

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:

The manuscript presents a new method for the landfast ice mapping based on SENTINEL-1 SAR imagery. The method is tested in the Kara and Barents Sea area and the resultant landfast ice (LFI) product compared to operational sea ice charts from Arctic and Antarctic research Institute, Russia (AARI charts). The results indicate the potential to derive reliable fast ice extent operationally. Unfortunately missing methodological details, inconsistent results and the large number of typographical and formatting errors do not make an impression of a self-contained manuscript.

Specific comments:

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1. Missing discussion of other relevant studies

The introduction give an overview on of existing methods of fast ice detection, including several SAR-based methods. However, it is not clear what is the potential advantage of the proposed algorithm compared to the existing ones.

p1, line 21-22: The author states: “in the fast ice zone only thermodynamic ice modeling is necessary as the modeled dynamic part can be omitted”. Fast ice can be formed dynamically, it also can breakup in response to the dynamical forcing. Please, clarify your statement. It would be good to provide some references to model studies to support your statements on p 1, line 21-23.

2. Missing clarity in the methodological sections

In general, the description of the work-flow is confusing. It needs to be clarified in order to be reproducible:

Were raster (gif, png) or vector (shp) AARI charts used? In general, the vector format is more convenient to work with. Fig 2. and Fig 6. (upper left) suggest that the raster format was used. What kind of software was used to re-project the rasters and extract fast ice extent?

I encountered that different product might give quite variable fast ice extent during its development in fall. It would be interesting to see whether the FMI methods show result in similar to the AARI fast ice extent in October-November. The Sentinel-1-based time series of LFI extent were derived from October 2015, however the comparison with the AARI charts covers a period from November on. What is the reason for shortening the comparison period?

p 4, line 16-17: The author should clarify what is “the daily LFI product”. Is it a product of FMI-A method? “Daily mosaics” seems to contain SAR data collected for a period of several days. The consecutive mosaic might have several days apart. In this case, the LFI product contains information from different dates.

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The construction of “daily mosaics” is described in 2.3 (p 3, lines 12-14). Later it becomes clear (p 3, lines 30-31) that some parts of a daily mosaic might remain from a previous day. The author should explain better how and for what time period the mosaics are constructed.

The use of 0.19 and 0.15 TCC threshold values are not explained. In general, the values seem to be rather low. According to Fig. 6, the threshold values work well for discriminating between fast ice and open water or newly-formed ice, but appear to work worse when fast ice is surrounded by the first-year ice (area south-east of the Severnaya Zemlya Archipelago). Were the thresholds picked based on a sensitivity study?

p 4, line 16-17: The author should clarify what is “the daily LFI product”. Is it a product of FMI-A method? “Daily mosaics” seems to contain SAR data collected for a period of several days. The consecutive mosaic might have several days apart. In this case, the LFI product contains information from different dates.

p 5, line 15. The temporal average and temporal median are mentioned for the first time. Please, provide more information in the methodology and results sections.

p 5, lines 26-31: Application of an extended land mask would help to properly compare your results with AARI charts. First, it would exclude summer fast ice. Second, it will reduce the difference between AARI and FMI fast ice extent (in Fig. 8) and therefore add more value to the number describing the remaining differences. I suggest that the extended land mask should be applied at least for the data comparison.

3. Inconsistency of the results

p 4, lines 23-24: The description method performance does not agree with figures and table. The qualitative description “quite good” is not in line with the quantitative characteristics presented in Table 1. According to the Fig. 8, FMI methods slightly overestimate fast ice area compared to the AARI charts. If I understand the numbers

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in Table 1 correctly, at least half (92.+-42.3%) of fast ice detected by FMI-A method is not actually fast ice on the AARI charts. A large area of spurious fast ice is located far from the coast between the Severnaya Zemlya and the mainland (Fig. 6, 7). Its presence can not be explained by different land mask, as suggested on p 4, line 27-28. The author claims that his methods are more precise than the AARI charts. Currently, operational charts is the most consistent and reliable source of information on fast ice extent. A cross-comparison of two data sets does not reveal a more precise method, but rather gives information about relative performance of the two. Fig. 6, 7 show that some fast ice areas (FMI-A and FMI-B) are detached from the coast, which suggests that automated methods are less precise, than the AARI charts. As the author says, different fast ice definition may indeed explain mismatch between data sets. The author however should explain what are the differences in fast ice definitions and how they may affect fast ice detection process. The given definition: “our automated algorithms locate the ice areas which has been static over a given time” is misleading. The presented method is based on reveling areas of low changes in surface backscatter characteristic, which does not ultimately mean that the ice (or any other surface) was motionless. It is also not clear what is the “given time”.

LFI area from FMI-B methods differs on Fig. 8 and Fig. 9. On Fig. 8 the maximal FMI-B LFI extent is reached between Julian days 100 and 120 (April-May); it is roughly 35 000 km². The LFI extent for the same time period on Fig. 9 (170 - 200 days from Oct 15, 2015) exceeds 40 000 km². Which of the figures is correct?

4. Questionable conclusions

p 5, lines 13-14: The author states that the method is suitable for operational LFI monitoring. Taking into account presence of large areas of spurious fast ice (Fig. 6, 7), inconsistent fast ice extent presented in Fig. 8, 9 and results from pixel-wise comparison with AARI charts (Table 1), I question that at this stage the methods can be use for reliable fast ice detection.

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5. Figures and table require a better explanation

The technical information shown in Fig. 1, 2, 5, 6, 7 can be presented more efficiently. E. g. the the study area (Fig.1) and the land mask and LFI mask (Fig. 5) can be shown in one figure. The AARI ice chart (Fig. 2) is duplicated in Fig. 6 (upper right corner). Fig. 6, 7 show the same information. Table 1 is poorly explained. Please name the steps in the flowchart (Fig. 4) in consistency with the text. E. g. Cross-corr. Is TCC in the text; area mask is referred as a mask in the text. What does Pixelw average stand for? Please, describe in the text. All figures require better captures, legend, geographic information and land mask (if applicable).

Technical corrections:

p 1, line 2: Please replace “ove Kara and Barents Seas” by “over the Kara and Barents Seas”

p 1, line 8: Please remove excessive spaces before commas in citations here and throughout the text

p 1, line 11: Missing citation after Zubov, 1945

p 1, line 12-13: Do Yu et al. (2014) indeed give this number in their paper? Please, rephrase, in case the 13% is not mentioned by Yu et al. (2014).

p 1, line 13: Please replace “sea ice coverage” by “sea ice cover”

p1, line 16: “quite similar criteria” is kind of vague. Please clarify.

p 1, line 20: Wrong citation. To support your statement, use the work by Maqueda, M., Willmott, A.J. and Biggs, N.R.T., 2004. Polynya dynamics: a review of observations and modeling. *Reviews of Geophysics*, 42(1). The importance of fast ice was not studied by Selyuzhenok et al. (2015). The paper rather describes changes in the fast ice regime. Please, move the reference to p 1, line 18 : “later formation and earlier disappearance (Mahoney et al. , 2014, Selyuzhenok et al., 2015)”.

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p 1, line 24-25: “The proposed method has been used and will be used for. . .” Has the method been used before? The sentence seems to be out of the context. Please move it to the end of the introduction, where the proposed method is introduced.

p1, line 25: What is the existing LFI time series? Are you referring to the AARI charts or another product? Please clarify.

p 2, line 8: Please replace “ in the case on” by “ in the case of”

p 2 line 14-15: The sentence starting with “In Mahoney et al. 2004, 2005..” sounds as the fast ice was detected based on mosaic edge, orientation and temporal difference. I suggest changing to “In Mahoney et al. (2004, 2005) LFI is detected based on vector grayscale gradient fields of 3 subsequent SAR images”

p 2, line 2: Replace “re-reprojected” by “reprojected”

p 3, line 21: adjacent daily SAR mosaic?

p 3, line 23: in Fig 4.?

p 3, line 25: To increase computing performance and to exclude. . . ?

P 3, line 29: Please replace “i,e, white areas in Fig. 4” by “i. e. white areas in Fig. 5”

p 4, line 1: “ less than zero”, Did you mean “less than one” or it is indeed negative?

P 4, line 8: Please remove “still” in “ We still additionally applied..”

p 4, line 12: Please remove “still” in “ we still additionally perform..”

p 4, line 12: Please replace “logical and operation” by “logical AND operation”

p 4, line 13: Please remove “in this context”

p 4, line 15: Typo in “results”

p 4, line 19-22: Inconsistent terminology: FMI algorithms, SAR algorithms

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p 4, line 25: Missing figure number (7)

p 4, line 32-33: Duplicated “whole study” and “our study area”

p 5, line 3: Typo in “erroneous”

p 5, line 10: Typo in “developed”

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

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