Dear Dr. Delaney,

We thank you, Dr. Wild, and the anonymous reviewer for the feedback provided on our revised manuscript (egusphere-2025-4771) and for the suggestions to further strengthen it. We have updated and refined the manuscript in response to the reviewers' comments and suggestions provided during this round of revisions.

Our responses to the comments are shown in blue font.

The revised clean manuscript and its marked-up version have been uploaded.

Thank you and best regards,

On behalf of all coauthors,
Yulia Antropova
Carleton University

Response to the comments from Reviewer 1 (Christian T. Wild) for TC manuscript: egusphere-2025-4771

Dear Dr. Wild,

Thank you for reviewing our responses and the updated manuscript, and for providing your feedback.

To address the main comments regarding the weaknesses of the manuscript, we have:

- provided additional examples of flexure curves (Fig. A4) to better demonstrate the bending effect and its influence on our grounding-zone delineations;
- revised and clarified our approach to using 23 out of the 59 tidal constituents in the reconstruction of ice-surface displacement and added the results of an L-curve analysis to more accurately determine the regularization parameter (Fig. B1);
- revised the manuscript text to better clarify the rationale for our approach within the framework of our analysis.

Our detailed responses to all comments are provided below in blue font.

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

By Christian T. Wild

July 2026

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.

Thank you.

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.

We understand that the bending correction can be extended to two dimensions but this is only practical when gridded ice-thickness datasets are available. We think it is important to keep our analysis to where available satellite, airborne, and in situ observations converge. Our airborne and in situ ice-thickness data are limited to several radar transects, and we do not have a sufficiently dense and evenly spaced network of ice penetrating radar observations to reliably and accurately assess ice thickness across the glacier within and near to the grounding zone. Therefore, our

analysis is built around the radar observations presented in the manuscript, and we consider a spatial analysis across the entire glacier width or DDInSAR scenes to be outside the scope of the current manuscript.

We have added the following explicit statement about limitations of our bending analysis to Section 2.2.3, "Assessment of uncertainties…":

"This analysis was limited to the available airborne and ground-based ice penetrating radar ice-thickness data along the transects (Fig.1)."

We have the following discussion in Section 4.2, "Flexure zone delineation uncertainties during steady and unsteady velocity periods," which addresses the question of generalizing our results for error assessment and, consequently, for the derived grounding-line patterns:

"Additionally, larger tidal displacements and thicker ice can increase errors associated with ice extension or compression caused by bending. In particular, the error due to ice bending can be on the order of one ice thickness based on a 1-D model (Rack et al., 2017). This effect may be reduced within embayments and enhanced around protrusions of the grounding line, as suggested by a 2-D model (Wild et al., 2018)."

We have added the following short discussion of the "alpha-map" approach and the limitations of our simplified reconstruction approach to Section 2.2.2:

"DDInSAR results provide information on differential ice-surface displacement, hence, reconstructing ice-surface height from DDInSAR observations at individual times could provide additional information. For example, a tide-deflection-ratio map (i.e., the ratio of tidal displacement at a given location to the displacement measured in a freely floating reference area derived from DDInSAR observations; Han and Lee, 2014) can be combined with a tide model to predict ice-surface displacement under individual tidal conditions across the SAR-covered area (Wild et al., 2019). Here, we employed a simplified approach without tide-deflection-ratio map to reconstruct the ice surface height timeseries by least square fitting of our DDInSAR displacements using 23 of the 59 tidal constituents with signal-to-noise ratios above 2 computed with the T_TIDE harmonic analysis (Pawlowicz et al., 2002) as well as IBE. We limited the reconstruction to DDInSAR-derived displacement of the freely floating ice tongue along the western and central transects and therefore did not take into account the ice response to changing tides within the flexure zone."

We lack GNSS observations acquired over the freely floating glacier at tidal timescales that would allow us to further assess our reconstruction results and associated uncertainties. This discussion, together with the reasonable results (i.e., the correlation and RMSE presented in the Results section), sufficiently clarifies the rationale for our approach within the framework of our analysis.

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.

We note that we did not use the flexure curves for the hinge-line delineations; rather, we used the DDInSAR images to delineate the hinge line as the landward limit of the dense pattern of several

full fringes, where one full fringe corresponds to approximately 2.8 cm of line-of-sight displacement. We then calculated and reported the horizontal delineation error due to bending based on the maximum possible horizontal surface displacement at our radar-derived ice thickness transects. We consider correcting the flexure curves for the bending effect (c.f., Rack et al., 2017) to be outside the scope of this manuscript.

To better demonstrate the bending effect from the perspective of different orbits, which is more relevant to our study, and its impact on our final results, we added Fig. A5 in the Appendix showing flexure curves from Orbit 09, which has the largest error due to bending, and Orbit 103, which has the smallest (negligible) error due to bending. We also plotted the positions of the final grounding zones for the three transects defined based on the steady and unsteady- periods along western transect, where the results from both periods agreed well, and for the central and eastern transects, where unsteady period resulted in more landward hinge-line delineations than steady period. As the plots suggest, the delineated grounding zones were not affected by the deflection of the curves from zero upstream of the grounding zone observed for Orbit 09.

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.

We reconsidered our approach and included only 23 out of the 59 tidal constituents with signal-to-noise ratio higher than 2. The constituents were included in order from the most to the least dominant tidal constituent. To better demonstrate that included constituents meaningfully contribute to the reconstruction of ice-surface displacement, we plotted RMSE against the number of included constituents (Fig. B1 b in the Appendix). The results suggest that all 23 constituents contribute to a decrease in RMSE, with no visible substantial plateau.

We also added an L-curve analysis (Fig. B1a in the Appendix) to more accurately determine the optimal regularization parameter and avoid overfitting. Figure 6 was updated accordingly based on the selected tidal constituents and regularization parameter, λ.

As noted above, our ice-surface reconstruction represents a simplified method of reconstructing ice-surface displacement at individual tidal stages using tidal constituents, IBE, and DDInSAR observations. Our analysis is limited to the freely floating part of the glacier (i.e., DDInSAR results along the transects) and does not account for the ice response to changing tides within the flexure zone. Therefore, the uncertainty associated with the presented reconstruction does not propagate into the grounding-zone results and does not affect the main conclusions.

In addition, we do not have suitable GNSS observations of ice-surface displacement over the freely floating glacier at tidal timescale against which we could independently assess the reconstruction result. Therefore, we consider further uncertainty analysis not feasible.

We have added the following clarifications to Section 3.2.:

"The demonstrated results are standalone and do not inform other analyses in this manuscript. … Future acquisition of independent GNSS observations over the freely floating glacier at tidal timescales could provide further opportunities to reliably assess our reconstruction results and associated uncertainties."

Minor comments (in chronological order)

Introduction**:**

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

The sentence has been revised as follows:

"Monitoring changes in the position of the boundary where marine-terminating glaciers transition from grounded to floating, known as the grounding line, is critical for advancing our understanding of glacier stability."

l17: Reword "ice tongue" to "glacier"

The requested replacement has been made.

l21: Atmospheric pressure "variability". Here and throughout, it's the pressure anomalies that matter for flexure.

The word "variability" has been added in this instance and throughout the manuscript where deemed appropriate.

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

The requested replacement has been made.

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.

We revised the sentence to formulate this idea more precisely within the framework of our study:

"Our results underscore that care must be taken to ensure that the low-error DDInSAR* results are used when monitoring changes in grounding-line position and that pinning points alter the grounding-line short-term migration and its long-term retreat."

*The term "DDInSAR" was restored instead of "DInSAR" following discussion with Reviewer 2.

l55: add "of relatively fast moving glaciers"

We added the requested wording as follows:

"The comparatively short revisit time of RCM enables high phase coherence, while its C-band wavelength provides sensitivity to small vertical displacements, allowing reliable DDInSAR measurements for grounding-line monitoring 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…"

Thank you. We have changed the term "hydrostatic equilibrium line" to "hydrostatic line" in this manuscript in accordance with your request and to be consistent with several related studies published in The Cryosphere (e.g., Bindschadler et al., 2011; Wild et al., 2025).

However, we note that this feature has been described using different wording in the literature, including The Cryosphere publications. Examples include "limit of hydrostatic equilibrium" (Fricker et al., 2002), "line of 'first' hydrostatic equilibrium" (Rignot, 1998), "landward limit of stable hydrostatic equilibrium" and "point H" (Wallis et al., 2024), "seaward limit of ice flexure where the ice shelf reaches hydrostatic equilibrium" (Ramanath et al., 2025), "inshore limit of the hydrostatic zone of freely floating ice shelf" or "seaward limit of ice flexure" (Fricker & Padman, 2006), and "hydrostatic equilibrium line" and "hydrostatic line" (Gomez-Fell et al., 2023).

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.

We have made the requested replacement.

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

We have made the requested replacement.

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.

We revised the excerpt as follows:

"predicted assuming hydrostatic equilibrium"

L83: Add "differential" displacement of sea surface height.

Thank you. We have added the omitted word.

L89: Add "in-situ measured" velocity changes

The requested wording has been added.

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.

The sentence has been revised as follows:

"Our results provide insight into short-term migration patterns of the grounding line based on its DDInSAR-derived proxy, the hinge line, and help elucidate the processes that drive long-term grounding-line retreat."

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.

The sentence has been removed.

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.

The word "variability" has been added as per comment above (i.e., l21).

The requested replacement has been made.

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

The clarifying adjectives have been added.

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

The sentence has been revised as follows:

"In addition, Antropova et al. (2024) noted that the post-2016 acceleration was preceded by the highest average grounding-line retreat rate of 89 m yr^{-1} along the glacier's centreline between 2011 and 2018, suggesting the presence of feedback mechanisms between grounding-line retreat and glacier acceleration."

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

The word “upstream” has been removed.

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

The requested correction has been made.

L231: Reword to “DInSAR combinations”

“DInSAR subtraction” has been replaced with “DDInSAR combinations” in accordance with the requests of both reviewers.

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..."

We added the following sentence to the list of objectives in the Introduction to make this idea more explicit:

“Test the hypothesis that a newly emerging pinning point along the eastern margin results from inland seawater intrusion driven by high sea-surface height (SSH).”

Testing this hypothesis did not require reconstructing tidal height, as we used simulated tidal heights at the SAR acquisition times to test the hypothesis.

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 semidiurnal 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.

We have included Fig. B 1 in the Appendix, which shows the contribution of the 23 out of the 59 tidal constituents to the reduction in RMSE. As we did not observe an obviously prominent plateau in the plot, we chose to retain all 23 constituents.

We added the following text to clarify this:

"The decision to retain 23 out of the 59 constituents was based on their signal-to-noise ratio and their contribution to the decrease in RMSE (B1 b). The optimal regularization parameter ($\lambda = 3.6$) was determined using the L-curve analysis (Hansen, 1992) shown in Fig. B1 a."

We reconstructed ice-surface height (rather than SSH) at individual SSH stages. The associated uncertainties do not propagate into the presented grounding-zone analysis, which was based on SSH rather than the reconstructed ice-surface height. For the reasons explained in our response to the final comment in the "Weaknesses" section, we consider further uncertainty analysis to be beyond the scope of the current manuscript.

L297: Add "mean" glacier flow heading

The word "mean" has been added.

L298: Add "mean" glacier surface tilt

The word "mean" has been added.

L303: Add "other" glacier flexure zones

The word "other" has been added.

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.

We used SSH (i.e., simulated T_TIDE tides + IBE, rather than reconstructed SSH) at the times of SAR data acquisition to estimate errors due to bending. Although DSSH could also be used directly, we chose SSH because it was easier to incorporate into our existing analysis framework. The reconstruction analysis was conducted for ice-surface height rather than SSH, and the reconstructed ice-surface height was not used in our bending analysis.

We added the following sentence to acknowledge the possibility of using DSSH directly for the bending analysis:

"We chose to use SSH because it was more natural for the logic and sequence of our analysis, although we acknowledge that DSSH could also be used directly instead."

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.

We did not identify any factual errors in this sentence, and AI was not used to write it. We cannot use "3-day and 4-day" in this context because the sentence refers to three or four individual SAR scenes used for each DDInSAR set in our analysis. Perhaps, there is some ambiguity regarding the statement "four days apart" in this context. This statement is always applicable to individual interferograms involving two SAR scenes, but not necessarily to double-difference interferograms, which can have 0-day, 4-day, 8-day, or longer intervals between the second image in the first DInSAR image pair and the first image in the second DInSAR image pair.

We revised this sentence to improve its clarity as follows:

"We delineated 116 flexure zones, from four different orbits (Fig. 3) using the DDInSAR technique applied to three RCM SAR scenes when there were no gaps in the 4-day acquisition cycle, or to four RCM SAR scenes when gaps occurred."

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.

Thank you. We added the ice thickness values to the table and the following text above the table:

"The ice thicknesses at the most landward hinge line did not vary substantially (255–263 m)."

We also revised a sentence in Section 4.1 of the Discussion as follows:

"Our ice-thickness observations revealed only minor differences for the three transects, with thicknesses ranging from 255 to 263 m at the most landward hinge-line position and mean values ranging from 200.5 to 225.6 m within the grounding zone, highlighting the fact that pinning points can further affect the width of the flexure zone."

L360: Remove"pointing up-glacier"

The requested words have been removed.

L382: Add displacement "on freely-floating areas"

We have revised the sentence as follows:

"T_TIDE-simulated tides that were IBE-corrected with in situ atmospheric pressure (i.e., displacement of sea surface height, DSSH) had a higher correlation with DDInSAR-derived displacement of the freely floating ice surface (Fig. 4) than uncorrected simulated tides (Fig. A3 a-c) or simulated tides with GEM-based IBE-correction (Fig. A3 d-f)."

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.

The sentence has been revised as follows:

"This also implies that, most of the time, the eastern part of the glacier, with ice thickness similar to the values measured along the central and western transects (Table 2), was grounded with negligible DDInSAR displacement."

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).

We have revisited our error values due to ice bending and velocity variability and their differences from the corresponding absolute mean values and have revised our discussion of the flexure curves as follows:

"Our uncertainty analysis results, discussed in detail below and shown in Fig. 7, confirmed that the absolute errors due to ice bending were relatively low (0.02 cm and 0.04 cm for the first and the second DDInSAR images, respectively) and smaller than the overall mean error of 0.06 cm. In contrast, velocity variability-related errors (i.e., 0.57 cm and 0.39 cm for the first and the second DDInSAR images, respectively) exceeded the unsteady-period mean value of 0.24 cm. Therefore, we attribute these bumps to velocity variability and the associated surface changes occurring within the acquisition intervals.

Both of these curves also illustrate an additional noticeable displacement trend of diverging from zero at approximately 36 km and subsequently increasing or decreasing up to 7 cm at 41 km, which could be explained by the combined effect of ice bending and velocity variability. The same trend was observed in two other DDInSAR images based on SAR acquisitions from: 1) November 6, 2023 (SSH: 6.4 cm), November 10, 2023 (SSH: 4.9 cm), and November 14, 2023 (SSH: −0.8 cm);

and 2) February 10, 2024 (SSH: −3.3 cm), and February 14, 2024 (SSH: −12.1 cm), March 1, 2024 (SSH: 4.5 cm), March 5, 2024 (SSH: 19.4 cm). Absolute errors due to velocity variability were similar to or smaller than the steady-period mean of 0.06 cm (i.e., 0.06 cm and 0 cm for the first and the second DDInSAR images, respectively), whereas absolute errors due to ice bending (0.15 cm and 0.08 cm for the first and the second DDInSAR images, respectively) exceeded the mean error value of 0.06 cm. We therefore attribute these displacement patterns predominantly to the effect of bending. The pattern of curve deflection from zero near the grounding zone due to ice bending and/or velocity variability was even more pronounced in the DDInSAR displacement results from Orbit 09 and negligible in the DDInSAR displacement results from Orbit 103 (Fig. A4)."

We have also added the labels "Bumps due to velocity variability" and "Divergence due to bending and/or velocity variability" to Fig. 5 in the manuscript. The same trend of divergence due to bending and/or velocity variability is now demonstrated in Fig. A5 in the Appendix, which shows flexure curves from Orbits 09 and 103.

We plotted the discussed curves in Fig. RR1 below. In particular, Fig. RR1 b demonstrates the flexure curves where viscoelasticity was previously hypothesized. However, we consider including this figure in the Appendix to be redundant.

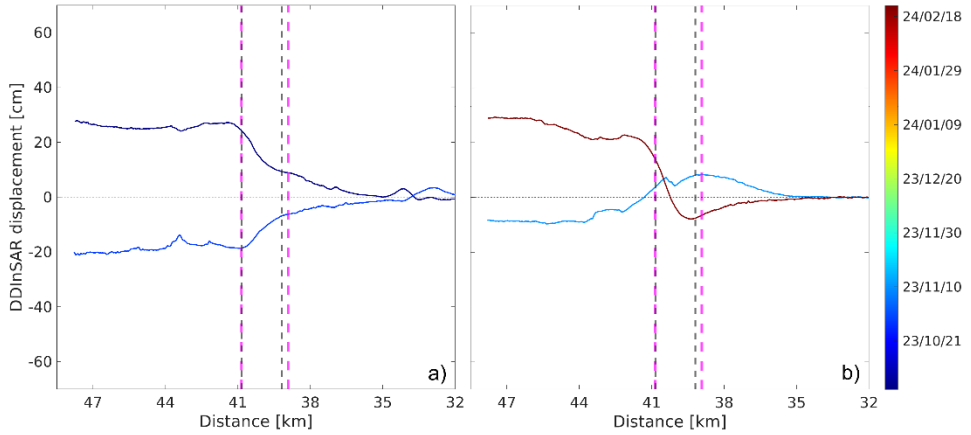


Figure RR1: Examples of DDInSAR-derived surface displacement curves from Orbit 43 along the central transect, showing cases in which the line-of-sight errors exceed the associated absolute mean error values for a) velocity variability and b) ice bending. Grounding zone (GZ) during the steady period and over the entire study period is shown as dark grey and magenta vertical dashed lines, respectively.

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.

As stated above, we added information about this hypothesis to the Introduction as one of the objectives to better highlight this idea throughout the manuscript.

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).

Please refer to our answers to the final comment under the "Weaknesses" section and L240 and 247 minor comment, where we explain the choice of including 23 out of the 59 constituents (with a new figure added in the Appendix) and clarify why we consider further quantification of uncertainty to be infeasible and beyond the scope of this manuscript.

L446: Typo Fig "A4"

Thank you. We have corrected it to "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.

The citations "Rack et al., 2017, Wild et al., 2018" have been added.

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.

We have added the following paragraph to the Discussion section to better explain these ideas:

"While SAR orbit sensitivity to horizontal displacement due to variable velocity and bending contributes to uncertainty in the current study, understanding and accounting for these horizontal components could also benefit the broader glaciological community. In particular, improved characterization and quantification of the horizontal and vertical displacement contributions to DDInSAR-derived deformation could provide further insights into shear-zone weakening (Wild et al., 2025) and help constrain the neutral depth layer (e.g., Rack et al., 2017; Wild et al., 2018). Furthermore, better constraints on the neutral depth layer could support applications that require

more robust estimates of vertical strain within the ice column, such as the assessment of basal melt rates using phase-sensitive radar (Vaňková et al., 2020)."

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

As noted in our comment above, we have added a discussion of possible applications that could be of interest to the broader glaciological community.

Discussion:

L565: Add atmospheric pressure "variability"

This formulation is not needed here because the construction "variations in tides and atmospheric pressure" already implies variability in both tides and atmospheric pressure.

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.

We have revised these sentences as follows:

"These locations have relatively large tidal ranges of about 2-3 m, while other locations, for instance, Rutford Ice Stream in Antarctica, with a tidal range exceeding 7 m, experience even larger tidal forcing (Padman et al., 2018). Atmospheric pressure variability only plays a minor role in the displacement of floating ice shelves and ice tongues in these locations. For marine-terminating glacier systems similar to Milne Glacier, where tidal range is much smaller (< 1 m) than in the aforementioned studies, atmospheric pressure variability plays a more substantial role in controlling SSH, and, consequently, the displacement of the floating ice tongue."

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

The sentence has been revised as follows:

"Based on the flexure-zone variables we analyzed, such as distance to the hinge line and flexure-zone width, we found that the grounding line migrates differently along each of the three transects, despite their similar ice thicknesses and exposure to the same water-level changes."

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

Thank you.

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.

We have incorporated this idea into the paragraph above, which discusses pinning points and the stress regime, as follows:

"Previous studies have shown that the presence of pinning points affects lateral and basal shear stresses, generating upstream compression and downstream tension, which, in turn, influences flow variability and can locally enhance the stiffness of the glacier system and its buttressing (Robel et al., 2017; Still & Hulbe, 2021). In addition, emerging pinning points increase the complexity of the grounding-line shape by forming convex and concave sections, thereby amplifying its sinuosity and further modifying the glacier stress regime (Wild et al., 2018). This can also increase lateral and basal resistance and potentially stabilize the glacier locally. Therefore, the complex interplay between local ice stiffness, grounding line shape, and glacier bed geometry results in the observed flexure-zone widths and governs grounding-line evolution."

L588: Add shallowest "sloping" western transect

The sentence has been revised as follows:

"The widest grounding zone was observed along the western transect, which has the shallowest bed slope, followed by the eastern transect and then the central transect, which has the steepest bed slope."

L603: Reword "SAR data acquisitions" to "InSAR pairs"

The sentence has been revised as follows to avoid terminology issues related to DDInSAR, DInSAR, and InSAR:

"This study is based on a commonly used DDInSAR approach, which assumes steady glacier flow between paired SAR data acquisitions that form a single interferogram, such that the horizontal component of displacement can be removed through differencing of the two interferograms to reveal the vertical component of displacement due to tidal flexure."

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.

We have added an additional paragraph discussing alternative glaciological applications of signal sensitivity, as stated in the comment above (L455–470).

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).

We have revised the text to further clarify our idea as follows:

"These findings support the idea that Milne Glacier, currently resting on a local retrograde slope within its grounding zone, has likely been influenced by a positive feedback mechanism in which grounding-line retreat leads to increased ice flux across the grounding line and further ice thinning, which, in turn, promotes additional grounding-line retreat. This process is often referred to as marine ice-sheet instability (MISI), although the classical theory was developed for an unconfined marine ice sheet and ice shelf system rather than for a marine-terminating glacier (Weertman, 1974; Schoof, 2007). Moreover, interactions between ice flow, basal processes, and environmental conditions can alter this mechanism and help stabilize ice sheets (Sergienko and Wingham, 2024) and, more specifically, marine-terminating glaciers (Sergienko, 2022)."

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

We have replaced "dramatic" with "pronounced".

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.

We have added the requested clarifications as follows:

The most reliable DDInSAR results, with low LOShV (i.e., line-of-sight horizontal component of displacement due to velocity variability) and LOShB (i.e., line-of-sight horizontal component of displacement due to ice bending) errors and associated uncertainties within one pixel, were obtained from orbits with low incidence angles and ascending tracks.

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

We have added the following sentence:

"The highest uncertainty, approximately 22 pixels, was obtained for an orbit with a high incidence angle and a descending track, which was most sensitive to horizontal ice-surface displacement caused by velocity variability and bending."

L689: Add atmospheric pressure "variability"

We have added the word "variability" here.

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.

We note that the need for detailed bed information was also discussed at the end of Section 4.1.

We have revised the concluding sentences to better articulate this idea here as well, as follows:

"Consistent and frequent SAR observations over Arctic and Antarctic glaciers in combination with improved datasets on their bed geometry and ice thinning will ultimately improve our understanding of ice deterioration in the grounding zone and its future evolution. In particular, detailed bed geometry upstream of the grounding zone, in regions that may become marine in the future, is key information required to improve glacier modeling capabilities and predictions."

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.

We have changed the line colour for the "IceBridge and IPR data" to improve its visibility. However, we prefer not to add an ice-thickness colorbar to this introductory figure, as we believe it would make the figure overcrowded. Ice thickness is already shown using a colobar and cross-sectional plots for each of the transects in Fig. 9. The transect labels have also been added.

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.

We have replaced the atmospheric pressure records with pressure anomalies. We prefer not to add the corresponding IBE records to the right axis of the manuscript figure because, as expected from the calculations, they would either mirror the pressure-anomaly record (Fig. R1) or overlap with it, depending on the sign convention, which makes the plot overcrowded.

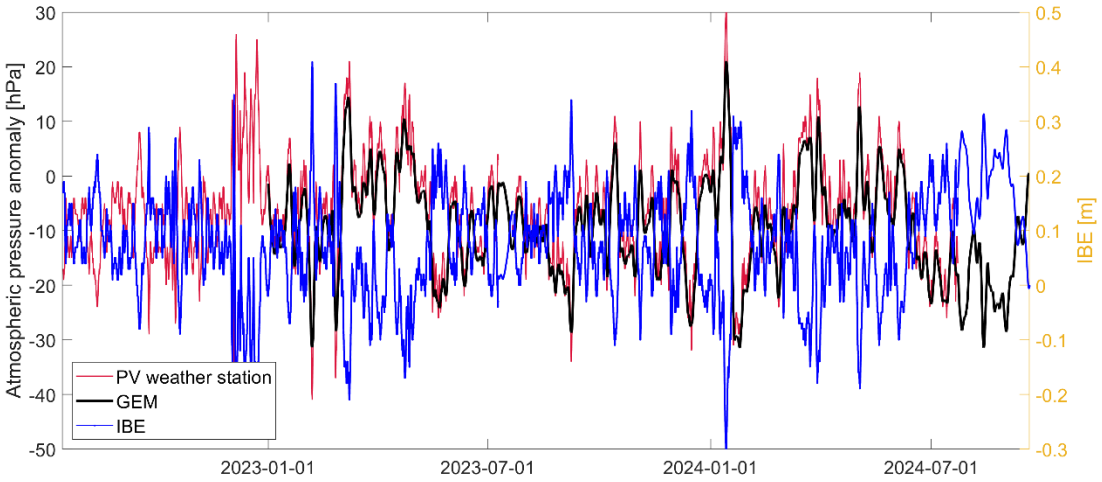


Figure RR2: Atmospheric pressure anomaly and corresponding IBE.

Yes, GEM has a negative bias compared to the AWS. We have added this information to the manuscript as follows:

"In particular, the correlation between the GEM model and in situ atmospheric pressure records used for IBE calculations was 0.96, with a root mean squared error (RMSE) of 3.6 hPa, and a bias of -2.4 hPa."

The blue and red shadings were added to the panel 2c (as per request below).

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

Following the discussion with Reviewer 2, we decided to retain the "DDInSAR" terminology. We have added explanations regarding this term and its usage to the Introduction.

Figure 4: good

Thank you.

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

We have added the arrows and labels.

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).

This comment likely refers to Fig. 5, which shows DDInSAR-derived surface displacement from Orbit 43. If so, Fig. 5 already demonstrates all flexure curves for both the steady and unsteady

periods. As shown in the figure caption, the combined line-of-sight mean error during the unsteady period was approximately 0.3 cm, which is negligible relative to the scale of the figure and much less than half the RCM radar wavelength (i.e., full fringe used for flexure-zone delineations) in LOS displacement. As discussed in the "Weaknesses" section, we have added flexure curves from Orbit 09, where the bending effect is evident, and from Orbit 103, which is almost unaffected by bending. Therefore, we consider adding a second row of panels to Fig. 5 redundant. We also consider correction of the flexure curves for the bending effect to be beyond the objectives of this study.

If the comment instead refers to conducting ice-surface-height reconstruction (Fig. 6) using the unsteady-period DDInSAR dataset, we would prefer to retain our current approach and keep the figure as is. Our reconstruction approach is based on selecting DDInSAR observations with the smallest errors, i.e., observations from the stable season, as confirmed by the LOShV and LOShB error calculations, and subsequently applying least-squares fitting. Our additional analysis indicates that the statistical performance improves when only DDInSAR observations from the stable season are used, compared with using all DDInSAR observations. Therefore, we consider the use of unsteady-season datasets unnecessary and redundant for the reconstruction analysis.

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

Thank you.

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)?

We have added the blue and red shading to Figs. 2c and 6a. Fig. A1 shows July only, which falls outside the shaded periods.

Figure 9: good

Thank you.

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.

We have reversed the y-axis direction.

Appendix A

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

Good.

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.

We have changed the labels to +π/-π.

Figure A3: good

Thank you.

Figure A4: good

Thank you.

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.

The L-curve has been added to the Appendix, as noted in our response to the "Weaknesses" section above. The regularization parameter λ was adjusted accordingly to avoid overfitting.

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."

Thank you for raising this question, particularly given the growing relevance of this topic. We are happy to clarify our use of AI tools in this manuscript. Before submitting the initial version of the

manuscript, we consulted The Cryosphere (TC) AI policy (https://www.the-cryosphere.net/policies/ai_policy.html), because several paragraphs in the Discussion section were edited using AI tools to improve sentence structure and readability, including punctuation, grammar, and spelling. According to the TC AI policy, this constitutes the "assistive" use of AI.

During the revision process, all responses were written by the first author or, where required, by the co-authors, and were often subsequently edited using AI tools (i.e., within the assistive use of AI). The responses were then reviewed and further edited by all authors. No text or code used for the analyses and presented in this manuscript was generated by AI (i.e., "generative" use as described in the TC policy), and AI was not used for code debugging.

We also acknowledge that English is not the first language of the first author, which may have contributed to some aspects of the writing style and possibly to the impression of "generic" or "repetitive" text. We also agree that some repetition may have been introduced during the revision process.

Our use of AI tools was minor and limited to the assistive use described in the TC AI policy, which does not require any declaration under the current TC AI policy. Therefore, we do not consider adding the suggested statement to be relevant to this manuscript and hope that this response clearly and transparently explains our use of AI tools in the manuscript preparation.

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Response to the comments from Reviewer 2 for TC manuscript: egusphere-2025-4771

Dear Reviewer,

We thank you for reviewing our manuscript and for your positive feedback on our revisions and on the resulting manuscript. We also appreciate your detailed clarifications regarding the DDInSAR/DInSAR terminology. In response to this major comment, we have revised the DDInSAR/DInSAR terminology and added corresponding clarifications to the manuscript.

Our detailed responses to all comments are provided below in blue font.

Review of egusphere-2025-4771: Intra-annual grounding line migration and retreat: insights from high-resolution satellite and in-situ observations over Milne Glacier in the Canadian High Arctic

This is a very well-written manuscript. I recognize that there have been some confusing and differing definitions of the flexure zone and grounding zone across different communities. In simple terms, the grounding zone encompasses the full range of grounding line positions, whereas the flexure zone is the area of the glacier that experiences tidal flexing. These concepts are now well described in the manuscript following the first revision.

This is the second round of revision, and I am taking over the previous reviewers' work. I can see that the authors have made a substantial effort to address the previous comments, and the manuscript has been significantly improved. Overall, I think the authors have done an excellent job revising the manuscript.

However, I strongly disagree with the terminology change from DDInSAR to DInSAR. I recognize that raising this issue again may cause additional work for the authors, but I believe that correcting the terminology would make the methodology and the manuscript considerably clearer.

Major comment: DDInSAR vs. DInSAR

I would like to reiterate my concern regarding the change from "Double Difference Interferometric SAR (DDInSAR)" to "Differential Interferometric SAR (DInSAR)." After reviewing the revised manuscript and the authors' response, I believe this terminology change should be reconsidered because it may not accurately describe the processing performed in this study.

As I understand the methodology, the authors first generate individual interferograms and remove the topographic phase from each interferometric product. This constitutes differential InSAR (DInSAR) processing, as described in L164; indeed, the processing described there is exactly the DInSAR step. They then difference the two resulting DInSAR products to isolate the phase associated with vertical displacement, assuming that the horizontal displacement is constant. This

subsequent differencing of two DInSAR measurements is commonly described as double-difference interferometry (DDInSAR).

Thus, in this context, DInSAR and DDInSAR are not interchangeable terms. Rather, DInSAR describes the differential interferometric processing applied to the individual interferograms, whereas DDInSAR describes the subsequent differencing of two such differential interferometric products.

I therefore do not agree with the rationale in the previous review comment that "differential" implies that two interferograms are being differenced. Differential InSAR does not inherently require the differencing of two interferograms. For example, in a two-pass DInSAR approach, the topographic contribution can be estimated using an external DEM and removed from a single interferogram. The use of additional interferograms in three- or four-pass approaches does not, by itself, make DInSAR synonymous with double-difference interferometry.

I also do not believe that the statement that DInSAR is the "modern terminology" for DDInSAR is sufficiently supported. Both terms remain in use in recent literature, and their usage is not completely uniform. More importantly, recent studies in the same application domain explicitly distinguish the two concepts. For example, Andersen et al. (2026), in a recent study of grounding line mapping, describe DInSAR as the individual differential interferometric measurement and explicitly define the differencing of two sequential DInSAR measurements as "double-difference interferometry." This is directly consistent with the processing terminology used in the present manuscript.

For this manuscript, I therefore recommend retaining "Double Difference Interferometric SAR (DDInSAR)" (or "double-difference interferometry") for the final differencing step used to determine grounding-line migration. The authors could clarify the relationship between the two terms at first occurrence, for example, by stating that the DDInSAR product is obtained by differencing two DInSAR interferograms. This would eliminate any ambiguity while preserving the distinction between the differential interferometric processing of the individual interferograms and the subsequent double-difference operation.

If my understanding of the processing described in the manuscript is correct, I strongly recommend that the authors reconsider this terminology change.

Yes, this is a correct understanding of the processing steps.
We acknowledge that there is some ambiguity in the terminology used in the literature. For example, the technique has been referred to as:

- DInSAR (Rignot et al., 2011, Rignot et al., 2024, Wild et al., 2025), although Rignot et al. (2024) do not provide an explicit description of topographic phase removal;
- quadruple-differential InSAR or quadruple-difference interferometry (Rignot, 1996; Rignot et al., 2014);
- Double Difference (or Differential) Interferometric Synthetic Aperture Radar (DDInSAR) (Han & Lee, 2014; Antropova et al., 2024; as well as the original version of this manuscript);
- double-difference interferometry (Chen et al., 2023; Andersen et al., 2026).

Therefore, our understanding is that there is no single, consistently used term for this technique. As long as the processing steps are clearly described, all of these terms have been used and accepted in the literature.
Given your comment, the recent use of "double-difference interferometry" as equivalent to DDInSAR in The Cryosphere (Andersen et al., 2026), and for consistency with our previous publication (Antropova et al., 2024), we have reverted to the term DDInSAR throughout the manuscript.

The following clarifications have been added to the Introduction:

"This method is based on computing interferograms with the topographic phase removed to reveal surface displacement, followed by differencing two interferograms to eliminate the horizontal ice-flow component of displacement and isolate the vertical displacement associated with tidal forcing (Rignot, 1996). Although different terms have been used to describe this technique, including DInSAR (Rignot et al., 2011; Wild et al., 2025) and quadruple-difference interferometry (Rignot, 1996; Rignot et al., 2014), hereafter we refer to this method as DDInSAR, consistent with Han and Lee (2014) and Antropova et al. (2024), and equivalent to double-difference interferometry (Chen et al., 2023; Andersen et al., 2026)."

Figures 4, 6, 7, A3, A4 have been modified accordingly.

- Andersen, J. K., Millan, R., Rignot, E., Gimenes, L., Scheuchl, B., Barré, J. B., & Bjørk, A. A. (2026). Brief communication: Updated grounding line mapping in the Amundsen Sea Embayment, Antarctica, from one day repeat Sentinel-1 SAR data. The Cryosphere, 20, 1589–1598. https://doi.org/10.5194/tc-20-1589-2026.

Minor comments

- L326: The citation should be (Porter et al., 2023) without the first name. I understand that the ArcticDEM webpage provides the citation as "Porter, Claire, et al., 2023, 'ArcticDEM, Version 4.1'..." However, when following the DOI, the actual citation is: "Porter, Claire; Howat, Ian; Noh, Myoung-Jon; Husby, Erik; Khuvis, Samuel; Danish, Evan; Tomko, Karen; Gardiner, Judith; Negrete, Adelaide; Yadav, Bidhyananda; Klassen, James; Kelleher, Cole; Cloutier, Michael; Bakker, Jesse; Enos, Jeremy; Arnold, Galen; Bauer, Greg; Morin, Paul, 2023, 'ArcticDEM - Mosaics, Version 4.1'..." Therefore, the citation in the manuscript should follow the standard author–year format and use "(Porter et al., 2023)"

Thank you for noticing this typo. We have removed the first name from the citation.

- L347: I think you are referring to the whole of Fig. 3 rather than to a single panel (Fig. 3a).

Thank you. We have corrected this typo.

- Figure 5: I believe the unit on the y-axis should be cm rather than m. It is also good to see that "DDInSAR" is retained here, which is appropriate given the processing described in the manuscript.

Yes, the DDInSAR displacement was supposed to be reported in cm here. We have corrected the typo. Thank you.

Otherwise, this is a very nice paper with important results, and I appreciate the authors' considerable effort in revising the manuscript. I recommend major revision primarily because of the terminology issue discussed above. I look forward to seeing the paper published after this issue has been addressed.

Thank you!

References:

Andersen, J. K., Millan, R., Rignot, E., Gimenes, L., Scheuchl, B., Barré, J. B., & Bjørk, A. A. (2026). Brief communication: Updated grounding line mapping in the Amundsen Sea Embayment, Antarctica, from one day repeat Sentinel-1 SAR data. The Cryosphere, 20, 1589–1598. https://doi.org/10.5194/tc-20-1589-2026.

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