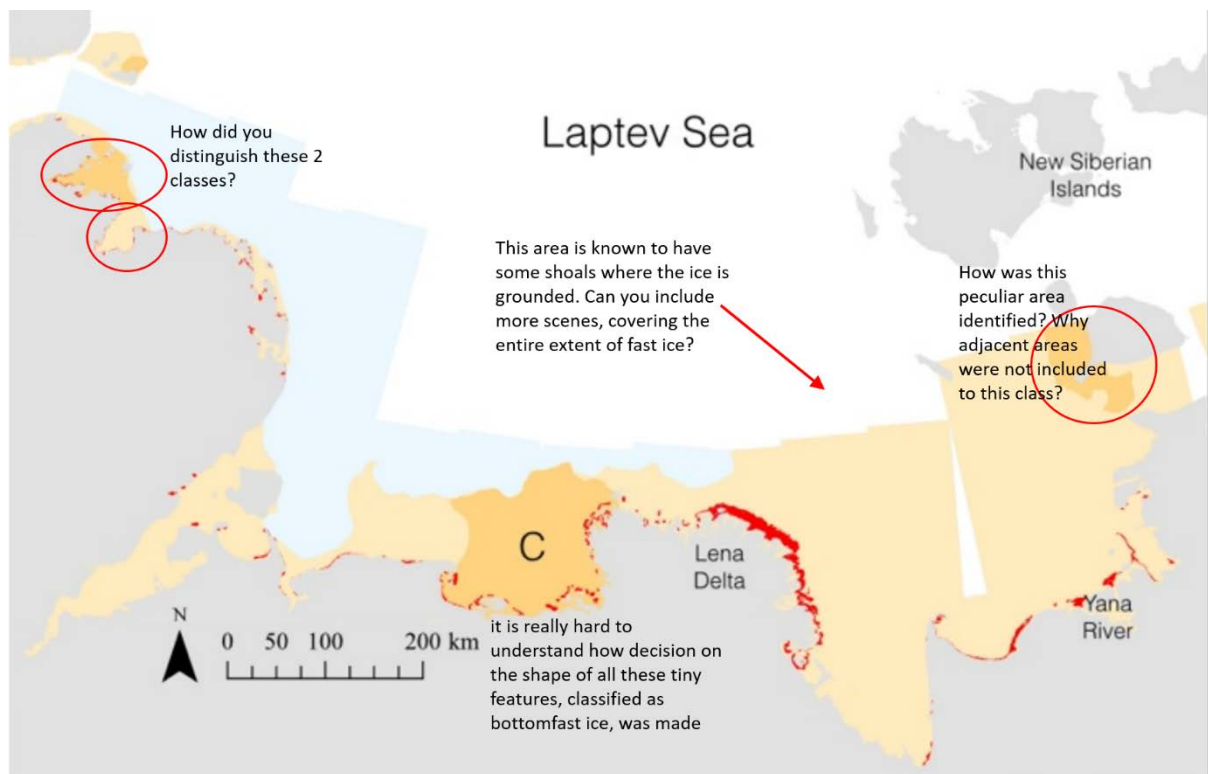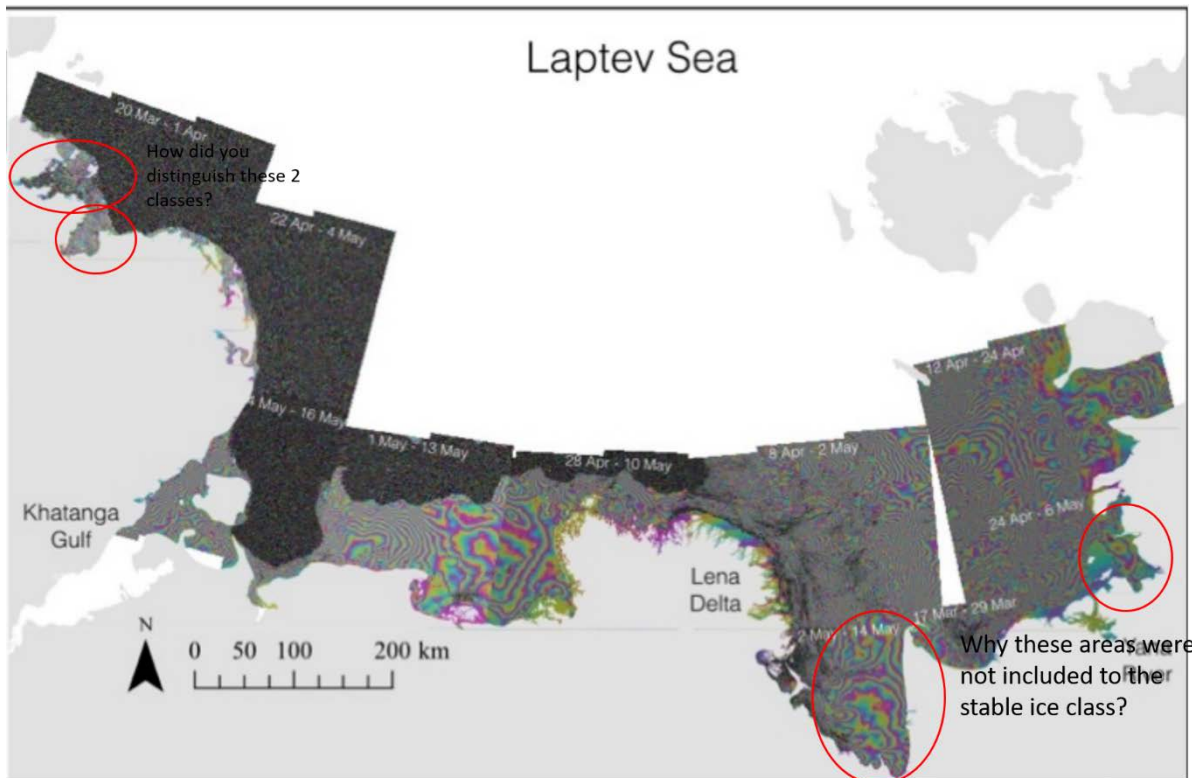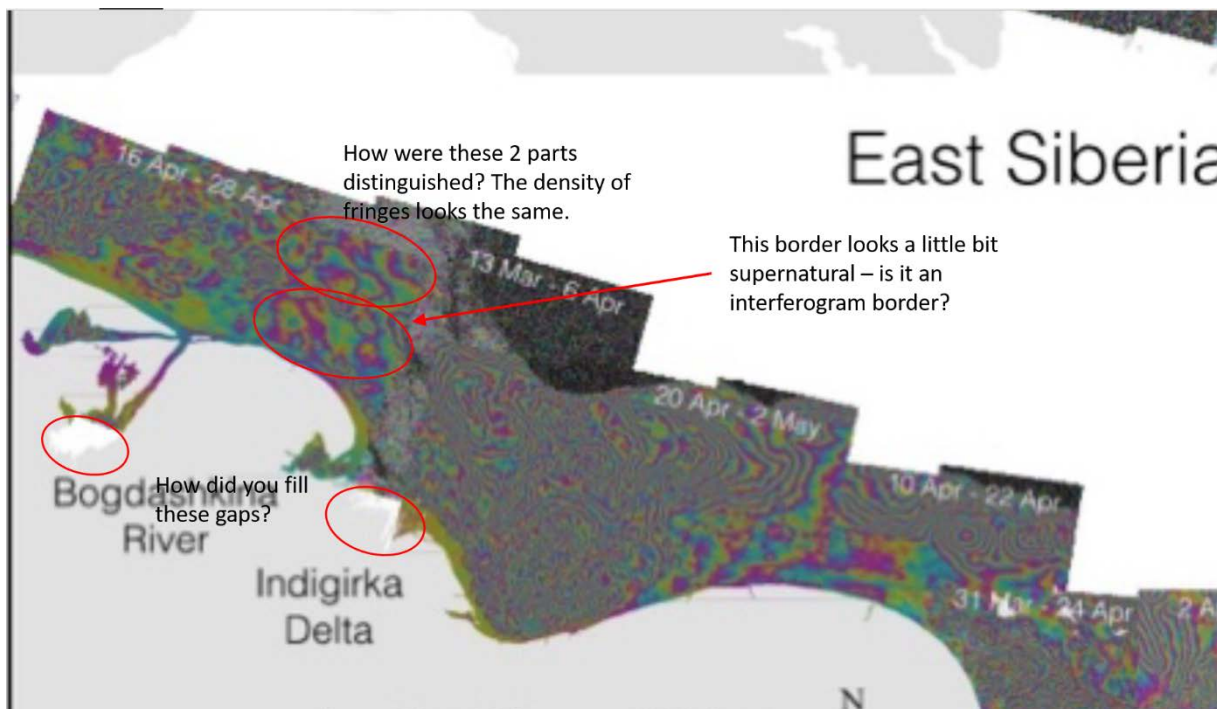# General comment

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. 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.

Laptev Sea

How did you distinguish these 2 classes?

Why these areas were not included to the stable ice class?

East Siberia

How did you identify these bottomfast ice areas?

What is the difference to the pattern here, for example?

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. 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? 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).

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).

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. 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. 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.

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).

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? 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"?

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.

Specific comments

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?)

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

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.

p.2 line 14: can grow laterally

p.2 line 19: "high to moderately stable landfast ice…"

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?

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

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

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

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

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

p.3 line 1: between two SAR images

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

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

p. 3 line 13: "as a mean…"

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

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.

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

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

p.3, line 33: "a sample.."?

p.3 line 36-37: this is a repetition from the previous chapter

p. 3 line 37: "the InSAR phase may be related…"

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

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

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

p. 4 line 8: "The results is…" – "The interferogram is…"

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

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

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.

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.

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.

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

p. 4 line 25: "abrupt changes"?

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

p. 4 line 33: in what study?

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.

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.

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

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

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.

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

p. 5 line 24: see my general comment

p. 5 line 25: "reoccurring"

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

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?

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

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

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.

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

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

p. 6 line 3: what means Accordingly here?

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

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

p. 6 line 14: "…landfast ice…"

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

p. 6 line 21-22: identified where?

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).

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

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

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

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.

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?

p. 7 line 15: or this part is also prone to the tidal movements?

p. 7 line 17: SAR backscatter analysis?

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.

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

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.

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

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

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?

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

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.

p. 8 line 13: "…multiyear ice…"

p. 8 line 14: In our study?

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

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

p. 8 line 36: potential applications?

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

p. 9 line 27: "multiple" → three?

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

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.

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

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

p. 15 Figure 2 caption: Conceptual scheme?

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).

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

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.

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.