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Interactive comment on "Estimation of Arctic Land-Fast Ice Cover based on SENTINEL-1 SAR Imagery" *by* Juha Karvonen

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General comments: This manuscript is a nicely self-contained "techniques" style paper describing a new method for generating maps of landfast sea ice from SENTINEL-1 EW imagery. It provides an effective overview of the significance of fast ice and the field of/recent progress in fast ice detection from satellite sensors. The author's use of English is not perfect, but the meaning was perfectly clear in almost all cases. Unfortunately the large number of formatting errors (erroneous spaces and parens in references; missing references; missing figure numbers; typographical error in the abstract) detracted a little from the presentation, but these can be fixed easily.

Thank You for Your comments. I have tried to improve the language, improve the

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formatting and checked the references and figure numbers. This version has done through a lot of changes from the previous version. If seen necessary I can still try to improve the English language and also if seen neccessary find asuitable perosn to make an English language proofreading.

Specific comments:

1) This paper is possibly of too limited scope to be considered to be considered for publication in The Cryosphere. As it stands, it is a nice “techniques” style paper, but there is little in the way of scientific results (a very short time-series of fast ice extent; and a snapshot of fast ice retrievals from AARI charts vs the two algorithms presented here, neither of which are explored in any detail). I suggest that the Editor consider whether such a techniques-focused paper is suitable for publication here. Alternatively, the author may significantly increase the amount of analysis of the data here, or expand its scope (spatially).

The S-1 dual-polarized data covers only the European Arctic and in other areas the S-1 data are in single-polarization (HH) mode. In principle the spatial scope could be increased to cover e.g. Greenland waters. However, we have collected the data and generate mosaics only over the area used in this study. Collecting the data even over all the European Arctic (from ESA SENTINEL SciHUB) would be an enormous job and the amount of data too much for DMI data handling and storage capasity. For this reason the study area has been restricted as it is. For climate studies the time series is still short, and the major purpose of this manuscript has been to demonstrate the capability of the proposed methodology method and S-1 data for LFI monitoring and also to start a LFI time series over the study area. For a SAR related manuscript the amount of data used is exceptionally large.

There have been many papers in TC with a significantly smaller amount of data and significantly shorter time period. One example of such paper is: The Cryosphere, 12, 343-364, 2018 https://doi.org/10.5194/tc-12-343-2018

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The major target of this manuscipt indeed is to represent the algorithms and demonstrate their capability. But also some analysis for the study area and the study period have also been included.

In addition it should be noted that the amount of data used for this study is exceptionally large, several thousands of SAR images were used to generate the mosaics even though the time period is less than two years. Actually many SAR-related papers only study a few images.

2) The paper claims that “we have used quite similar criteria” to Mahoney et al., 2005 for LFI. Notwithstanding the use of “we” for a solo author, this claim is hard to support. You say that you use similar criteria to Mahoney et al’s “contiguity” and “20 day” criteria. Neither appears to be true: a. Nothing in your methods description suggests that you enforce contiguity in any way; and b. Your FMI-A and FMI-B algorithms use a two day and 14 day time-scale for fast ice classification, respectively. FMI-B could easily have been changed to a 20-day criterion if you really wanted to be similar to the Mahoney work. I’m not suggesting that your failure to have similar criteria to Mahoney et al is a bad thing – in fact, at least in the Antarctic, contiguity with the coast is certainly not necessary for fast ice formation, and the 20 day figure suggested by Mahoney is certainly up for discussion. I only take issue in the fact that you suggest you are enforcing similar criteria whereas you certainly aren’t.

I included the adjacency to land condition in the algorithms and the results have been reproduced. I still use the 14 day period instead of 20 days as the results are quite similar (based on our experiments with the Baltic Sea LFI) but with a shorter time period the temporal resolution is better. "quite similar" has been removed.

3) Section 3, the methodology, is lacking in clarity. a. P3,L26: I don’t believe the mask is 100 km from the nearest coast, as you say. E.g., see Fig 5 – a polygonal shale is clearly seen around small islands, probably the consequence of mathematical morphology operators for a certain number of pixels – and not a 100 km distance from

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nearest coast, as stated. b. As you state, an ice mask based on bathymetry would be so much better, and almost as easy to implement. Are there any regions of fast ice that would have been identified more than 100 km offshore, which you are ignoring here because of this? c. P3,L30 to P4,L1: Aren't you using daily mosaics, meaning that a cross-correlation of r=1 between daily mosaics would be impossible? I suspect your description of mosaic construction is lacking. How many days can scenes get "reused" for? Doesn't this temporally smear your result, especially in the case of FMI-A? d. There is no mention of how the TCC thresholds were decided upon. What's the sensitivity of the choice? e. Similarly, there's no description of the choice of 14 days for the FMI-B algorithm.

The mask has been generated iteratively from the GSHHG coastline, so each pixel adjacen to coastline are first given a distance from the coastline (either 1 ot sqrt(2) depending on whether they are horizontal/vertical or diagonal neighbors. Then this in applied incrementally to the neighboring pixels of the pixels with a distance from the coastline assigned until diustances from coastline for the whole area have been assigned. This description has been included. Even though the distances are not exact, the mask is useful. The aim of the mask is to fasten the computation, actually it does not have any other effect on the results, only the faster computation. The faster execution is important in getting time series computed in a reasonable time (even though for a single day the computation typically only takes a few minutes).

More detailed description of the mosaic construction has been added.

The TCC thresholds in the previous version were based just on experiments and visual interpretation of two (February and April) mosaic images. In the updated version the TCC thresholds were based on comparing the thresholding results to 5 monthly (Jan-May 2016) AARI ice charts and defining the optimal (corresponding to minimal classification error) thresholds by varying the threshold and computing the classification errors. This information with related figures have been included.

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4) P1,L17: You need to explicitly mention which region these statements are true for.

I mean Arctic in general, "Arctic" added in the text.

5) P2,L21: Conspicuous absence of the Meyer et al reference here - doi:10.1016/j.rse.2011.06.006

Reference added. Thank you!

6) P2,L28-31: Unnecessary detail for a manuscript – consider removing

I think the exact information of the test area location is important, for example if someone would be interested to compare his/her results to the results in this manuscript. So I did not remove this detail.

7) P5,L13: “rather good” is not quantitative enough – stemming from the rather qualitative comparison between datasets.

The comarison results with the updated parametrization have changed and Table 1 has been simplified and made more clear.

"rather good" replaced by "at an acceptable level".

8) P6,:15: This temporal average and temporal median is introduced here (discussion and conclusion) for the first time, but I have no idea what it’s referring to. This whole section (on processing time) seems excessively lengthy.

Changed to temporal cross-correlation average andd median. Temporal average and temporal cross-correlation median refer to averaging or taking median of cross-correlation values at the same grid point in multiple mosaic images representing different days. I hope this has been expressed more clearly in the revised manuscript.

9) P5,L21-25: You state that HH alone would be sufficient for the techniques here, but there is no evidence to back this statement up.

It has not been studied comprehensively yet as in this area we have HH/HV data.

Some tests with Baltic Sea ice have given promising results for HH alone. I have tried to reprase this sentence. Unfortunately no real numerical comparison to support this assumption yet and not much resources for the work. Only some visual comparisosn have been made and according to them the most significan effect of using HH alone was a slight increase in false detections (compared to AARI ice chart LFI).

10) P5,L26-31: A repeat of earlier in the paper.

I think it is also an important conclusion that over whole Arctic and/or Antarctic only HH mode SAR data are available from SENTINEL-1 and any operational pan-Arctic/Antarctic operational system should be based on HH alone.

11) You say how quick this algorithm runs. Why do you need to use a LFI search area mask then? Why not run it everywhere in the ocean?

It is true that the algorithm is fast for one day case. However when running it for a time series I wanted to fasten the processing by applying the mask. Actually, using the mask does not have effect on the result, it only fastens the processing. In computing longer time series the time saving is significant, for example the time to compute the results for the whole study period can be reduced by 12 hours. Also in possible future operational production fast delivery is a useful property.

12) I'm left with a lingering desire for a map of average fast ice coverage, even though your time series is very short. This would be a nice result for this paper, and allow comparison to the earlier Divine work. Somethin akin to the Fraser et al., 2012 figure 2 (https://doi.org/10.1175/JCLI-D- 10-05032.1) would be ideal.

Added/updated figures: LFI entent time series figure updated by adding three different areas of Kara Sea (as in Divine 2004), and a figure describing the relative time fraction of existence of fast ice at the grid points.

Table 1: I have no idea what the column headings represent. (A(IC), etc.) This whole table is very poorly described. Figure 1: This map is poorly presented, and appears to

be of low resolution. Borderline illegible.

Table 1 has been updated and simplified, I hope it is more clear and understandable now.

Figure 2: Completely illegible.

Figure 2 has been removed.

Figure 3: The choice of a black mask is not appropriate given the high amount of black in the right sub-figure. The figure needs to be made bigger. An overlay needs to be included (lat/lon, coastlines, etc.). Coordinates and north arrow have been included.

Land mask color has been changed. The coordinates and north arrow have been added.

Figure 4: Caption doesn't indicate that this is for FMI-B. Iquestion the necessity of this figure too.

The figure is actually for FMI-A, FMI-B is derived from multiple FMI-A results by the logical AND operation. FMI-A has been included in the caption.

Figure 6: The caption should refer to FMIA and FMI-B. Again, overlay needed (lat/lon/colour legend for upper left). This figure seems redundant with Fig 7 also included. Figure 7: Need a legend describing all 8 potential colours used in this figure. Also overlay, etc.

The figures have been replaced in the updated version.

Figure 8: No comment on why the AARI charts underestimate fast ice compared to your work.

It depends on the selection of parameters, in the new version the algorithm parameters have been selected such that the detected FMI-A LFI area is close to the AARI LFI area. For the new parameters this comment is not relevant.

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Figure 8: Doesn't the value of 0 kmËĘ2 fast ice at day 180 in the FMI-B algorithm indicate that your coast mask is just fine? This is contrary to your statement in the text that some land pixels are not correctly masked out.

There have been some kind of error in the previous version FMI-B time series at that point, it should not go to zero. And either in the results with the new parameters of the revised manuscript it does not go to zero in the summer. The summer ice areas have now been masked off (based on FMI-A August 2016) and after this correction both LFI-A and LFI-B show zero LFI extent during the summer.

Thank You once again for Your comments! I hope the revised version has improved compared to the first version and we are iterating towards the correct direction!

Juha Karvonen, FMI

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2017-260/tc-2017-260-AC1-supplement.pdf

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Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2017-260, 2017.