in each bin. (e.g., In panel b, if the first bar is all profiles with a glacier thickness between 2.0 and 2.1 km, then it should stretch all the way to 2.1 km).
We removed Figure S4, as we believe it is unnecessary for comprehension, given that it duplicates the information presented in Table S1.

We hope the revisions to the manuscript address the Reviewer's concerns and look forward to any further feedback.