

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

**Anonymous Referee #1**

Received and published: 4 January 2018

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.

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

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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). 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. 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 shape 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 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

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description of the choice of 14 days for the FMI-B algorithm. 4) P1,L17: You need to explicitly mention which region these statements are true for. 5) P2,L21: Conspicuous absence of the Meyer et al reference here - doi:10.1016/j.rse.2011.06.006 6) P2,L28-31: Unnecessary detail for a manuscript – consider removing 7) P5,L13: “rather good” is not quantitative enough – stemming from the rather qualitative comparison between datasets. 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. 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. 10) P5,L26-31: A repeat of earlier in the paper. 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? 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.

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. Figure 2: Completely illegible. 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.) Figure 4: Caption doesn't indicate that this is for FMI-B. I question the necessity of this figure too. Figure 6: The caption should refer to FMI-A 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. Figure 8: No comment on why the AARI charts underestimate fast ice compared to your work. Figure 8: Doesn't the value of  $\sim 0 \text{ km}^2$  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

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are not correctly masked out.

Originality (novelty): - Yes – new methods, insights and data.

Scientific quality (rigour): - Purpose of the work is clearly articulated, but the description of the methodology is borderline poor. Techniques appear to be valid and suitable. Results are limited – this appears to be mainly a techniques paper with basic validation. Consideration of other work is up to scratch though.

Significance (impact): - In current form, significance is limited. It's a very short time series covering a relatively limited geographic region (despite the grandiose title!). It is a good “techniques” paper but there is scant exploration of the time series presented. Given its limited scope, I question its suitability for TC.

Presentation quality: - The presentation quality is not very good. Typographical errors throughout, references missing, references not presented well, etc. Figures are lacking clarity, size, and essential elements. English use is quite strange in parts but I unfortunately don't have time to go through it in more detail.

In the full review and interactive discussion, the referees and other interested members of the scientific community are asked to take into account all of the following aspects: 1. Does the paper address relevant scientific questions within the scope of TC? Y 2. Does the paper present novel concepts, ideas, tools, or data? Y 3. Are substantial conclusions reached? N 4. Are the scientific methods and assumptions valid and clearly outlined? Y 5. Are the results sufficient to support the interpretations and conclusions? Y 6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? N 7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Y 8. Does the title clearly reflect the contents of the paper? N 9. Does the abstract provide a concise and complete summary? N 10. Is the overall presentation well structured and clear? N 11. Is the language fluent and precise? N 12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

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N/A 13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Y – method section needs clarification 14. Are the number and quality of references appropriate? Y 15. Is the amount and quality of supplementary material appropriate? N/A

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