Review of Antropova et al., submitted to The Cryosphere Discussion

Intra-annual grounding line migration and retreat: insights from high-resolution satellite and in-situ observations over Milne Glacier in the Canadian High Arctic

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Summary:
The revised manuscript by Antropova et al. presents an analysis of an extensive SAR dataset over Milne Glacier, documenting the emergence of a new pinning point in the glacier's eastern sector associated with grounding-line retreat behind a rise in the subglacial topography, as identified from airborne and terrestrial ground-penetrating radar observations. Compared with the original submission, the authors have substantially strengthened their interpretation by extending the discussion beyond uncertainties associated with the assumption of steady glacier flow in DInSAR processing to also consider the influence of tidal-flexure-induced geometric ("bending") effects on grounding-line detection. The manuscript provides a valuable case study demonstrating that careful interpretation of DInSAR-derived grounding lines is required, particularly where grounding-line migration is accompanied by the formation of new pinning points that modify the spatial pattern of retreat.

Strengths:
The authors demonstrate a strong command of the DInSAR methodology and have clearly addressed the key concerns raised during the first round of review. In particular:

1. The quantification of the tidal-flexure-induced geometric ("bending") effect shows that apparent grounding-line migration can be spurious by approximately one local ice thickness (around 200 m), resulting in errors of a magnitude comparable to those arising from the common assumption of steady glacier flow. The authors further show that this effect is most pronounced for scenes acquired at high incidence angles and use this insight to identify and exclude unreliable DInSAR scenes from their grounding-line analysis.
2. The tidal reconstruction has been extended to include the full set of tidal harmonics derived from the field observations. The comparison with a regional tide model demonstrates that incorporating the in situ data leads to only minor differences in the reconstructed tidal signal, highlighting the robustness of the approach and suggesting that the methodology is transferable to locations where field observations are unavailable.
3. These methodological improvements enable the authors to apply a more rigorous selection of DInSAR scenes and present a convincing reconstruction of the emergence of a new pinning point. The manuscript provides an interesting interpretation of how this feature relates to ocean-water intrusion and the evolving pattern of grounding-line retreat, placing the local observations into a broader glaciological context.

Weaknesses:
While the importance of the tidal-flexure-induced bending effect is now convincingly demonstrated, the correction is only applied along the three radar transects rather than spatially across all DInSAR scenes. I assume this reflects the limited availability of ice-thickness measurements outside the radar profiles. If so, it would be helpful for the authors to state this explicitly.

If suitable auxiliary ice-thickness datasets are available, I encourage the authors to assess their accuracy against the radar-derived thicknesses along the existing transects and, if appropriate, use them to extend the bending correction to two dimensions. If this is not feasible, I would appreciate a brief discussion of how the conclusions drawn from the three profiles can be generalized to the spatial grounding-line patterns presented in the revised manuscript. Although the authors chose not to pursue the alternative "alpha-map" approach, a short discussion of this choice and its implications would also strengthen the manuscript.

In addition, I would find it useful to include the flexure curves after application of the bending correction for both the steady-flow and unsteady-flow cases. This would help the reader better assess the magnitude of the correction and its effect on the inferred grounding-line position.

Finally, while the harmonic analysis used to reconstruct sea-surface height represents a valuable addition to the manuscript, I have some questions regarding the use of all 59 tidal constituents. It is not immediately clear that all constituents are statistically significant or contribute meaningfully to the reconstruction. I encourage the authors to provide some justification for this choice, for example by discussing the statistical significance or signal-to-noise ratio of the retained constituents, and an L-curve analysis to avoid overfitting. It would also be helpful to quantify the resulting uncertainty in the reconstructed tidal signal (e.g., amplitude and phase errors) and discuss how this uncertainty propagates into the grounding-line analysis and whether it affects the main conclusions.

Minor comments (in chronological order)

Introduction**:**

l14: Reword “Observing and quantifying” to “Monitoring changes of the boundary…”

l17: Reword “ice tongue” to “glacier”

l21: Atmospheric pressure “variability”. Here and throughout, it’s the pressure anomalies that matter for flexure.

L27: Reword “water intrusion behind a” to “the emergence of a new”, which is more appropriate in light of the storyline

l28/29: The current wording of this general statement may be somewhat too broad, as the suitability of a given DInSAR result depends on the specific research question being addressed. The statement is valid in the context of delineating grounding lines, where minimizing positional uncertainty is essential, but higher-error observations may still contain valuable information for other applications. For example, DInSAR signals affected by tidal-flexure-related uncertainties could potentially provide constraints on processes such as neutral-layer depth within a floating ice beam. I therefore suggest rephrasing this sentence to clarify that careful selection of DInSAR scenes should be guided by the specific scientific objective, rather than implying that high-error results are generally unsuitable.

l55: add “of relatively fast moving glaciers”

l59: Terminology here and throughout. Thank you for adjusting the terminology, but it is really called “the hydrostatic line (HL)” in the literature, and many recent presentations. Reword to “(i.e. hydrostatic line), where hydrostatic equilibrium is reached…”

l60: Reword “ice tongue” to “glacier”. In this general part of the introduction, we really talk about glaciers, ice tongues also bend with tides, but it’s really the minority of studies looking at ice tongue flexure.

L64: Reword “to the line of hydrostatic equilibrium” to “from the hinge line to the hydrostatic line” for simplicity.

L73: Reword “by the hydrostatic equilibrium equation” to “predicted by assuming hydrostatic equilibrium” as there is no such thing as THE hydrostatic equilibrium equation (some assume mean ice column density, others assume a two layer model of ice and firn), but there is the assumption thereof.

L83: Add “differential” displacement of sea surface height.

L89: Add “in-situ measured” velocity changes

l96: Remove “help elucidate ice deterioration processes”, which reads like over-interpretation of the presented results. Ice deterioration is normally the structural weakening of ice through accumulation of damage (for example in lateral shear zones). Which are speculated but not quantified in the current form of the manuscript.

Study site and methods

l114: Remove “The floating ice tongue experiences moderate stresses due to tidal bending in the flexure zone”, as it is implied by the following sentence where the tidal range and tidal regime are introduced.

L117: Add atmospheric pressure “variability” here and throughout. Also, reword “tides in the fiord” to “the tidal forcing” as the IBE is commonly included in the tidal forcing that drives numerical simulations.

L118: add “vertical” displacement, and “flexural” stresses it experiences.

L124: Add highest grounding-line retreat rate “of ? km/yr” along the glaciers centerline.

L126: Remove “upstream” from glacier acceleration as it implies that buttressing will be quantified later in the study.

L177: Reword to “stiffness in lateral shear zones” as glaciers have one on each side.

L231: Reword to “DInSAR combinations”

L234: This statement represents the central hypothesis of the manuscript and may be easily overlooked at its current position. I suggest moving it to the end of the Introduction, where the research objectives and hypotheses are clearly outlined. It may also be useful to integrate this statement with the paragraph beginning around L95, which already introduces the motivation and

scientific questions addressed in the study. "We test the hypothesis that a newly emerging notch/pinning point along the eastern margin is expressed in DInSAR observations associated with SAR acquisitions during periods of high sea-surface height (SSH). Testing this hypothesis requires reconstructing the tidal height at the time of each SAR acquisition from the available DInSAR observations..."

L240 and 247: I appreciate the inclusion of the harmonic analysis to reconstruct sea-surface height (SSH), as this is an important component of the methodology. However, I would appreciate further clarification regarding the selection of the 59 tidal constituents included in the analysis. Given the length and characteristics of the available in-situ record, it is not immediately clear whether all constituents are statistically significant or sufficiently constrained by the observations.

Could the authors specify whether a signal-to-noise ratio threshold or another statistical criterion was applied to determine which constituents were retained? Based on my experience, relatively short in-situ records generally provide the strongest constraints on the dominant diurnal and semi-diurnal constituents, while higher-frequency or weaker constituents may be less robustly resolved.

Furthermore, it would strengthen the manuscript to quantify the uncertainty of the reconstructed SSH signal. In particular, the amplitude and phase uncertainties of the significant constituents should ideally be propagated to provide an estimate of the total reconstruction uncertainty. It would also be valuable to indicate what fraction of the observed SSH variability can be reproduced with confidence by the harmonic model. This would allow the reader to better assess how uncertainties in the tidal reconstruction propagate into the grounding-line analysis and the main conclusions of the study.

L297: Add “mean” glacier flow heading

L298: Add “mean” glacier surface tilt

L303: Add “other” glacier flexure zones

L304 to 308: I suggest revisiting this section, as the current description appears more complex than necessary and may obscure the underlying physical framework. In an elastic approximation, the differential sea-surface height (DSSH) can be used directly, as it is mathematically equivalent to double-differencing the individual SSH values from the SAR acquisitions. This is also why the bending-effect correction can be derived directly from the DInSAR observations without requiring a tidal reconstruction.

The reconstruction of the individual SSH values becomes necessary when moving beyond the elastic approximation and considering a viscoelastic framework, where the flexural response is time-dependent and depends on the history of loading. Since real glacier systems exhibit viscoelastic behavior, the distinction between these two approaches is important. I suggest clarifying this distinction in the manuscript and explaining more explicitly why the chosen approach requires SSH reconstruction in the context of the study.

Results:

L347: Reword to “3-day and 4-day RCM SAR scenes and remove “acquired four days apart”. This is a classic AI error to my experience.

L355: Please consider adding the approximate ice thickness at the grounding line for each transect to this table. When first reading the table, I interpreted the difference in flexure-zone widths as potentially reflecting differences in ice thickness between the transects. However, Figure 9 suggests that the grounding-line ice thicknesses are actually quite similar. This would therefore provide an additional argument that the observed differences in flexure-zone width are primarily controlled by the presence of the pinning point rather than by variations in ice thickness.

L360: Remove”pointing up-glacier”

L382: Add displacement “on freely-floating areas”

L394: This is only true if the ice thickness is not much thinner in the eastern transect than in the central and western transect. Please add that the ice thicknesses are similar, which then underscores the interpretation that the eastern section was grounded.

L404 to 408: Can you add a Supplementary Figure that shows only the flexure curves where viscoelasticity is hypothesized. I would also suggest revisiting the diagnostic used to identify viscoelasticity. The viscoelastic response is not directly related to the absolute sea-surface height (SSH), but rather to the time-dependent loading and therefore to changes in SSH over time. The time derivative of SSH may therefore provide a more appropriate proxy for assessing whether the observed flexure response is consistent with viscoelastic behavior (see, for example, Figure 8 in Wild et al., 2017, JoG).

L420: The mention of the "hypothesis" here caught my attention, as it had not yet been clearly introduced at this point in the manuscript. After revisiting the text, I realized that this refers to the hypothesis stated around L234. This reinforces my earlier suggestion to move the hypothesis statement to the end of the Introduction, where the objectives and hypotheses of the study are presented explicitly. Doing so would make the logic of the manuscript easier to follow and avoid requiring the reader to recall a hypothesis introduced earlier in a different context.

L435/437/438: Again 59 constituents are likely not all statistically significant, and their amplitude and phase errors would add uncertainty to the reported correlation values and RMSE (see Fig. 12 in Wild et al., 2019, TC how this is done for the K1 constituent).

L446: Typo Fig “A4”

L453: Please add Rack et al. 2017, TC, and Wild et al., 2018, Frontiers to these citations. Both deal specifically with the bending effect that is described here.

L455to 470: The sensitivities of orbit geometry and incidence angle to velocity variability and surface bending represent an important outcome of this analysis. Therefore, I would encourage the authors to provide additional guidance in the Discussion section on how these findings can inform DInSAR applications beyond the specific objective of delineating grounding-line retreat around an emerging pinning point.

The current manuscript focuses on identifying reliable DInSAR scenes for reconstructing grounding-line migration, which is a valuable application. However, the demonstrated sensitivities are also relevant for other glaciological research questions, where observations affected by these signals may contain useful information rather than simply represent sources of uncertainty. For example, such signals could potentially contribute to investigations of shear-zone weakening, neutral-layer depth, or other processes involving ice deformation and flexural response.

I therefore suggest adding a dedicated paragraph or section in the Discussion that places these methodological insights in a broader context. This would also strengthen the connection to my earlier comment regarding the Abstract and would help ensure that the conclusions communicate the wider applicability of the results beyond the specific case study presented here.

L485 and L489: This difference is a huge signal that one might want to take advantage of...

Discussion:

L565: Add atmospheric pressure “variability”

L568: over 1m tidal range is only relatively large when compared to Milne Glacier, but is tiny when compared to other regions in Antarctica (Rutford has about 7 m range) and Greenland (several meters). Please reword.

L573: Add the same water level changes “and similar ice thicknesses”.

L572 to 583: Great discussion about the link between pinning points and lateral shear zones.

L584 and concluding sentence L599: I suggest adding a few sentences discussing how changes in grounding-line sinuosity associated with emerging pinning points may influence local buttressing. The manuscript currently emphasizes ocean-water intrusion as a mechanism affecting grounding-line evolution, which is important, but the development of additional pinning points and increased grounding-line complexity may also modify the stress regime.

In particular, increased grounding-line sinuosity can enhance local buttressing by providing additional lateral and basal resistance, potentially reducing ice discharge across the grounding line and slowing further inland retreat. Including this perspective would provide a more complete discussion of the competing effects associated with pinning-point formation: while ocean intrusion may promote retreat, the associated increase in grounding-line pinning and buttressing may locally stabilize the glacier.

L588: Add shallowest “sloping” western transect

L603: Reword “SAR data acquisitions” to “InSAR pairs”

L624: This would be the perfect place to add that the identified sensitivity to high incidence angles can in turn be exploited to constrain the depth of the neutral layer.

L666: I suggest adding the qualifier “local” when referring to a marine ice-sheet instability. Based on the bed geometry shown in Fig. 9b, it appears that the retrograde bed slope extends only about 2 km upstream before transitioning to a prograde slope, with the glacier becoming grounded above sea level further upstream of point A.

Given this limited spatial extent of the retrograde bed configuration, I would appreciate some clarification regarding the broader implications of invoking marine ice-sheet instability in this context. Classical marine ice-sheet instability is generally associated with sustained grounding-line retreat over much larger inland reaches of retrograde bed geometry (often extending hundreds to thousands of kilometers).

L671: Consider removing “dramatic”. Grounding-line retreat calculated over 31 years is seldom dramatic.

Conclusion

L687: please define LOShV and LOShB again in the conclusion. Some readers might not read the entire paper and would miss this key information.

L688: Please add the least reliable information here too., as this might motivate other research questions than what is presented here.

L689: Add atmospheric pressure “variability”

L694 to 696: I suggest revisiting the final sentence, as the current wording may not fully capture the broader significance of the results. In my view, an important implication of this study is not only that new pinning points can emerge during grounding-line retreat, but that their occurrence and impact depend strongly on the subglacial topography inland of the present grounding line. Numerical modelling studies have already demonstrated the potential for new small ice shelves to form during grounding-line retreat and for grounding-line sinuosity to increase around bedrock highs (e.g., Morlighem et al.; Gudmundsson et al.). Therefore, a key challenge for predicting future grounding-line evolution is obtaining sufficiently detailed knowledge of bedrock geometry in regions that may become marine in the future. I encourage the authors to consider emphasizing this perspective in the concluding statement, as it highlights the broader importance of high-resolution bed mapping for anticipating future grounding-line behavior.

Figures:

Figure 1: (a) the light blue line for “IceBridge and IPR data” is hard to see on the blue background. Can you use another colormap and add a colorbar for ice thickness. (b) Consider adding labels for “Eastern”, “Central” and “Western” flowline on their downstream end.

Figure 2: (c) Instead of the absolute values, please show pressure anomalies as these are used for the IBE. Consider adding a second yaxis on the right that shows the corresponding IBE values in meters. It looks like GEM has a negative bias when compared to the AWS.

Figure 3: Change “DDInSAR” to “DInSAR” in the legend, as requested in the first round of review.

Figure 4: good

Figure 5: (a) add arrows and labels to “bumps due to velocity variability”.

Figure 6: It would be good to also see examples of flexure curves for the unsteady period, maybe as a second row of panels. These would strengthen the reasoning to take care when selecting DinSAR scenes for flexure-zone delineations. (I’d personally like to see uncorrected and corrected flexure curves with and without the bending effect, but that might go beyond the scope of the revision).

Figure 7: good. I’m glad the panels include the LOShB error.

Figure 8: By now I have forgotten what the blue and red shadings mean. Looking back to Figure 2 they correspond to steady and unsteady periods. Can these be added to the other Figures showing time series data for compareability (Figs. 2c, 6a, A1)?

Figure 9: good

Figure 10: good. It’s a untypical to have time progress upwards the y axes in a Hofmöller diagram. Please flip the y direction, this way also the migrated GNSS point along the central transect doesnt’t appear to move upstream.

Appendix A

Figure A1: That’s quite a spectacular success of the Gr1kmTM model

Figure A2: legends show +pi/-pi elsewhere, here it’s +3.14 and -3.14. I do like these labels much more than the ones presented in Figure 3d. Consider changing them to better align with the main text.

Figure A3: good

Figure A4: good

Appendix B

L750 to 752: The choice of the regularization parameter is not sufficiently clear to me, and the current description gives the impression that this value may not be strongly constrained. Given that the inversion includes 59 tidal constituents, I am concerned that the combination of a large number of degrees of freedom and the selected regularization strength could potentially lead to overfitting of the observed time series.

I therefore encourage the authors to provide a more thorough justification for the regularization parameter choice. In particular, an L-curve analysis would help demonstrate that the chosen parameter provides an appropriate balance between fitting the observations and avoiding overfitting. This would increase confidence that the reconstructed tidal signal is robust and that the inferred grounding-line behavior is not sensitive to inversion choices.

AI usage: I would like to raise a general point regarding transparency in manuscript preparation. Some parts of the main text and the author response letter read somewhat generic and, in places, repetitive. While this may simply reflect the revision process, I encourage the authors to consider including a dedicated statement on the use of AI-assisted tools during manuscript preparation, of course in accordance with the journal's current policy.

Such a declaration would improve transparency by clarifying whether and how AI tools were used (for example, for language editing, grammar checking, coding assistance, or other aspects of manuscript preparation), while making clear that the scientific content, analyses, interpretations, and conclusions remain the responsibility of the authors.

For reference, a possible statement could follow the general structure:

"Artificial intelligence tools were used during the preparation of this manuscript to support language editing, including suggestions for grammar, wording, and clarity, and/or to assist with technical tasks such as code debugging. All scientific analyses, interpretations, and conclusions presented in this study are the work of the authors and have not been generated or substantively altered by AI tools."