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Interactive comment on "Open-source sea ice drift algorithm for Sentinel-1 SAR imagery using a combination of feature-tracking and pattern-matching" *by* Stefan Muckenhuber and Stein Sandven

Anonymous Referee #1

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General comments The authors present a new approach for sea ice motion tracking, combining a modified feature tracking algorithm (Muckenhuber, 2016) with a basic pattern matching approach using cross correlation. The authors thereby replace the often used iterative cross correlation approach within an image resolution pyramid by a feature tracking step (which involves a resolution pyramid as well) to predict the search direction for the higher resolution levels of the cross correlation step. A. Berg and L. E. B. Eriksson (2014) presented with their paper on "Investigation of a Hybrid Algorithm for Sea Ice Drift Measurements Using Synthetic Aperture Radar Images," based on the combination of pattern matching (cross and/or phase correlation) and feature tracking.

In 2014 Komarov and Barber published an algorithm (also referred in this paper), which uses a kind of correlation based feature tracking – since it first identifies characteristic points for the following correlation. The idea to combine feature tracking and pattern matching for sea ice drift estimation is tempting and I really like it. It would potentially allow estimating sea ice motion faster and in the case of appropriate feature descriptors even that are rotationally invariant for areas which contain not only translational motion but rotational motion as well. This characteristic can be especially useful in regions like the marginal ice zone, where rotational motion occurs relatively often. However, the devil is in the details. The idea of study is first step in the direction of a rotational invariant drift algorithm (or at least more robust against rotational motion) for the marginal ice zone and would therefore be worth being published in the Cryosphere after major revisions. However, due to some open questions regarding the implementation of the approach and its validation I cannot recommend its publication at this point. I would like to encourage the authors to continue the work on this interesting idea and resubmit a strongly revised version of this work in the future, but being a bit more careful next time.

My main concerns are: 1. the suggested logarithmic scaling and its surprising limits (I guess there is something wrong with the calibration routines,) 2. The very vague description of the combination of feature tracking and pattern matching 3. And the slightly irritating validation approach

I would be happy if I was of any help for your review process and wish you Season's greetings and best wishes for the New Year!

Specific comments

Introduction Page 3 Line 62-63 "the resulting vectors are independent of their neighbours [which] is an important advantage . . ." – I'm afraid I have to disagree at that point, especially given the implemented feature tracking algorithm. – It has the advantage that it is fast, that it does not get confused by rotational motion and is able to estimate the translational motion even in regions with occurring rotational motion (and that is already great!) but since the employed feature tracking uses a resolution pyramid as well and simply combines all vectors from the different levels of the resolution pyramid, the resulting vectors are neither necessarily all independent nor have the same accuracy (given that some of them are based on a coarser version of the image). Regarding shear and deformation zones, I would claim that a pattern matching algorithm could do the same with an optimised search strategy. Even more problematic, the suggested feature tracking algorithm only identifies a given number of features for the whole scene. In the worst case, a shear zone or a divergence / convergence zone would not be covered at all, if other features in the scene have a higher score.

Page 3 Line 69 "comparable quality estimate for each vector" – I wish there were! There has been a first suggestion by Hollands, Linow and Dierking in 2015 and there is definitely the potential to do so but it is far from being a standard.

Data Page 3 Line 92 "this data type" - the dual pol version of this data type is only available for the southern part of the Arctic and the Coastal regions and not at all for Antarctica. Since their feature tracking algorithm prefers HV polarisation I wonder if the authors have analysed the results of their algorithm in the case of HH polarisation only to predict a potential performance for the otherwise omitted regions.

Method

Page 4 Line 118 "good geolocation accuracy" – I believe I remembered some discussions, that there were some geolocation problems with Nansat earlier, which effected the drift estimation. If I remember correctly: is there a chance that the authors could quantify what "good" means in this respect?

Page 5 Lines 126 – 135 For a start I would suggest to change the order of the explanation and first mention the conversion from linear to log scale before the authors mention the scaling to integer values between 0 – 255 but this is the easier part. The more difficult part might be that we have a problem if there are no typos in these lines

and I understood everything correctly. Log(0.013) = -1.88 dB while log(0.08) = -1.1 dB. If their minimum backscatter values are in dB as well (units missing!), it would mean, that the authors only use the range between -3.25dB – -1.88dB for HV and the range between -2.5dB – 1.1 dB. Could the authors please comment on this and even rephrase this part if I just misunderstood the authors? The problem I see is that their chosen backscatter range only represents a minor part of the backscatter range to be expected for sea ice in the logarithmic scale. If these are the correct numbers, the authors might as well want to check the calibration routines for their data.

Page 6 Line 166 "serves as a quality estimate of the matching performance" - After it has been shown by Hollands, Linow and Dierking (2015) that there is no relation between the matching error and the correlation coefficient I would prefer a proof why the authors can use it as a quality measure. Even their Fig. 7 shows that the authors also dismiss good values, using the correlation coefficient as a quality value. Admittedly there is a group of large error values in their histogram but I wonder if this is significant. A correlation coefficient is only meaningful if the respective texture is characteristic enough. - I suggest to google Anscombe's quartet. Combination

Page 6 Line 173 – 176 "To filter outliers, . . . removed" – I have to admit, it would help me, if the authors could describe this outlier removal in more detail – based on the current description it is difficult to evaluate what the authors actually did.

Page 6 Line 177 – 181 "The remaining feature vectors . . . neighbouring feature tracking vectors" – Just for the better understanding: What happens if there is a large area with no vectors at all framed by a few sparse vectors. Would the authors just triangulate over the whole area (potentially containing deformation or shear zones)?

Page 6 Line 181-183 "To provide a drift estimate . . . combination of x1 and y1." – similar to Line 173 -176 it is hard to say, what the authors actually did. May be the authors could add some details, making it easier to follow.

Page 6 Line 187- 190 I find it a bit confusing that we have a given size of the window before it is tuned. The same is true for dmin and dmax: It only became clear when I reached section 3.3. I would suggest that the authors mention here that they are going to identify the optimal parameters and may be as well why the authors decided to choose formula (4) for the window size.

Page 7 Figure 1 It would be interesting so see a SAR image for the same area and may be a drift vector field. Is it correct that there is land where the distances are low and sea ice where the distance colour scale is saturated? Actually the authors already anticipate a result of their parameter tuning here. That makes it difficult to read. May be the authors should reorganise this part.

Page 7 Line 195 "–beta +beta with step delta beta" – it is confusing that the authors suddenly start to introduce rotation as well since it has not been mentioned beforehand. The authors should have at least introduced it in section 3.2 II.

Section 3.2 page 5-7 Given that this section is meant to be the innovative part of this study I suggest restructuring it, to make it more concise. Right now, it is quite confusing and has varying level of detail and order (e.g. the window size question is a specific cross correlation question. I would urge the authors to state clearly when they introduce a parameter which they want to tune in the later course of the paper. Additionally I would suggest adding a flow chart, highlighting the steps, described in this paper.

Page 8 Formula 6 Why did the authors choose this distance measure instead of the RMSD in Formula 5?

Section 4.1 Honestly, I would suggest skipping this section – it is not surprising that the logarithmic scaling leads to a higher number of features since the logarithmic histogram scaling favours the structures in the sea ice which are mainly represented in the shadow and medium backscatter values but hardly in the highlights.

Page 10 Section 4.2 / Table 2 I have various questions: âĂćI understood that the authors tuned their Influence domain parameter dmax based on one image pair over

Fram strait as well as the side length for their template but how did the authors tune their Dmin value and the MCCmin value? âĂć70 x 70 pixel for t1 means that their correlation window covers an area of approx. 6.3 x 6.3 km – how does this go along with their claim to resolve deformation and shear zones? âĂćSince their influence domain influences the size of their search window t2 it would mean that the authors add a degree of freedom of +/-1.8 to +/-11.25 km to their first feature tracking based guess, which would push their 0.5 m/s maximum ice drift limit for the feature tracking to about 0.6 m/s – right? Its contribution would however vary depending on the time span between both images of the scene. For the same constant drift velocity (but speed variations with in the scene), an image pair with a longer time span would then show larger displacement differences with in the scene while having the same maximum degree of freedom of +/- 11.25 km like an image pair that has been acquired at the same day – this might cause a problem, don't the authors think?

Page 10 Section 4.3 line 249: "on a grid with 8 km spacing" – I suggest to summarize the information of their resulting product somewhere. It is not necessarily obvious to find the information on their grid spacing in the Parameter tuning and Computational Efficiency Section.

Page 10 Section 4.3 Given the resolution of 8 x 8 km even pattern matching only based algorithms show a similar performance or even better. But I admit that the robustness to rotational motion is very useful in the marginal ice zone, where many of the pure pattern matching algorithms fail.

Page 10 Section 4.4 line 261: What does the size of 34 pixel mean? Is the feature described as a patch of 34 x 34 side length? May be the authors should add a short explanation to their feature tracking part on page 5.

Page 12 Line 268-271: Why do the authors choose a minimum Cross Correlation Coefficient of 0.35? If the authors found a logarithmic function their distance distribution seems to follow, the authors could name it. Otherwise less strict term would be that the distance distribution seems to show a logarithmic behaviour or something like this. A peak at 300m is not necessarily meaningful (e.g. what would be the peak without their Cross Correlation Threshold? How many drift vectors form a peak?) but even if the authors have a peak, it does only represent the systematic component of the error and not the random one. In order to identify the distribution I would suggest smoothing the histogram and fitting a distribution to it.

Page 14 Table 4: I would think that it is not the best approach to validate an algorithm based on the drift vectors I tuned it to. For a real validation the authors need at least another independent image pair with an independent set of manually derived drift vectors. I would strongly encourage the authors to change this! The authors compare apple with oranges if the authors compare an algorithm tuned to this specific scene with algorithms like the one from CMEMS. Additionally it would be great, if the authors could quantify both systematic and random error.

Page 14 Line 290: "To further estimate the accuracy of the algorithm . . ." – here it would be interesting to see, how the other algorithms perform as well. Additionally it would be great, if the authors could quantify both systematic and random error. The authors might want to check the regular validation document for the CMEMS ice drift as a start: http://myocean.met.no/SIW-TAC/doc/myo-wp14-siw-dtu-icedrift-glob-obs-validation_latest.pdf The peak of a distribution is no error value!

Page 14 Line 302-303: "Hence, . . . image resolution" I agree there are various factors influencing the result of the algorithm and thereby influencing the validation but I cannot agree with this statement. It might be but the authors have not shown this yet!

Technical corrections

Page 1 Line 5: "respective advantages of the two approaches" - the authors should emphasise in more detail what the advantages are, since this is the basic justification for this paper and this not only in the abstract but in the introduction/motivation as well

Page 3 Line 37 "covers the Arctic every week with a spatial resolution of 5 km" – I'm not sure but the authors might want to check it: as far as I know the, RGPS covers a large part of the Western Arctic Ocean but not the entire Arctic, due to the acquisition area of Radarsat. Up to my knowledge, the 5 x 5 km spatial resolution is a gridded drift field, which does not necessarily represent the actual spatial resolution, given that the RGPS searches features in a 10 or 25 km grid respectively. See also the RGPS Data User's Handbook (Fig. 1 and Fig. 2)

Page 3 Line 73 "respective advantages" – If possible, be clearer about the respective advantages and summarise them here together with the disadvantages the authors still have and those the authors bypass with their approach.

Page 2 Line 44 "pattern-marching and feature tracking respectively" – even terms are somehow flexible: I would claim, that Komarov and Barber do somehow a basic feature tracking as well, since they identify features, with certain characteristics before the correlate them – in that way, they have implemented the search for descriptors in a way. The use of correlation does not necessary mean that the approach is a pattern matching approach, since the correlation itself is the distance measure only, that is used to assess how similar a feature or a pattern is, compared to the reference. It might be a bit pedantic, but the authors might still want to give it a second thought.

Page 2 Line 52-55 "Making use . . . Copernicus.eu)." – I agree, that it is an important product, which should definitely be mentioned in the frame of this article but I think, the statement does not really fit there where it is right now because it interrupts their motivation.

Page 6 Line 185 -186 "Figure 2 shows. . ." – I would suggest moving the sentence a few sentence down to Line 195 after ". . .correlation value is returned

Page 10 Section 4.3 line 252-254: "NB: The vectors near . . . treated with caution" - I completely agree but it is no question of computational efficiency

Page 10 Section 4.4 line 256: Strictly speaking the authors should compare their estimated drift vectors to their manually derived vectors and not the other way round and the authors estimate a drift vector and do not calculate it but this is a minor technical issue I guess.

Page 11 Table 3: Is it correct, that their drift estimation is only based on HV polarisation? I guess the authors should state it somewhere in the beginning. Given their experience with dual pol motion tracking, I assumed that the authors used both polarisations here as well? I suggest being clearer about it from the beginning, if this is the case.

Page 15 Line 311: "The parameters can easily be varied. . ." – a short tabular overview on the range for the individual parameters and their effect on the algorithm performance would be nice even though probably difficult.

Page 15 Line 329: "the real sea ice velocity" – the velocity the authors observe is not wrong, they might underestimate the speed and its variation as well as the variation of the drift direction but velocity is defined as distance per time, and the resulting velocity vector, being a sum of velocity vector variations over the observation interval is the resulting velocity vector. A higher temporal resolution is interesting but it is as interesting and influences the "realness" of their velocity vector the same way higher spatial resolution does. It would be great if the authors could give this phrase a second thought.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/tc-2016-261/tc-2016-261-RC1-supplement.pdf

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