Response to Reviewers' Comments

Manuscript egusphere-2025-567

Wintertime Evolution of Landfast Ice Stability in Alaska from InSAR

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Overview

Below, we provide our responses explaining how we have addressed each of the reviewers' comments. For clarity and ease of reference, we have broken some comments into individual points. Our responses to all comments are highlighted in blue. Descriptive comments that we feel did not include specific points that required addressing are italicized in grey text.

Reviewer #1 comments

The authors present a study that proposes an InSAR coherence threshold to be used with Sentinel-1 image pairs to find the extent of landfast sea ice on the north coast of Alaska. They mention a loss of coherence in at the beginning and end of the winter season. Most of the study is an extension of Damman et al. (2019), with the current study finding 'apparent strain' values that separates landfast sea ice into three categories: bottom-fast ice, stabilized ice, and not-stabilized ice. The authors describe nuances related to the location of the category thresholds and the seasonal evolution of strain values. The efficacy of the methods is evaluated against a climatology for landfast sea ice extent.

Major points

The Introduction is rather brief. Please include material defining and relating stability, strain, and displacement, in the context of sea ice. Please also include a survey of similar InSAR techniques for sea ice, including the references listed below, and particularly a more-detailed description of the works on which this study is built (i.e., Dammann et al. 2019; Meyer et al. 2011; Pratt 2022).

- Dammert, P. B. G., Lepparanta, M., & Askne, J. (1998). SAR interferometry over Baltic Sea ice. International Journal of Remote Sensing, 19(16), 3019-3037.
- Li, S., Shapiro, L., McNutt, L., & Feffers, A. (1996). Application of satellite radar interferometry to the detection of sea ice deformation. Journal of the Remote Sensing Society of Japan, 16(2), 153-163.
- Morris, K., Li, S., & Jeffries, M. (1999). Meso-and microscale sea-ice motion in the East Siberian Sea as determined from ERS-1 SAR data. Journal of Glaciology, 45(150), 370-383.
- Wang, Z., Liu, J., Wang, J., Wang, L., Luo, M., Wang, Z., ... & Li, H. (2020). Resolving and analyzing landfast ice deformation by InSAR technology combined with Sentinel-1A ascending and descending orbits data. Sensors, 20(22), 6561.

Thank you for pointing out we need to include more content relating to sea ice and more specifically landfast ice stability. In addition to adding content relating to the 3 articles mentioned, we plan on adding more content in the introduction relating to the 3 papers which formed the foundation of this work: Meyer et al. (2011), Dammann et al. (2019), and Pratt

(2022). These more thorough descriptions of the mentioned journal articles, in addition to other supplementary texts, will provide the readers with a complete understanding of landfast ice stability, the techniques previously used to observed stability, and the utilization of InSAR in the sea ice field.

The map figures in Figures 1, 2, 3, 5, 6 and 9 are inconsistent in the application of standard map elements. Use a consistent lat/lon grid, scalebar type, land colour, and shadow zone colour throughout.
We will create a standard style used in all maps and figures. The style will use similar colors to represent similar meaning (Ex. stability categories having the same color on a map and plot figures). The style will be CVD-friendly.

The use of the 0.1 coherence threshold value found by Meyer et al. (2011) for L-band is not well-supported for C-band. Coherence values for C-band are usually significantly higher for sea ice. The assumption of an adequate trade-off between temporal baselines and wavelengths may be true, but this should be supported with evidence. A sensitivity study related to the Figure 4 results may help. Please also provide a representative example of a coherence image alongside a calibrated SAR image, showing landfast sea ice and open water or mobile sea ice. The coherence values should be presented in such a way that they are easy to discern.
You are correct that we did not substantiate our choice of the 0.1 coherence threshold. Our plan is to take a subset of our data, 5-10 images from the Chukchi region and 5-10 from the Beaufort region from various months and apply different coherence thresholds to identify landfast ice. We can then compare the position of the seaward landfast ice edge identified as landfast ice by each threshold. In addition, when we chose the 0.1 threshold, we did some general testing to confirm our choice and noticed when increasing the threshold above 0.1 the areas identified as landfast ice constrained a considerable number of holes. These localized areas of low to zero coherence are still considered landfast according to our definition. We also noticed the landfast ice edge did not change considerably when adjusting the threshold. Regardless we need to substance these observations within this article. We will also include the representative example of a coherence image alongside the calibrated SAR image as requested.

The use of the 90th percentile (Figure 8) as the threshold between stabilized and not-stabilized is not convincing. It seems to be based on a rather vague notion that some of the not-stabilized is actually stabilized. This is presented without any supporting evidence. The 10th percentile threshold is also not well-supported. A more robust method for finding the thresholds should be applied. There are many statistical methods to choose from. Given the mix of distribution types, a non-parametric technique such as a decision tree is recommended.
We appreciate the constructive criticisms of our methodology. We believe that this comment is the result of poor explanation of the methodology. We believe the miscommunication occurred when differentiating between sheltered vs stabilized landfast ice. We will better define and differentiate sheltered landfast ice from stabilized in the updates of the article. However, we would like to provide some text provide context and a better explanation. When deriving the apparent strain thresholds we created 3 classes: bottomfast, sheltered, and not sheltered. These classes were not based on any observations we made. Instead, the extents of each class were either defined by a pervious study, Dammann et al. (2019) for bottomfast ice, or geomorphologic characteristics of the coastline. The extent of the sheltered landfast ice polygon (orange polygons in figure 2) indicates areas of the coastline which have barrier islands oceanward of

that position. The positions of these barrier islands are known and negligible between seasons. Therefore, we know that whatever landfast ice forms within the bounds of the orange polygons will have a grounded feature, the barrier island, oceanward of its position. Barrier islands perform 2 roles for the landfast ice formed shoreward of the barrier island, sheltering the fast ice from dynamic forces such as wave and currents and provides an offshore anchor point or stabilizing feature. Similarly, grounded ridges provide an offshore stabilizing feature for fast ice shoreward of its position. The ridges will mitigate some of the dynamic forces, such as attenuation of wave energy, but does not shelter the fast ice the same way the barrier islands do. Conceptually sheltered fast ice fast ice that is shoreward of a barrier island where the fast ice is sheltered from dynamic forces and stabilized by the barrier island where stabilized fast ice is just stabilized by an offshore grounded ridge.

Our theory when coming up with a way to determine the apparent strain thresholds for the stability categories used the following thought process. We know that grounded ridges do not always form in the same location each season thus we cannot identify a line of grounded ridges that would accurately delineate stabilized fast ice from not-stabilized inter-seasonally. We also know that barrier islands provide a similar role to grounded ridges in terms of stabilizing and the location of the barrier islands does not change inter-seasonally. When looking at Figure 2, any fast ice which forms in the sheltered region, orange polygons, is guaranteed to have a stabilizing feature offshore of its position in the form of a barrier island. Within the blue cross filled regions fast ice formed here it is possible the landfast ice formed here has an offshore stabilizing feature, in the form of a grounded ridge, but it is also possible there is not a grounded ridge offshore of its position. It is this nuance that separates sheltered from stabilized and why we define the upper threshold of the stabilized fast ice as the 90^{th} percentile of the sheltered fast ice apparent strain. One last important distinction is that the classes bottomfast, sheltered, and not sheltered are defined by the location of where the fast ice resided where the stability categories bottomfast, stabilized, and non-stabilized are based on apparent strain values.

We will incorporate better and more thorough explanations of these nuances and within the updated text. We will include explicit definitions of the stability categories, and the classes used to derive the stability thresholds.

However, it seems that the use of monthly averages is obscuring the abrupt change from stabilized to not stabilized strain values, as can be observed in the individual scenes in Figure 11. Perhaps a more meaningful method can be found to estimate the strain thresholds, based on individual images and the coast vectors.
This is a great suggestion. We plan to explore a methodology which utilizes single apparent strain maps to derive the thresholds. However, we will still use apparent strain maps from the month of April as the fast ice has the best coherence and the most fast ice using this method along the Beaufort coastline.

Overall, the paper needs to substantiate the coherence and strain thresholds further. It may be better to localize the analysis to two or three sub-regions, and investigate these in more detail, as was done in Figure 11, in concert with air temperature data. This may lead to a better understanding of what is affecting the coherence and strain values, and lead to better estimates for the strain thresholds.
We see the value in having localized analysis. We do not believe the thresholds will vary greatly if the analysis local. With regards to incorporating air temperature into our analysis, which we

believe the reviewer is meaning strain associated with thermal contraction/expansion of the fast ice we are doubtful adding air temperature to our analysis will improve our understanding. An analysis that includes air temperature and uses air temperature in concert with interferometric phase changes in a 2D inverse modeling method was done by Fedders et al (2024). Their methodology excluded interferograms with curved fringe patterns and their study area was limited to Elson lagoon, a sheltered embayment where the fast ice is not exposed to significant dynamic forces. In Elson Lagoon the deformation of fast ice is dominated by thermal forces. Since our analysis includes areas which are dominated by both dynamic and thermal forces. Without the ability to assume what the dominant forces are and that they vary across the study area it is not possible to use the 2D method demonstrated by Fedders et al (2024). In line with a related minor comment, we plan to create a confusion matrix which compares how pixels were categorized by Dammann et al (2019) and our stability categories to validate our thresholds.

The analysis of the strain images to identify grounded ridges is a useful element of this study, and one that could be expanded upon by analyzing more image pairs of smaller regions.
We agree with the need for more data, however the data included in the analysis is all the data made available to the authors. We included over 2000 Sentinel-1 A/A or B/B pairs in our analysis and utilized all data available to us.

Minor points:
Line 52: In Figure 1, the eleven smaller areas should be briefly mentioned in the text, or if they are unimportant, they can be removed from Figure 1. Indicate what the difference is between the cyan and blue vectors.
The authors realize the ambiguity of the numbers and colors contained in Figure 1. We will improve the figure and supporting text to clearly indicate that the numbers in figure 1 represent the subregions in Figure 10. The colors there are shaded regions the align with the regions described in Figures 4 and 10. We will improve the coloring of the coast vectors as we mistakenly used the same colors to differenced adjacent subregions as the shaded regions denoting the regions.

Line 66: If the coast-normal vectors are conceptual in Figure 1, please indicate that in the caption. Otherwise, indicate the time period that the vectors represent.
The coast vectors represented in Figure 1 are every 10^{th} coast vector. We will clarify this in the caption and apply corrections associated with the colours from previous comments regarding Figure 1.

Line 88: Please describe the “other processes unrelated to motion that reduce coherence”, with references.
We will add text describing the processes which are not associate with sea ice drift which can affect the coherence. Specifically, we will discuss surface melt, snow drift, and surface deformation.

Line 93: The grammar needs to be improved for this sentence.
We will adjust the grammar to the following: Following the work of Meyer et al. (2011), we apply the same coherence threshold to identify landfast ice using C-band Sentinel-1 imagery,

under the assumption that the increased coherence resulting from a shorter repeat orbit interval at least partially offsets the reduced coherence associated with the shorter wavelength.

Line 146: How can the extent of the 'not sheltered ice' be known a priori, in order to create a mask for it. Also, it is not clear what these masks will be used for.
In line with previous major comments, we will improve our description and distinction of the sheltered and not sheltered regions. To directly address this comment, we defined the sheltered fast ice region as any area where landfast ice forms which has a barrier island offshore of its location. By deduction then any area where fast ice forms which a barrier island does not have offshore of its position is considered Not Sheltered. The offshore edge of the Not Sheltered was created such that all fast ice identified using the coherence threshold is accounted for.

We will also improve the clarity of text associated with the masks represented in Figure 2 to explain how they were defined and how they are used.

Line 160: The Figure 3 caption references Figure 1 regarding the 'shadow' zones. However, these zones are not indicated or obvious in Figure 1. Also, what do the shadow zones represent? Please indicate the Chuckchi-Beaufort border in Figure 3.
The reviewer is correct. We mistakenly omitted the shadow zones within Figure 1. We will add these regions to Figure 1. These shadow zones are areas where our coast vectors do not reach, mainly associated with complex coastline shapes. The coast vectors cannot cross a landmass which is not an island. For example, Admiralty Bay, ~50 km east of Barrow is a shadow zone as the coast vectors originate at the shoreline of Elson Lagoon. Additionally, Near Kotzebue the coastline is quite complex and since the coast vectors cannot cross any non-island landmass areas such as Hotham Inlet and Selawik Lake are classed as shadow zone. We will also add similar region boundaries from Figure 1 to Figure 3.

Line 163: In Figure 4, the x-axis text is too large, with the location names bleeding into one another. Also, these locations do not seem to align with the sub-regions in Figure 1. There are two Kotzebue locations on the x-axis.
We will alter the x-axis labels such that they do not overlap. The labels represent villages or coastline features, not the sub-regions. We will alter these labels to be oriented to the centre coast vector within each subregion and have the subregion name.

Line 172: In Figure 4e, the high variability in extent just east of the Chuckchi-Beaufort border is not evident in Figure 3, which is also for April. Please explain the inconsistency between figures.
The variability at the boundary between the Chukchi and Beaufort regions is associated with Kotzebue Sound. The order of the coast vectors is a bit jumbled but will be sorted properly in the edits.

Line 191: Please explain how the percentage values (y-axis in Figures 7, 8, and 10) are calculated. What does percentage represent, especially in Figure 8? The percentages do not appear to add up when comparing Figures 7 and 8. Also indicate in the Figure 7 and 8 captions, the region the data represent.
Thank you for pointing out the confusion. Figure 7 is the distribution of both the Chukchi and Beaufort regions for each month while Figure 8 is just from April in the Beaufort region. The bins are spaced differently in Figures 7 and 8 to provide a clearer result in Figure 7. The larger bin sizes smooth the distribution and allows for easier distinction between the months. Figure 7

becomes cluttered when a similar bin window to Figure 8 is used. We found it important to depict the noise in Figure 8.

Line 201: In Figure 8, please add an overall distribution so that the reader can see if the modes are present in the overall un-masked data.
We can add the total distribution to Figure 8. This will not be the same distribution as plotted in Figure 7 (cyan with squares) as it will just be the Beaufort region. We do see the value in having these data plotted on Figure 8 and will add to the plot.

Line 224: Is this single April 2017 comparison the only validation for the proposed thresholds for stability classes? If so, then the evidence is not convincing enough to say that the proposed threshold "…can be usefully applied…". The large areas seemingly misclassified as bottom fast ice are adjacent to much of the not-stabilized ice. This juxtaposition does not support the statement that "the boundary between stabilized and non-stabilized landfast ice agrees between methods". Furthermore, the outer extent of the not-stabilized ice is significantly greater in Dammann et al. (2019), which does not support the statement "both methods show good agreement on the position of the SLIE". This comparative analysis should be redone with additional data. Given the delineations from Dammann et al. (2019) it is reasonable to include a quantitative comparison, e.g., a confusion matrix.
The review is correct that this the only validation for the thresholds. We will produce a confusion matrix as suggested to compare how our classifications aligned with Dammann et al. (2019). With the different method for the threshold derivation using single SAR pairs instead of the monthly average we are confident the thresholds will produce a confusion matrix where the majority of pixels are classified the same using both methods.

Line 230: Is the 'particular time' four years of April data? If so, then are the data for this region's stabilized ice included in the distributions in Figure 8? If this is an anomalous area, then why not use a more representative area?
"this particular time" refers to the SAR pairs acquired from April of 2017 used by Dammann et al (2019) and this study. We will rephrase to clarify this statement.

Line 237: Should this not be > 0.1?
Yes. Thank you for point this out. We will correct the text to mean greater than 0.1.

Line 240: Provide a quantity instead of 'slightly less'.
We believe the best statistic to quantify the difference would be a percent difference of the fast ice width at each coast vector. We will calculate this value and likely include mean value for each month in a table.

Line 250: Where is the Colville Delta?
We will update all maps to include better labels of places and features references within the text.

Line 252: Please provide the May air temperatures to corroborate the attribution.
We can provide general idea of the air temperate in May and how this likely lead to surface melting. In addition, we will show the degradation of the coherence from April to May and June do depict why we believe the cause is surface melting not deformation.

Line 253: It is not clear how or where the 12-day repeat prevents landfast ice identification.

Thank you for pointing this out. This is an awkwardly phrased sentence that we will improve. We were trying to indicate that with a 12-day orbit interval the combination of C-Band and the surface melt cause the loss of coherence. The way we posed the sentence this way was there is shorter orbit intervals, and we believe this would allow for better coherence values during the spring season.

Line 261: Refer to Figure 7 in the first sentence.
We will refer for Figure 7 in the first sentence of this paragraph.

Line 265: In Figure 10, should not Figure 10l (whole study region) be the same as Figure 7? The values and monthly peaks are different. Also, is 'interferometric phase gradient' in the caption supposed to mean 'apparent strain'?
Figure 10l and Figure 7 contain the same data. The y-axis limit is different which could cause a perceived difference between the plots.
The reviewer is correct that in the caption "interferometric phase gradient" should be Apparent Strain.

Line 269: Is the extent not a function of coherence, which is indicated to be poor in May. Does this affect the pdf results shown for May?
This brings about an interesting point about defining landfast ice and the method and criteria needed to be classes as landfast ice. You will note that in Figure 4 the coherence-based method for identifying fast ice consistently underrepresented the amount of fast ice compared to the EM2024 dataset in May. We observed that in June the coherence-based identification of fast ice did not work due to suspected surface melting. The method struggles to accurately identify fast ice once melting occurs at the surface. This is a bias with this method and why we limited our analysis to the winter months (December-May).
While we identified the coherence during the month of May as poor, there were still areas which met our coherence-based requirements. We believe that the poor coherence during May is acceptable since we are aware of the bias. We will include addition text to ensure that we fully describe that shortcoming of the method. May differs from June as there is still extensive areas identified as fast ice during May.

Line 287: The statement that the apparent strain threshold 'work well' has yet to be shown. This may need to be revised in light of previous comments.
After the new methods and threshold evaluation are conducted, we will reassess this sentence. We will also ensure that we do present how well the thresholds work (i.e. confusion matrix).

Line 301: In Figure 11's caption, indicate that the thick red line is the boundary between the stabilized and not stabilized landfast ice. Indicate the location of the red line in panels c and d as well.
We will implement the boundary between Stabilized and Not Stabilized from panels a and b into panel c and d.

Line 304: Some of the text in Figure 11c and d is too small.
We will alter Figure 11 to ensure all text is an appropriate size and legible.

Line 302: The sentence needs to be reworded: "This boundary by another steep…".

Thank you for pointing this out. The sentence should read “This boundary is marked by another steep gradient in apparent strain, but one which difference from those associated with tide crack in that it represents a step change between regions of comparatively low and high apparent strain.”

Line 320: The factors unrelated to motion causing a loss of coherence could be investigated in this study.
In response to other comments, we plan to investigate air temperature and the associated thermal expansion and contraction of the landfast ice.
Line 323: Why would a shorter period improve coherence if the cause is temperature and snow moisture related? An analysis of the air temperature would likely provide some evidence towards a cause.
The shorter orbit time simply provides less time between acquisitions for the surface to change (melt or snow event). With a shorter period between acquisitions there is less opportunity for these processes to degrade the signal.

Line 343: The thickening of the ice is a reasonable assumption as to the cause of the seasonal decrease in apparent strain. This should be investigated in this study, using air temperature data and a simple freezing model.
We can implement a simple freezing degree day model to approximate the ice thickness over the season from 2017-2021 to justify our hypothesis that the reduction in apparent strain throughout a season is associated with thickening of the landfast ice.

Line 347: Why was a 2-D method not pursued?
The 2D model demonstrated by Fedders et al. (2024) had differences compared to this study which made a similar analysis not possible. The study area for Fedders et al. (2024) was Elson Lagoon. Based on the coastal morphology or Elson Lagoon and the requirement that the fringe patterns had to be simple, Fedders et al. (2024) was able to assume that the dynamic forces such as waves, winds, and pack ice interactions were small and the dominate forces deforming the fast ice were thermal forces. Since our study contained fast ice where we did not know if dynamic or thermal forces were dominating the 2D method is not possible.