The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-260-AC2, 2018

The Cryosphere Discussions

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

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Received and published: 5 March 2018

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 formating errors do not make an impression of a self-contained manuscript.

Thank You for the valuable comments. I have tried to improve the manuscript according

to the reviewer comments, tried to improve the description of the methodology, description of the results and manuscript layout and language. 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. 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.

Clear advantage compared to methods based on ice drift is that ice drift detection is very slow compared to direc cross-correlation computation. The advantage compared to cross-correlation minimum is that average and median are more robust to errors than minimum which is only one value instead of statistics. Methods based on SAR backscatter do not wotk in all conditions as SAR backscattering from sea ice varies according to many physcial parameters, for example including surface and snow cover wetness and SAR incidence angle. Have included a few sentences on this in the concluding section.

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 have tried to improve this part also. Also trying to make clear that the raster AARI ice

charts were used in this study.

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?

I have included a monthly comparison of the classification accuracy w.r.t. AARI ice chart LFI. In August-October 2016 there was not much LFI and comparison was not feasible. A shorter time period was used in the original study made for the Horizon-2020 EC project SPICES with the data at my disposal at that time. I have now extended the AARI comparison to cover a whole year time span.

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. 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 daily product is based on the daily mosaics and naturally also the mosaics of the preceding two week time period have been used in the averaging process. Not all SAR mosaic locations (grid points) are actually updated daily, but the mosaics are continuously updated daily, so it is called a daily mosaic and daily product. Daily means that the LFI extent using the most recent mosaics and mosaic history of the preceding two weeks is issued daily. The SAR mosaic is updated on average in about two days after launching of SENTINEL-1b (late April 2016) even more often , but at some grid locations the values can be even three days old. Construction of the mosaics has been

tried to be explained in more detail in the revised manuscript.

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?

The threshold in the previous version were defined experimentally based on visual interpretation of a few images. In the updated version the threshold have been selected based on a comparison to AARI LFI and selecting the optimal (minimizing the error) value. Also figures showing the error as a function of the threshold have been included in the updated version to shown the sensitivity. The four AARI ice charts used in defining the thresholds have been excluded from the evlauation of the results.

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.

By these I mean temporal cross-correlation average and median. I hope this is more clearly explained ibn the revised mansuscript. also see my response to reviewer 1 on this topic.

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.

Extended land mask was not applied. Instead the summer (August 2016) LFi detected by FMI-A were excluded. The same erroneous LFI areas also appeared in the summer 2017 FMI-A results and it was concluded that these are because of small errors in the

positioning of the land mask at certain locations.

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 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”.

The text has been updated.

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?

The numbers were incorrectly computed in the previous version, they have been updated to correct numbers now. Also it should be taken into account that the time step in the time series with the AARI data is one week and in the full time series it is one day. Some abrupt changes can not be seen in the weekly time series.

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.

The revised results with thresholds defined based on the AARI ice chart LFI are closer to AARI ice charts. It should also be taken into account that AARI ice charts also have their error sources. also the time span of AARI ice chart input data and the FMI algorithms are differemnt (a few days vs two weeks).

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 doest Pixelw average stand for? Please, describe in the text. All figures require better captures, legend, geographic information and land mask (if applicable).

Many of the figures have been updated. The study area figure and the mask figure are still separate figures, in my opinion int is clear this way and they can bhe placed

in a suitable place within the manuscript as separate images. after all, in an electronic publication, like TC, the number of figures is not that relevant as it may be in a traditional printed media.

The figures and figure captions have been updated according to the erviewer comments. Some figures with not much contribution have been removed/replaced. Also the text referring to the figures has been updated.

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

Corrected.

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

I think these are automatically generated by latex based on the copernicus template.

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

Corrected.

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).

It is not directly said in the reference, but can be derived from the numbers given. I have tried to rephrase this.

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

Corrected.

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

Some explaining text has been added.

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)”.

Thank You for the reference! cjanged as suggested and reference added.

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.

The method has been used in this study and will be used in completing the time series (next time will likely be in spring 2018 after the busiest Baltic Sea ice season will be over).

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

At least there are ice charts, including AARi ice charts. I am not aware of all possible time series, not all of them are public. Added ice charts in the sentence.

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

This is not included in the revised text any more.

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”

Changed as suggested.

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

Changed.

p 3, line 21: adjacent daily SAR mosaic?

Changed.

p 3, line 23: in Fig 4.?

Changed.

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

Changed.

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

In the revised version it is Fig. 4.

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

Yes, changed.

P 4, line 8: Please remove “still” in “ We still additionally applied..” p 4, line 12: Please remove “still” in “ we still additionally perform..”

Text has been changed.

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

AND now written with capital letters.

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

REmoved.

p 4, line 15: Typo in “results” p 4, line 19-22: Inconsistent terminology: FMI algorithms, SAR algorithms 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”

These have (hopefully) disappeared in the revised text. Much of the text has been updated in the revised version.

I hope the revised version has improved compared to the first version and we are iterating towards the correct direction!

Thank You for Your comments!

Juha Karvonen, FMI

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2017-260/tc-2017-260-AC2-supplement.pdf

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