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Interactive comment on "Landfast sea ice stability – mapping pan-Arctic ice regimes with implications for ice use, subsea permafrost and marine habitats" *by* Dyre O. Dammann et al.

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Reviewer 1

Dear reviewer,

We greatly appreciate your insightful comments. We have followed all your recommendations, which we acknowledge has led to an improved manuscript. Please see detailed responses below.

With best regards, Dyre Oliver Dammann

Dammann and others present a nice study demonstrating a novel utility of SAR In-

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Discussion paper

terferometry (InSAR) for mapping landfast sea ice. They show it is possible to classify several different types of landfast sea ice as well as demonstrate potential predictability for ice arch collapse using Nares Strait as an example. This paper is well-written and the figures clearly fit the text for which they were created. I do however have a couple suggestions that I think need to be addressed prior to publication.

General Comments

1. It is possible to classify landfast ice directly from calibrated SAR imagery. In this respect, it would be useful to illustrate to the reader how much additional information C1 the interferogram provides. A good way to do this would be show a SAR image in Figure 3 and point out how identifying the individual landfast classes without the use of an interferogram would be difficult.

Great point. We have followed your suggestion and included the backscatter now as Figure 5b. We have also commented on this by including:

P6 L26: “The respective master images exhibit a sharp discontinuity in backscatter (see arrows in Figure 5b) along the general location of the landfast ice edge from Figure 5a and can be assumed to be the landfast ice edge. Determining the landfast ice edge can in some instances be achieved by evaluating a single amplitude image as here, but is not consistent and only works in cases where there are stark discontinuities in backscatter due to different ice types or a severely deformed landfast ice edge. The interferograms indicate in this case a similar landfast ice edge by a complete loss of coherence seaward of the discontinuity apparent in Figure 5b (Figure 5c).”

And later:

P7 L7: ”Although the landfast ice edge can in some instances be delineated using a single backscatter image, the delineation of bottomfast and stabilized ice cannot be discriminated as is apparent by comparing the delineations from Figure 5e with Figure 5b.”

2. Discussion on ice arch collapse embodies more literature than cited in the manuscript. The Barber et al. (2018) reference is good and very appropriate but investigations into this process have been ongoing for many years prior. Melling (2002) originally suggested it with respect to the Northwest Passage in the Canadian Arctic under a warming climate. This process was then quantified by Kwok (2006) and more recently by Howell et al. (2013) using Radarsat imagery, with the latter study showing evidence for multi-year ice increases in recent years. Haas and Howell (2015) further provided ice thickness measurements for a gradient from the Arctic Ocean to the southward shipping channels of the Northwest Passage. Overall, the relevance of the technique presented in the manuscript for predicting ice arch collapse from InSAR is important but some additional citing literature (as outlined above) providing a more comprehensive discussion is required.

Thank you for providing reference to great literature, which we acknowledge has contributed to strengthen the discussion in Section 4.2. All your suggested literature has been included.

Specific Comments

Page 3, Line 28 Specify the dates and beam mode for the S1 images. Not the entire list of images just more details on the beam modes used on the analysis.

We have restructured the section and state this more clearly now. We also now include a mentioning of the full list in the supplementary data:

P3L30: "All images used were captured in interferometric wideswath (IW) mode ($\sim$250 km swath width) during the months of March through May 2017 (see supplementary data for full list of images used)."

Page 4, Line 35 I would think (and argue) that landfast regions in the Canadian Archipelago are as just as important for shipping. They would also be useful a region to test the ice collapse prediction process similar to Nares Strait. See General

Comment 2.

We agree with this and rephrased the sentence, which we acknowledge could be read like Canada was less significant in this respect. It now reads:

P5L15: ”The Alaskan and Russian coastlines have high economic significance for the shipping and natural resource industries and feature diverse ice stability regimes and large areas of bottomfast ice are expected in these regions.”

Page 8, Line 12 and Lines 33-34 The process of ice arch breaking and thick multi-year ice from the Arctic Ocean being transported southward and into shipping lanes extends beyond the work cited in the C2 manuscript. See General Comment 2.

See comment to general comment 2.

Page 8, Lines 14-28 A recent study by Moore and McNeil (2018, GRL) documents the 2017 ice arch collapse in Nares Strait. It might be worthwhile verifying if the interferograms are in agreement with what is documented by Moore and McNeil.

Great suggestion. Yes, the findings seem to match quite well. A comment has also been added:

P9L15: “This failure event occurred relatively early as compared with past events (Kwok, 2005) partly in response to thinner ice conditions and northerly winds (Moore and McNeil, 2018).”

References

Haas, C., and S. E. L. Howell (2015), Ice thickness in the Northwest Passage, Geophys. Res. Lett., 42, 7673–7680, doi:10.1002/2015GL065704.

Howell, S. E. L., T. Wohlleben, M. Dabboor, C. Derksen, A. Komarov, and L. Pizzolato (2013), Recent changes in the exchange of sea ice between the Arctic Ocean and the Canadian Arctic Archipelago, J. Geophys. Res. Oceans, 118, 3595–3607, doi:10.1002/jgrc.20265.

Kwok, R. (2006), Exchange of sea ice between the Arctic Ocean and the Canadian Arctic Archipelago, Geophys. Res. Lett., 33, L16501, doi:10.1029/2006GL027094.

Melling, H. (2002), Sea ice of the northern Canadian Arctic Archipelago, J. Geophys. Res., 107(C11), 3181, doi:10.1029/2001JC001102

Moore, G.W.K and K. McNeil (2018), The early collapse of the 2017 Lincoln Sea ice arch in response to anomalous sea ice and wind forcing, Geophys. Res. Lett, https://doi.org/10.1029/2018GL078428

Reviewer 2

Dear reviewer, Thank you for conducting a very thorough review. This is much appreciated and have helped us to substantially improve this manuscript by following your suggestions. Please find detailed responses to each of your comments below. Thanks again! Best regards, Dyre Dammann

General comment This study analysed the potential of Sentinel-1 interferometry to distinguish three classes of landfast ice stability at the pan-Arctic scale. The method uses the fringe density pattern in wrapped interferograms. The delineation of the classes is performed then manually. The approach is clear and definitely shows a potential of the Sentinel-1 InSAR for the operational monitoring of the landfast ice at the pan-Arctic scale. However, in my opinion, the manual delineation of the classes is a serious weakness of this study. My feeling is that the manual separation of the fringe patterns was not performed accurately enough to go on with a quantitative and perhaps even qualitative analysis of the classes' area.

Please see the reply below regarding implications of the subjective nature of this analysis

By looking at the interferograms I often could not see a difference between fringe patterns in the areas, which you separated to different classes. Maybe in some areas this is due to a lack of details in Figure 4, and if this is the case, you should provide detailed examples. The following examples illustrate my concerns.

Thank you for pointing out these concerns. We have numbered these into 11 separate concerns (See numbers in Fig 1-3), which are here each addressed in detail:

1) These sections had been separated previously when considering a different delineation approach. Due to an oversight, this area was now still classified as stabilized by a mistake. This area have been marked as non-stabilized in the new version of the figure.

2) We initially tried to constrain ourselves to strictly the coastline, to make the computational effort manageable. However, your idea is good, to include this area due to the known shoals. We have now included this area and the findings are discussed below in response to your other comments regarding this area.

3) Thank you for pointing out this oversight. This is explained in Dammann et al. (2018c), but was not mentioned here. We therefore include in Section 4.1: "However, we greatly reduced such errors by not delineating areas that appears to be low-laying land and sediment bars in the SAR backscatter images. This can lead to the appearance of sporadic areas of bottomfast ice, separated by areas with a phase resembling that of bottomfast ice, but which in reality is land. In areas where the landmask does not appear to fit the coastline, delineating the intricate coastal morphology can be a time-consuming task, hence delineation on a pan-Arctic scale will inevitably contain inaccuracies." We have also included an explanation in the Figure 5 caption: "Due to minor inaccuracies in the landmask, the occurrence of bottomfast ice can appear sporadic."

We have further cropped out an example of the analysis in the area of your concerns (Fig 4) showing the interferogram overlaid the backscatter image with the areas classified as bottomfast ice (top), the classification (middle), and a zoomed-in area (black box in the middle figure) and the delineated bottomfast ice (bottom). In the bottom figure, it is clear that there exist minor areas that are not delineated. Outlining absolutely every little piece would not be practical at the scales addressed here, but is possible on the scales used in Dammann et al. (2018c). This is now addressed in Section 4.1.

4) Thank you for pointing this out. We agree that this area was not delineated properly according to our criteria outlined in Table 2. We have now delineated this area more carefully.

5) Please see point (1).

6) The bottom area is not delineated since there are no strong fringe gradients and there is a persistent fringe signature. Per our delineation criteria outlined in Table 1, this area has not been identified as stabilized. The contrast between low and high fringe density near the top of your circle is due to the image boundary between two interferograms acquired at different times. We realize that it is not always easy to discriminate between different interferograms. We have therefore now marked each boundary with a dashed white line to make this easier (see new version of the figure).

The easternmost area within the circle should be counted as stabilized ice and have has now been included. The westernmost area resembles bottomfast ice and is marked as a shallow area in your attached figure, but in fact appears to be largely comprised of land (see Fig 5) and is hence not marked as bottomfast.

7 and 8) This image was not properly geocoded for some reason resulting in a poor match between landmask and land in the image. This has now been adjusted and corrected for. Therefore, most of the areas identified as bottomfast ice is now apparent and not covered by the landmask. Even so, a couple of areas still exist that we identify as land in the backscatter image, but is not covered by the landmask. These areas feature zero phase change, but are still not identified as bottomfast ice. The most prominent example is the westernmost part of the northernmost bay.

Furthermore, the gaps you point out were filled out shoreward of the respective class, but we agree this is not really useful or correct. It is better to leave these areas out of the analysis, which has now been done.

I have the feeling that the delineation is too subjective and that the resulting classes could look very different if analysed by other operator. Therefore, the areas, provided in Table 3 can substantially differ depending on the delineation criteria.

It is true that different operators could delineate differently, which occurs on a daily basis with ice charting of other properties. However, the subjectivity does not take away the value of ice charts and we argue that is similarly the case for stability.

Also, a number of gaps are present in the data (Khatanga Gulf, Laptev Sea to the south from New Siberian Islands, eastern and central part of East Siberian Sea, Gulf of Ob), which makes the areal analysis incomplete. Are there Sentinel-1 data available? Could they also be included to the analysis?

Most of these are available, but as stated through the manuscript, we limited our analysis to 50 interferograms (now 52 with the inclusion of the Eastern Laptev Sea) and the general coastline. If we were to include the island groups and all the gulfs we would have to increase the analysis substantially, which is very time consuming. We have not attempted to make a complete pan-Arctic dataset, but aim here to show what is possible on a large scale. The only lack of accessible data we observed for the coastline was the Gulf of Ob. We have analyzed imagery in this Gulf in 2018 when imagery was available. However, we did not include this imagery to stick with a one-year analysis in this work.

Moreover, I think that bathymetry should be used together with interferograms to make an adequate delineation. IBCAO data can be used to identify potential bottomfast ice areas and also can clearly exclude areas, erroneously mapped as bottomfast ice if water depth exceeds a certain threshold (for instance, 2 m, see the figure below).

Thank you for this suggestion. Our concern in that the IBACO or any other bathymetry dataset is not known to be accurate in shallow areas. By including such dataset in the analysis would then at the same time introduce added uncertainties and sources for error. For instance, a large part of the red area you highlight is interpreted as above sea level in the backscatter image (see Fig 5). Dammann et al. (2018c) identified good correlation between phase and areas of known bottomfast ice. They used a high resolution local bathymetry dataset, but also there, the necessary interpolation lead to uncertainties, which would be much larger for a large-scale dataset such as IBACO. Furthermore, Dammann et al. (2018c) did not find reason to suspect substantial areas to be falsely identified as bottomfast ice simply because floating ice will inevitably exhibit some lateral motion during the 12-day temporal lag considered here.

As the Referee 1 suggests, I also think you could use the backscatter images to support the delineation of the bottomfast ice, and potentially other classes (or to show that the backscatter has no / limited use for this purpose).

Agree. This has now been included

Furthermore, a big improvement could be an unwrapping of interferograms w.r.t. the land area, which should be stable during the winter. By doing so, you could derive the magnitude of the ice movement, and then the ice stability classes could be distinguished based on the magnitude. In this case, the subjective delineation could be substituted by an automated classification. Moreover, the vertical and horizontal movements could potentially also be distinguished using ascending and descending orbits. This is probably beyond the study intention, but definitely should be discussed.

Thank you for these comments. This is all something we initially contemplated as well, but we found not to be an ideal strategy for the following reasons:

Phase unwrapping over sea ice can be tricky due to a large amount of discontinuities as discussed in Morris et al. (1999) and Dammann et al. (2016). It would therefore be difficult to make sure this can be done in a straight-forward manner for use in operational analysis and to provide consistent results everywhere. With that said, it could be done to some degree, but most likely without any improvement in the stability product.

The derived deformation (through phase unwrapping) will only signify deformation in cross track, hence the total deformation will strongly depend on the mode and direction of deformation. To resolve this one would have to use inverse modeling similar to the approach used by Dammann et al. (2018b) to determine the level of deformation. However, this approach would not easily lend itself to an analysis at this spatial scale. Even if one were to be able to back out the deformation, it would still be difficult to determine the stability of the ice cover since the deformation depends on forcing conditions between acquisitions. In other words, it would be difficult to determine whether low deformation is due to stable ice or low wind speed or other forcing conditions.

In the discussion section we now explain: "Without evaluating the phase response for each area of interest in detail during different forcing scenarios, it may be problematic to understand under what conditions the ice remains stable. Classification of stability based on relative differences in fringe density is also complicated by the use of non-simultaneous interferograms to provide complete coverage of a region. "With the approach we suggest here, we consider changes in fringe spacing within the same scene, hence a strong gradient will not be due to a change in forcing conditions, but rather a sign of different stability.

Vertical and horizontal motion cannot easily be discriminated using ascending and descending passes as discussed further in Dammann et al. (2016), since deformation changes too much between passes as opposed to when used for glaciers. However, by evaluating the fringe patterns, it is possible to identify vertical deformation as described in detail in Dammann et al. (2016).

You suggest that the used approach is rather simple and does not require SAR expertise. However, you do need to have some expertise to decide on the phase change gradient, and this seems to be a major problem at the moment.

This has now been moderated to: P10L18: "Hence, the approach can potentially be adapted by organizations without the need for trained SAR experts."

We agree some training will be needed, but we argue one doesn't need to be a SAR expert. The subjectivity is addressed above.

The unwrapping step is included into every standard interferometric workflow, and therefore, could be tried, at least exemplarily. This, perhaps, could also improve the identification of the bottomfast ice, as the InSAR phase in this case should be the same as over the land.

As discussed above, the phase unwrapping is not expected to add much value to the analysis we present in this work, but would add an added step and complication to the approach.

The authors provide a validation of their result for the Beaufort Sea, by comparing stability zones with long-term frequent position of the landfast ice edge from Mahoney et al. (2014). As stated in the Results chapter, they associate the second discontinuity in the InSAR phase with the nodes, identified in Mahoney et al. (2014). This discontinuity separates the stable/non-stable zones, whereas the nodes coincide with the edge of the landfast ice. Therefore, it appears to me that the comparison with the nodes from Mahoney et al. (2014) can only validate the overall extent of the fast ice, without being able to validate stable/non-stable classes identification. Perhaps, it can be partly attributed to the interannual variability of the landfast ice edge position (and position of the border between stable and non-stable fast ice). Nevertheless, I find such comparison important, and strongly suggest to add comparisons for other areas, for instance, for the Laptev Sea, where the positions of the fast ice edge are presented in Selyuzhenok et al. (2015). For that, the full extent of the fast ice in the Laptev Sea would be required, while at the moment, the important ice grounding zones are excluded from the analysis (see Figure in my general comment).

It would be great with more validation data, but we are here limited in terms of data availability of landfast sea ice stability.

Mahoney et al. (2014) find that the landfast ice edge in this section of the Beaufort Sea coincide with the 20 m isobath. Due to the dynamic ice conditions in this region, it is expected that the prevailing outermost margin will be supported by grounded ridges. The nodes will thus only occur in areas of frequently stabilized ice.

Comparing our analysis with Selyuzhenok et al. (2015) is a great idea. We have now added two more images in the Laptev Sea to make that possible. We have now added to Section 3.2:

P6L12: “Due to limited data availability in the central part of the Laptev Sea, the interferogram of the ice surrounding the Stolbovoy Island had to be acquired as early as February before the time of maximum ice stability (Figure 3). Even so, it is still clear that the area to the northwest of the island features stabilized ice (see “D” in Figure 4). This exact area features a shoal of < 10 m water depth leading to earlier formation of fast ice than the surrounding areas (Selyuzhenok et al., 2015) likely due to the formation of grounded ridges on the shoal resulting in increased stability. The ability of interferometry to signify the stabilized ice in this region lends support to our approach.”

Some concerns about a proper terminology: can you use simply “stable” and “non-stable” (or unstable”) ice classes, instead of stabilised / non-stabilised? Or do you want to emphasize that the stabilised class can be unstable before grounding?

Good question. The use of the terms stabilized and non-stabilized is essential. Whether the ice is stable depend on the individual ice users and scenarios as outlined in Dammann et al. (2018a). For instance, unstable ice for an industrial ice road, may not be viewed as unstable ice by coastal communities. Therefore, we choose to focus on relative local stabilization of the ice rather than trying to derive boundaries between stable and unstable ice as seen from the individual stakeholders perspectives. However, even if one wanted to use a threshold to determine stabile ice, it would be difficult to determine this based on the variable forcing scenarios which largely impacts the fringe density. For instance, it would be difficult to determine whether low fringe density is due to low forcing conditions or stable ice. This is why we chose to focus on local stabilization as we also discuss above in response to your earlier suggestion around phase unwrapping.

Also, the three stability classes are called “regimes”, “zones”, or “classes” throughout the paper. I suggest using one term consistently, and for me personally, the term “regime” does not seem to be correct, as you are focusing on a snapshot and not time series. Also, I’m not sure but I think the term “delineation” imply a linear feature, so when you refer to an areal object, maybe it is better to use “mapping”?

Great point. We have changed to mapping of zones throughout.

I also recommend the language proofreading to simplify and smooth the style and improve the clarity. Some sentences are very long, using redundant words, too many subordinate clauses. A tip from Elsevier, for instance: “Nowadays, the average length of sentences in scientific writing is about 12 to 17 words”. There also are a few typos.

We have conducted another proofreading to simplify and sorting out additional typos.

Specific comments

Thank you so much for providing such through review in terms of these specific comments. It is a great help!

p.2 lines 5-8: sentence is too long, and the second part of it is not clear (“defined here as the immobility” – defined where?)

This sentence has been split and simplified:

P2L5: “Similar to the drifting pack ice, landfast ice has declined significantly during the last few decades, in particular in terms of delayed freeze up (Mahoney et al., 2014;Selyuzhenok et al., 2015). Later freeze up critically impacts stakeholders through increased mobility (reduced stability) of the landfast ice in response to wind, ocean, or ice forcing (Dammann, 2017).”

p.2 lines 8-11: could you split the sentence in two?

Done

p.2 line 13: “from left to right” – better use “from coast towards open ocean” as you should not describe a figure in the main text.

Good suggestion. Changed.

p.2 line 14: can grow laterally

Included p.2 line 19: “high to moderately stable landfast ice...”

Good catch. Changed.

p.2 line 21-27: I suggest to add more structure to this part. Do you want to say that low-stability ice is simply dangerous for ocean-based operations? How are grounded ridges related to the low-stability ice? Do ice arches belong to the stable class?

Good point. This was not very clear. We have changed this to:

P2L21: “Low-stability ice is potentially relevant for ocean-based operations such as shipping trans-Arctic passages close to the coast where, patches of landfast ice occasionally break off and drift into nearby shipping lanes, potentially causing hazards. Even in areas hundreds of km from landfast ice can be impacted through the failure of ice arches. Ice arches consist of stationary ice forming between islands during freeze-up and when collapsing in the spring can send hazardous old ice into shipping routes (Bailey, 1957;Wilson et al., 2004;Barber et al., 2018). Stability is also of relevance for destinational cargo shipping in the Arctic as less stable, thinner ice is easier to break through resulting in opportunities for docking in areas of substantial landfast ice. For navigating through landfast ice, stabilization through ridging is also important to identify since ridges can be problematic to navigate and associated with hazards (Hui et al., 2017).”

p.2 line 28: No need to specify “sea”, just landfast ice

Taken out

p.2 line 29: I believe you do not need to mention in situ data, it is absolutely clear

Taken out

p.2 line 33-36: consider to compact the sentence

Done

p.2 line 37: what kind of types? Age?

Specified to multiyear and first-year.

p.2 line 38: because the movements are too small to use change detection?

Yes. Included:

P3L1: "However, backscatter does not give information pertaining to ice stability since the internal movement of the landfast ice is too small (mm/day) to be identified with change detection."

p.3 line 1: between two SAR images

Included

p.3 line 2: either specify that this is the case of the sea ice, or leave it more general for any kind of displacement/topography analysis

Took out sea ice.

p.3 line 11: "These studies have demonstrated..."

Done

p. 3 line 13: "as a mean..."

Done

p.3 line 15-17: consider to split and compact the sentence.

Changed to:

P3L17: “Hence, we explore InSAR as a tool to provide pan-Arctic information pertaining to stability relevant to subsea permafrost, biological habitats, and sea ice use.”

p.3 line 17: you do not need to determine the ability to create interferogram. It can be created for any interferometric pair. Reformulate or simply delete this.

Taken out

p.3 line 17-22: reformulate and simplify the sentences and do not refer to the Table and Sections, it is distracting.

Simplified to:

P3L18: “The goal of this work is to determine the Sentinel-1 interferometric data availability along substantial parts of the circumpolar coastlines and explore whether the different ice stability zones can consistently be analyzed and mapped in different geographic regions.”

p.3 line 27: in slant or ground range? This sentence should go after you specify that you use IW mode

Specified. Done

p.3, line 33: “a sample..”?âĂÍ

Supposed to be as is: ample

p.3 line 36-37: this is a repetition from the previous chapterâĂÍp. 3 line 37: “the InSAR phase may be related...”

Agree. Took out first sentence and moved the citation to past chapter.

p.4 line 3: can you specify the limits? I believe that the critical topography height can be calculated from the maximum baseline.

This has now been specified to 50 m.

p. 4 line 5: what means “only” here? You should also mention here atmospheric phase delay and other contributors to the InSAR phase.

This has been changed to:

P4L8: “Of the phase change attributed to motion, only displacement in line-of-sight direction. . .”

And we have included:

P4L4: “A phase signature can sometimes also be attributed to factors not related to surface motion or topography such as atmospheric phase delay and coregistration errors, but these effects are small compared to ice motion and can often be corrected.”

p. 4 line 7-8: “wrap around” – find more rigorous phrasing

Changed to:

P4L11: “. . .phase values to result in more than one fringe and ambiguous phase values.”

p. 4 line 8: “The results is...” – “The interferogram is...”

Changed

p. 4 line 12: again, any interferogram can be successfully created, please reformulate

Changed to:

P4L16: “The interferometric phase values will only be useful if scattering elements. . .

p. 4 line 13: ranging between 0 and 1?

Agree. Changed

p. 4 line 14: “A complete list..” – this sentence does not flow logically from the previous one. Either remove it or provide a logical connection.

Taken out

p. 4 line 19-21: After such a long technical introduction to the InSAR, the processing flow in two lines is way too short. Please provide more details.

This has now been included:

P4L24: “The images were first geometrically coregistered to ensure that the images cover exactly the same area with sub-pixel accuracy. The images were then mulit-looked 10 and 2 pixels in ground range and azimuth respectively resulting in reduced speckle and a final pixel spacing of roughly 23x28 m. Next, spectral filtering were performed to ensure both images comprise the same spectral range, which reduces phase noise in the final interferogram. The interferometric phase was calculated for each pixel in the geocoded and filtered images. Furthermore, the expected ramping phase in cross-track direction from a stationary flat earth surface was removed. The phase noise of the final interferogram was also reduced using an adaptive phase filter. All of these steps were completed for each interferogram using the GAMMA RS software (Werner et al., 2000).”

p. 4 line 23: in my view, “determining relative strain rates”, even roughly, is a little bit of exaggeration of what you really do. Just say that you use fringe density as an indicator of stability.

Done

p. 4 line 24-25: I don’t see how this sentence helps in understanding the following one. Consider removing.

We agree this sentence was unclear. It is now rewritten:

P4L33: “There are many factors that affect fringe density in addition to stability, including the atmospheric and ocean forcing conditions, satellite viewing geometry, and the prevalent mode of ice deformation (Dammann et al., 2016), making it problematic to evaluate absolute stability from fringe density alone. Instead, we focus on abrupt changes in fringe spacing within individual interferograms that allow us to identify variations within an area imaged under largely the same conditions, but where fringe gradients towards lower fringe density corresponding to increasing ice stability and possibly a different stability zone outlined in Table 2."

p. 4 line 25: "abrupt changes"?âĂÍ

Done

p. 4 line 26: "...to identify three stability zones: list them". Consider joining Tables 1 and 2.

Done:

P5L5: "The three zones (i.e. bottomfast ice, stabilized ice, non-stabilized ice) are subjectively. . ."

p. 4 line 33: in what study?

Changed to:

P5L11: "The approach we present here"

p. 5 line 2: was coherence low for all available pairs for these regions? I see that you mention that you do not "attempt to derive alternative interferograms in these cases" but this data gap can probably be easily filled, even in potentially operational mode.

Through all of our InSAR work utilizing different sensors and time frames over the years, this region does not feature good coherence, so utilizing different scenes would likely not result in improved coherence.

p. 5 line 7: I suggest not to start your Results with a description of results from the other study, even though relevant here. I suggest to move the chapter to the end of

Results or even to Discussion.

The first and second sections have now been switched

p. 5 line 10-11: consider simplify the sentence; you do not need here the technical explanation how these edges were derived.

This has been simplified

p. 5 line 14-15: do not describe the figure in the main text.

This has been taken out

p. 5 line 21: message in parentheses is confusing. I think you can see a pattern in these areas as well. Depends on what you call a pattern.

Taken out

p. 5 line 22: “This discontinuity has been shown...”. I do not think it can be called a gradient.

This has been changed

p. 5 line 24: see my general commentâĂÍ

Addressed in response to the general comment

p. 5 line 25: “reoccurring”âĂÍ

Changed

p. 5 line 27: what is meant by reduced phase response? Also, the sentence is too long.

Taken out, which also lead to shorter sentence.

p. 5 line 27-29: please reformulate and compact the sentence, it is hard to follow. What are the points of higher stability and how the points can be seaward?

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This has been clarified:

P7L3: “The discontinuity is not a straight line, but features multiple curves towards land ending in seaward points. At these points, the stability is higher than adjacent areas with the same distance from shore similar to the expected increased stability directly shoreward of grounded ridges.”

p. 5 line 32: Mapping pan-Arctic landfast ice stability zones?

Regimes has been changed to zones throughout the manuscript when referring to the three zones.

p. 5 line 34: please do not refer to the Sections

Taken out

p. 5 line 35: see my general comment. Without a convincing explanation how the bottomfast ice was distinguished from the non-bottomfast, I think that the current mapping is not accurate enough.

A short explanation has now been included:

P5L1: “The often-strong fringe gradient leading to an area of near-zero phase change has been shown to represent the boundary of where the ice is frozen to the sea floor and can subsequently be used to map bottomfast ice (Dammann et al., 2018c).”

We are also stating in Section 3.2:

P6L35 “One discontinuity separates the area of near-zero phase change from an area with relatively low fringe density. This discontinuity indicates the boundary between bottomfast and floating ice as two of these interferograms were validated both on Elson Lagoon near UtqiaÄąvik and the Colville Delta (Dammann et al., 2018c)”

p. 5 line 36, please do not refer to the SectionsâĂÍ

OK. Taken out throughout the manuscript

p. 6 line 1-2: what is meant by substantial bottomfast ice?âĂÍ

specified to:

P5L29: "...with extensive bottomfast ice reaching several km from shore are..."

p. 6 line 3: what means Accordingly here?âĂÍ

Doesn't belong. Taken out

p. 6 line 7-8 please do not refer to the figures and Sections here, it should be clear without that.

Agree. Done

p. 6 line 12: revise the sentence, something is missingâĂÍ

This has been changed to:

P6L1: " Conversely, the greatest area of stabilized landfast ice was found in the adjacent Beaufort Sea, with almost equal extent of non-stabilized and stabilized landfast ice."

p. 6 line 14: "...landfast ice..."

Done

p. 6 line 15-18: please shorten and clarify the sentence

Done

p. 6 line 21-22: identified where?

Added:

P6L5: "along the Russian Arctic coast"

p. 6 line 22-23: as in my general comment examples, I do not see how this area is different from, for instance, the area around Bely Island (right above the Yamal peninsula).

Yes, some of the areas by Yamal should be categorized as stabilized. This has now been changed

p. 6 line 24-25: I think in this case you simply should not mention it here at all.

Agree. Taken out

p. 6 line 27: "...limitations for mapping stability classes"?

Agree. That is better. Changed.

p. 6 line 29: typically or in your study?

Changed to:

P7L13: "...have been identified"

p. 6, line 36-p.7 line 5: this repeats the paragraph in the Result section. I suggest to remove it from the Results as it is a discussion point with speculations.

Good catch. This has been taken out and merged with the text in the discussion

p. 7 line 14: how the definition of the "highly stable" from Eicken et al. (2005) is related to the definition of stable in your study? Can they be compared?

This reference is to the entire region, so it wouldn't be possible to do a direct comparison p. 7 line 15: or this part is also prone to the tidal movements?

Yes, it is possible, but if the fringes were solely a sign of changed vertical motion, it would mean that the ice was elevated several meters in certain regions, which is implausible. We now include this:

P8L9: "Based on the overall fringe counts and patterns, only part of the phase response is likely attributed to tidal motion"

p. 7 line 17: SAR backscatter analysis?

Changed

p. 7 line 18-19: but the mobile ice would be incoherent in your results? Also “one month after the initial freeze up” – while you consider late winter situation. As recommended in the general comment, please include more scenes for the Laptev Sea to have an entire fast ice extent – maybe it can shed more light on the situation here.

We are here merely speaking to the stability of this region as a whole and that it may not be as stable as previously thought. The time frame is indeed different. More scenes are now included and are discussed above.

p. 7 line 27: why to mention it here if it is discussed below?

Taken out

p. 7 line 28-30: or it can aid in the interpretation, e.g. by confirming the pattern, or by featuring the temporal changes between interferograms. Also, I do not see what is meant to be in the northernmost region of the Lena Delta in Figure 4.

Changed to:

P8L18: “Hence, the use of interferograms based on different dates can aid interpretation by confirming consistent fringe patterns and identify temporal changes. However, the temporal change will also result in a phase gradient at the image stitching not related to different stability zones, which may further complicate the mapping process.” Took out reference to Lena Delta since not strictly necessary

p. 7 line 33: is this connected to the previous sentence? If not please restructure

Not related. Swapped the following sentences to make it more clear.

p. 7 line 35: so they are not available in late winter 2017 or may not be available?

May not be available. This has been further specified:

P8L29: “Furthermore, IW imagery are predominately acquired over land, hence it is

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likely not possible to construct interferograms away from the coast to cover extensive landfast ice approaching the 250 km IW swath such as that in the East Siberian Sea."

p. 7 line 36: as you say "consistent coherence loss", now I probably got an answer to my question to p. 5 line 2. Can it simply mean that there is no landfast ice?

In reference to your earlier question, there may be some areas in the Kotzebue region where the dynamics is large enough to the point where the ice may not be called landfast, such as in the Kotzebue Sound. However, the coherence loss is more widespread covering inlets that are known to be stable.

p. 7 line 36-37: temporal progression or spatial? Not clear what is meant here.

Spatial. This is now included

p. 7 line 38: why would we be interested in using this analysis for the melt onset period, when the ice starts to degrade and be unstable anyway? Please develop you thought here.

Included:

P8L27: "It is worth mentioning that this technique can only be used before the onset of melt when widespread coherence loss occurs, hence it is not possible to evaluate the retreat of bottomfast ice or reduction of ice stability in response to melt"

p. 8 line 13: "...multiyear ice..."âĂÍ

included

p. 8 line 14: In our study?âĂÍ

Not limited to our study, so suggest to keep it as is

p. 8 line 15: Sentinel-1 backscatter imagery? Did it capture the formation?

Included. Took out "formation"

p. 8 line 16-17: this goes to the figure captionâĂÍ

Done

p. 8 line 36: potential applications?âĂÍ

Agree. Changed

p. 9 line 23-26: please split the sentence

Done

p. 9 line 27: “multiple”→three?âĂÍ

Changed

p. 9 line 28-31: aren’t these classes used for all seas? Why to specify Beaufort Sea here?

They are, but we are arguing that the classes fit good in the Beaufort, but that the Russian sector potentially warrant additional classes

p. 10 line 3-8: this is too detailed for the Conclusions, and mainly repeats the paragraph from the Results. Please generalise your findings in Conclusions.

Taken out

p. 10 line 19: did you actually consider year-to-year timescale?

Good point. Taken out

p. 14 line 4: remove the word “ice” before “sea ice”

Done

p. 15 Figure 2 caption: Conceptual scheme?

Included

p. 16 Figure 3: place the red ovals a,b,c on top of the b) panel; caption: explain a,b,c nodes in a).\

Done

p. 17 Figure 4. I suggest to redistribute the panels in such a way that all of them can be enlarged.

The images are currently distributed so they can take up a page in landscape mode. Unless you suggest images on separate pages in separate figures, we think this will work quite well.

p. 18, Figure 5: enlarge, the same as figure 4. The river you refer as to Angara, is Yenisei! Please check other geographical names and add river shapes on the maps.

Same as above. Good catch. Checked. Names of rivers are added for rough geographical context in relation to the rivers. We feel that adding river shapes of all the different channels etc. may be distracting and not strictly relevant.

p. 19, Figure 6: please add the overview figure showing the location of the arch. Use additional graphic to delineate the arch and the flow direction. Some readers are unfamiliar with this sea ice feature.

Good point. This has been included

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2018-129/tc-2018-129-AC1-supplement.pdf

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Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-129, 2018.

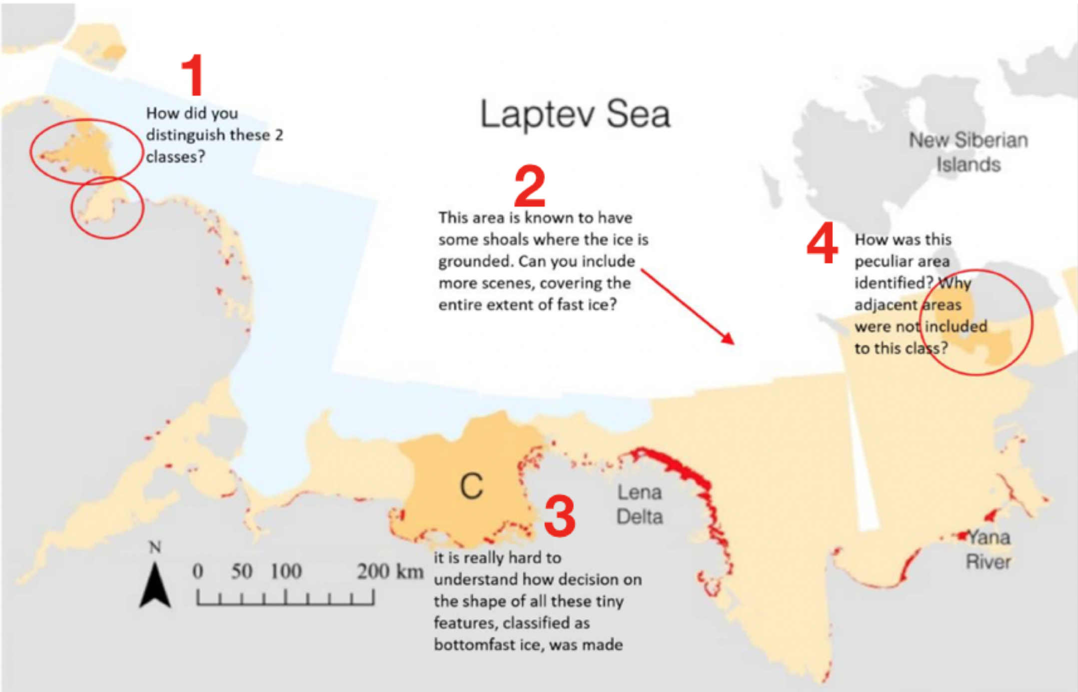
Laptev Sea
1
How did you distinguish these 2 classes?
2
This area is known to have some shoals where the ice is grounded. Can you include more scenes, covering the entire extent of fast ice?
New Siberian Islands
4
How was this peculiar area identified? Why adjacent areas were not included to this class?
C
3
Lena Delta
Yana River
N
0 50 100 200 km
it is really hard to understand how decision on the shape of all these tiny features, classified as bottomfast ice, was made

Fig. 1.

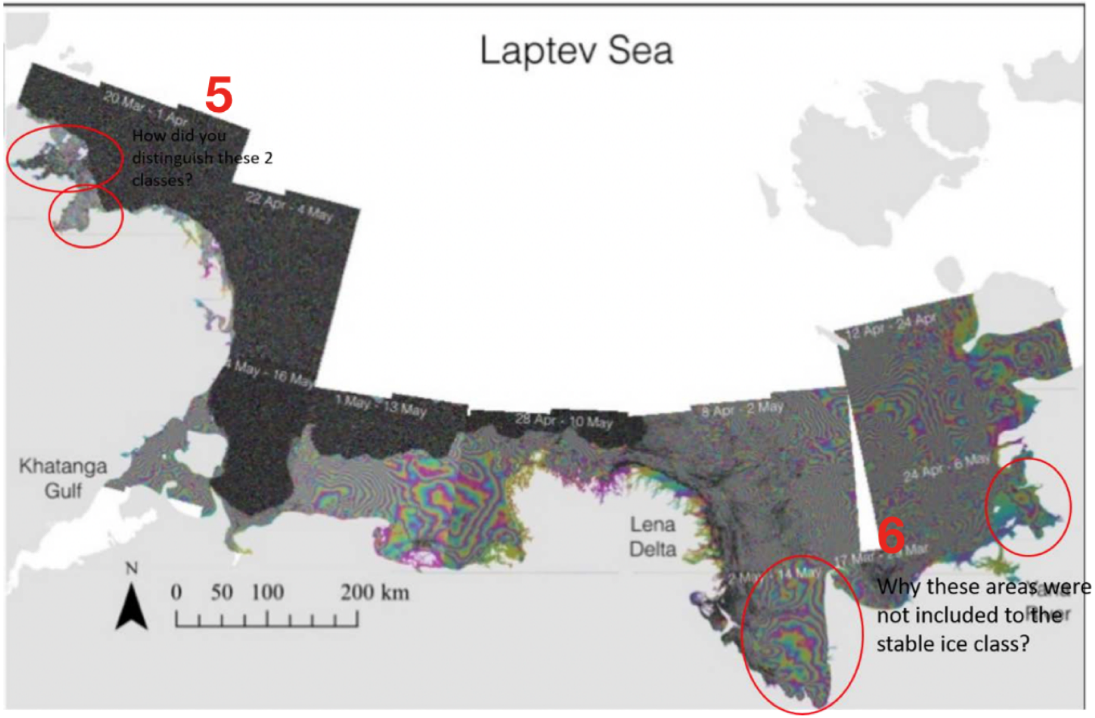

Fig. 2.

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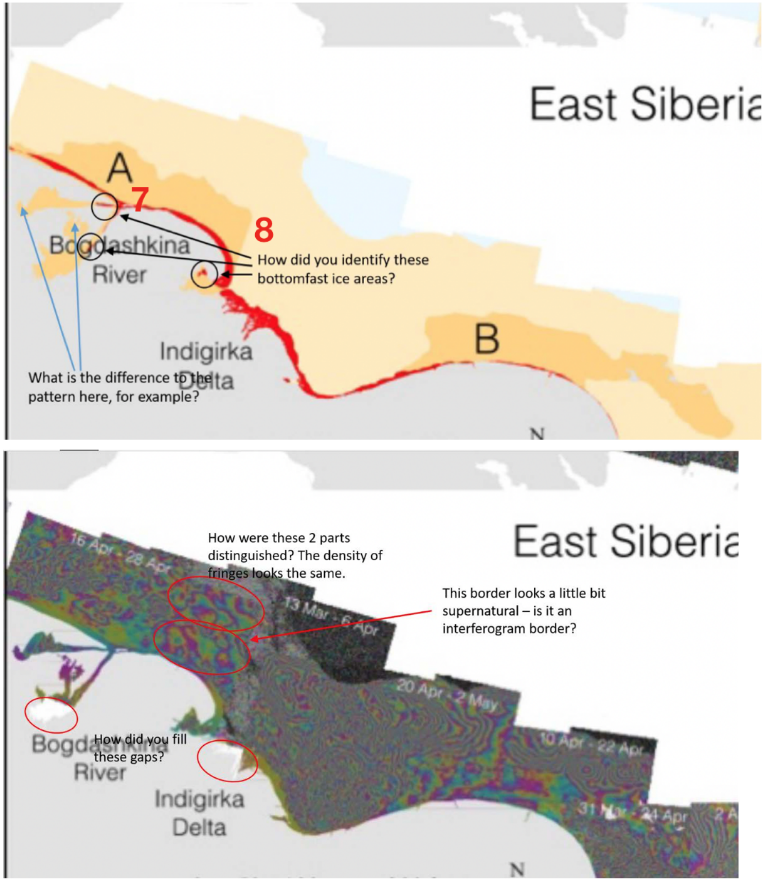

Fig. 3.

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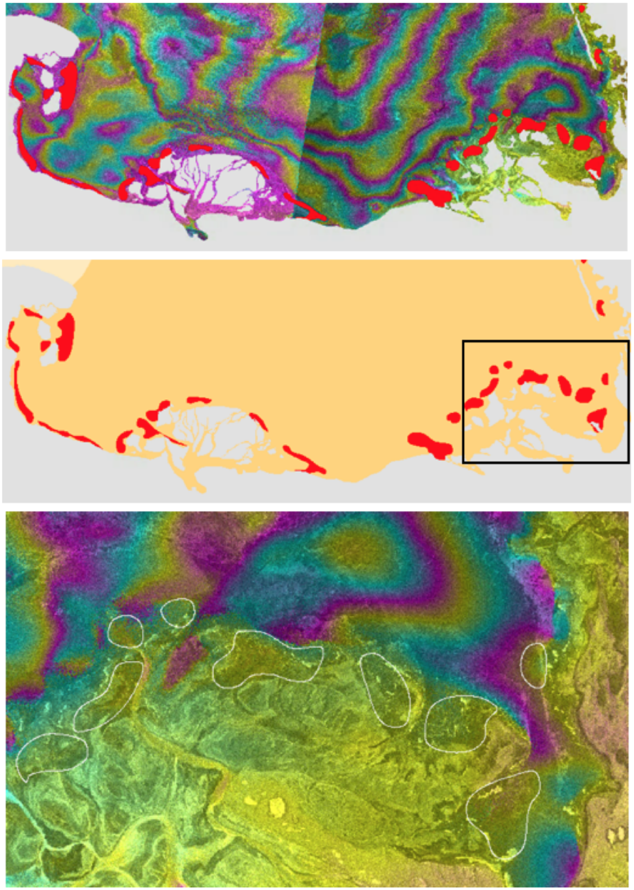

Fig. 4.

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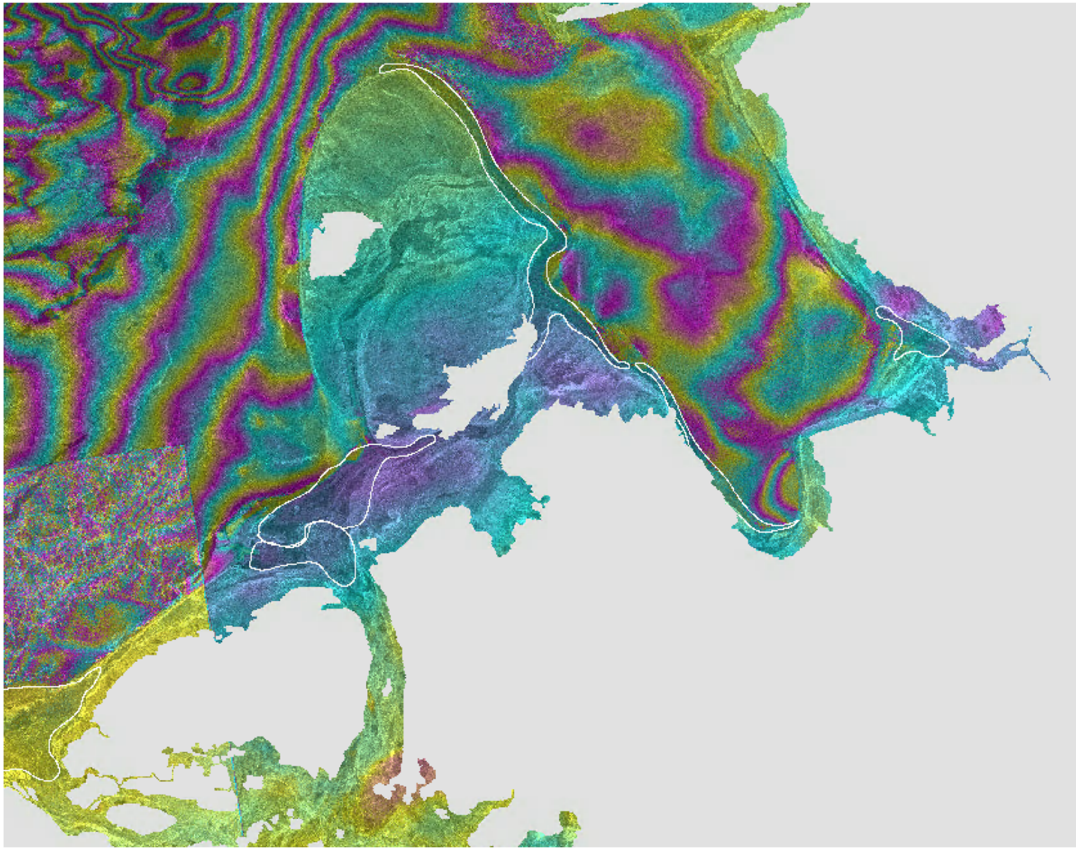

Fig. 5.

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