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Response to Referee # 1

’Open-source sea ice drift algorithm for Sentinel-1 SAR imagery using a combination of feature-tracking and pattern-matching’

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Dear Referee # 1,

Thank you very much for helping us improving our paper.

Please find here the **answers** to your comments and the corresponding ***changes in manuscript***:

1 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 work from Berg and Eriksson needs to be mentioned and we included

Berg and Eriksson (2014) introduced a hybrid algorithm for sea ice drift retrieval from ENVISAT ASAR data using phase correlation and a feature based matching procedure that is activated if the phase correlation value is below a certain threshold.

Unlike Berg and Eriksson (2014), the feature-tracking step is performed initially and serves as a first guess to limit the computational effort of the pattern-matching step.

To specify the approach from Komarov and Barber 2014, we changed ’pattern-matching’ to

combination of phase/cross-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.

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

There is a typo in previous Section 3: the values of the brightness boundaries were given in B and not dB. We corrected that and included a histogram of a representative image pair to illustrate the chosen boundaries. The description of the algorithm has been changed and extended. The validation approach has been changed and the error analysis has been extended. Details are given below.

2 Specific comments

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.

With the term 'independent', we wanted to refer to the fact that features are identified without taking the position of other features into account and matched from one image to the other without taking the drift and rotation information from surrounding vectors into account. It is true that features can overlap, the resolution varies due to the resolution pyramid and the independent feature positioning can lead to missing important drift information. We changed the sentences to:

This can be done computationally efficient and the resulting vectors are often independent of their neighbours in terms of position, lengths, direction and rotation, which is an important advantage for resolving shear zones, rotation and divergence/convergence zones. The considered feature-tracking approach identifies features without taking the position of other features into account and matches features from one image to the other without taking the drift and rotation information from surrounding vectors into account (Muckenhuber et al., 2016). However, due to the independent positioning of the features, very close features may share some pixels and since all vectors from the resolution pyramid are combined, the feature size varies among the matches, which implies a varying resolution. In addition, the resulting vector field is not evenly distributed in space and large gaps may occur between densely covered areas, which can eventually lead to missing a shear or divergence/convergence zone.

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.

We agree and removed this part of the sentence.

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.

The focus of this paper is put on HV, since this polarisation has a better feature-tracking performance and we found a good coverage of this data type in our region of interest. We did not yet analyse the HH performance of the algorithm on a large dataset, but this will certainly be addressed in our future work. We added the following to Section 2:

The introduced algorithm can utilise both HH and HV channel. However, the focus of this paper is put on using HV polarisation, since this channel provides on average four times more feature tracking vectors than HH Muckenhuber et al. (2016), representing a better initial drift estimate

for the combined algorithm.

To further address the polarisation topic, we added the following to Section 5:

The focus of this paper in terms of polarisation was put on the HV channel, since this polarisation provides on average four times more feature tracking vectors than HH and therefore delivers a finer initial drift for the first guess. We found our area of interest well covered with HV images, but other areas in the Arctic and Antarctic reveal a better coverage in HH polarisation. Considering the four representative feature-tracking image pairs from Muckenhuber et al. (2016), the the relatively best HH polarisation performance (i.e. most vectors from HH, while at the same time fewest vectors from HV) was the image pair that showed the least time difference, i.e. 8 h, compared to 31 h, 33 h and 48 h. Therefore, we assume that the HV polarisation provides more features that are better preserved over time. And more consistent features would also favour the performance of the pattern-matching step. However, at this point, this is just an assumption and will be addressed in more detail in our future work.

Utilising the advantage of dual polarisation (HH+HV) is certainly possible with the presented algorithm, but increases the computational effort. A simple approach is to combine the feature tracking vectors derived from HH and HV and produce a combined first-guess. Pattern-matching can be performed based on this combined first-guess for both HH and HV individually and the results can be compared and eventually merged into a single drift product. Having two drift estimates for the same position, from HH and HV pattern-matching respectively, would also allow to disregard vectors that disagree significantly. However, this option would increase the computational effort by two, meaning that the presented Fram Strait example would need about 8 min processing time.

After implementing the presented algorithm into a super-computing facility, we aim to test and compare the respective performance of HV, HH and HH+HV on large datasets to identify the respective advantages.

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?

We discovered drift artefacts in high latitudes between the ground control points before we introduced spline interpolation and reprojection to stereographic. We tested the performance before and after introducing these steps and the artefact disappeared using either one of the steps. To ensure the best possible performance, we apply both steps. The geolocation accuracy depends on the accuracy and amount of ground control points that are delivered in the metadata of the Sentinel-1 scene. At the ground control point the location accuracy should be highest. We cannot give an error value in meter, since we do not have validation

points on the ground and cannot control the accuracy of the Sentinel-1 ground control points. However, from our experience and comparison with buoy drift data, the geolocation accuracy is expected to be in the order of the image resolution.

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.

We changed the order of explanation and first mention the conversion from linear to log scale before the scaling to integer values. There has been a typo with regards to the units: the values of the brightness boundaries were given in B and not dB. We corrected that, added the units and included a histogram of a representative image pair (Figure 1) to illustrate the chosen boundaries and show the representative image pair after the conversion into the integer range (Figure 2).

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.

We agree and removed the claim that the cross correlation value could serve as quality estimate. However, using the considered validation data, we found for our data type, time period and area of interest that the probability for large errors decreases with increasing cross correlation value. The following was added to Section 3:

We found that the probability for a large D value (representative for the error) decreases with increasing maximum cross coefficient value MCC. Therefore we suggest to exclude matches with a MCC value below a certain threshold MCC_{min}. This option is embedded into the

algorithm, but can easily be adjusted or turned off by setting $MCC_{min} = 0$.

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.

The algorithm description has been changed and more details have been included. A subsection 'II Filter' and Figure 4 have been added to describe and illustrate the filtering process.

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)?

If the considered area lies in between three feature tracking vectors, we triangulate over the area to provide the first guess. This initial drift estimate however, will then be adjusted by the pattern-matching approach. If the closest feature-tracking vector is far away, we apply the lowest restrictions defined by d_{max}. We found a useful value for d_{max} for our area and time period of interest to be 100 pixels, meaning that the search area is defined by an 8 km radius around the first guess. The lowest restrictions can easily be adjusted according to expected ice conditions and computational performance.

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.

The algorithm description has been changed and more details have been included. A subsection 'III First guess' and Figure 5 have been added to describe and illustrate the process that leads to the first guess.

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.

We changed the description of the pattern-matching step and adjusted the order according to this comment. We clarify which parameters need to be specified for the introduced pattern-matching procedure and how we find the recommended setting for each parameter.

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.

We added Figure 2 to illustrate the related SAR images and show the corresponding feature-tracking vectors in Figure 3. The colour scales of the left and middle panel represent the first guess of the end positions on SAR_2 and the colour scale of the right panel indicates the distance to the closest feature tracking vectors, i.e. values of $d = 10$ represent 0-10 pixel distance to the closest feature-tracking vector and values of $d = 100$ represent 100-∞ pixel distance. We split the figure and changed the algorithm description accordingly to make the process better understandable.

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.

The algorithm description has been changed accordingly and rotation is introduced in the new subsection 'IV Pattern-matching'.

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.

We adjusted the algorithm description accordingly and added more details. Figure 3 includes a flow chart and respective example images to illustrate the algorithm steps and the resulting products.

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

The $RMSD$ equation (previous Equation 5) and the comparison to the manually drawn vectors have been removed. The distance measure D (previous Equation 6) has been used to get an individual error value for each compared vector pair, consisting of one validation vector and one algorithm vector. Since we found a logarithmic error distribution for the buoy comparison, a mean value as expressed by the $RMSD$ does not represent the found distribution.

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.

We skipped this Section and briefly mention in the data pre-processing why a logarithmic distribution is used:

Using a logarithmic scaling provides a keypoint distribution for the feature tracking algorithm that depends less on high peak values, while the total number of vectors increases.

Page 10 Section 4.2 / Table 2 I have various questions:

- I 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?

The parameter tuning was removed from the manuscript. Instead, useful restrictions that limit the computational effort of the pattern-matching were found and a useful MCC_{min} value was found according to the error distribution from the buoy comparison.

- 70 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?

We agree that this resolution is not sufficient and changed the recommended setting to 34×34 pixels in order to be consistent with our goal. We added the following to Subsection 'IV Pattern-matching':

The size of the small template $t_{1s} \times t_{1s}$ defines the considered area that is tracked from one image to the next and hence, affects the resolution of the resulting drift product. In order to be consistent with the resolution of the feature-tracking step and achieve our goal of a sea ice drift product with a spatial scaling of less than 5 km, we use the size of the feature-tracking patch of the pyramid level with the highest resolution to define the size of t_1. That means, we use $t_{s1} = 34$ pixels (2.7 km).

- Since 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?

Yes, this is a good point and needs to be considered. We added the following to Section 5:

The current setting of the feature-tracking algorithm applies a maximum drift filter of 0.5 m/s. We found this to be a reasonable value for our time period and area of interest. However, when considering extreme drift situations in Fram Strait and a short time interval between image acquisitions, this threshold should be adjusted.

During a KV Svalbard cruise in summer 2016, we deployed three GPS tracker in Fram Strait that recorded their positions with a temporal resolution of 5-30 min between 8th July until 9th September 2016 in an area covering 75° N to 80° N and 4° W to 14° W. Considering the displacements with 30 min interval, we found velocities above 0.5 m/s on a few occasions, when the tidal motion adds to an exceptionally fast ice drift.

The GPS data from the hovercraft expedition FRAM2014-2015 (https://sabvabaa.nersc.no), that was collected with a temporal resolution of 10 s between 31st August 2014 until 6th July 2015, did not reveal a single 30 min interval during which the hovercraft was moved by ice drift more than 0.45 m/s. The hovercraft expedition started at 280 km south from the North Pole towards the Siberian coast, crossed the Arctic Ocean towards Greenland and was picked up in the north-western part of Fram Strait.

In case the estimated drift from feature-tracking reaches velocities close to 0.5 m/s, the pattern-matching step might add an additional degree of freedom of up to 8 km, which could eventually lead to a higher drift result than 0.5 m/s, depending on the time interval between the acquisitions. The smaller the time difference, the larger is the potentially added velocity. In order to be consistent when combining the drift information from several image pairs with different timings, one should apply a maximum drift filter on the final drift product of the presented algorithm that has the same maximum velocity as the feature-tracking filter. The corresponding function is implemented in the distributed open-source algorithm.

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.

The considered grid is not meant as a given parameter of the resulting product, but serves only to provide an estimate for the computational efficiency of the presented approach. The points of interest, given in longitude and latitude, represent the input for the algorithm. This can be the position of a ship, the grid of a model or an evenly spaced grid with any wanted resolution. The algorithm includes a routine that can derive points of interest in lon-lat on an evenly spaced grid. We hope that the changed algorithm description improved the explanation regarding points of interest and considered grid.

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.

We changed the resolution of the example and put more focus on the rotational motion.

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.

We removed this Section and added a description of the considered feature patch sizes to Subsection 'I Feature-tracking'.

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.

We smoothed the histogram and fitted a logarithmic normal distribution to it. We found the chosen minimum cross correlation coefficient $MCC_{min} = 0.4$ by plotting MCC values against distance D, that represents the error. This is now shown in Figure 11.

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.

We removed the parameter tuning and do not compare our results against the manually drawn vectors anymore. Figure 11 is included to illustrate 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.noSIW-TACdocmyo-wp14-siw-dtu-icedrift-glob-obs-validation_latest.pdf The peak of a distribution is no error value!

We illustrate the error in Figure 11 according to this suggestion. We removed the comparison with CMEMS and simple feature tracking, since we don't have drift results of these two algorithms at the buoy locations.

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!

We removed these two sentences.

3 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

The feature-tracking and pattern-matching description in Section 1 has been improved and the corresponding part in the abstract has been changed to:

Feature-tracking produces an initial drift estimate and limits the search area for the consecutive pattern-matching, that provides small to medium scale drift adjustments and normalised cross coefficient values. The algorithm is designed to combine the two approaches in the most meaningful way in order to benefit from the respective advantages. The main advantages of the considered feature-tracking approach are the computational efficiency and the independence of the vectors in terms of position, lengths, direction and rotation. Pattern-matching on the other side allows better control over vector positioning and resolution.

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)

We changed the sentence to:

The geophysical processor system from Kwok et al. (1990) has been used to calculate sea ice drift fields in particular over the Western Arctic (depending on SAR coverage) once per week with a spatial resolution of 10-25 km for the time period 1997–2012. This extensive dataset makes use of SAR data from Radarsat-1 and ENVISAT (Environmental Satellite).

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.

The feature-tracking and pattern-matching description in Section 1 has been improved and in addition, we added to Section 1:

The main advantages of the considered feature-tracking approach are the computational efficiency and the independence of the vectors in terms of position, lengths, direction and rotation. Pattern-matching on the other side allows better control over vector positioning and resolution, which is a necessity for computing divergence, shear and total deformation.

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.

We changed the sentence to:

Komarov and Barber (2014) and Muckenhuber et al. (2016) have evaluated the sea ice drift retrieval performance of dual-polarisation SAR imagery using a combination of phase/cross-correlation and feature-tracking based on corner detetction respectively.

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.

We moved the sentence into the paragraph above.

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

The method description has been restructured taking this comment into account. We refer to this Figure at a later point in the description.

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

This sentence has been removed.

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.

The comparison with the manually drawn vectors have been removed and we took this comment into account, when describing the comparison to the GPS buoy dataset.

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.

Yes, the considered drift estimates in this work are based on HV. We added the following to Section 2:

The introduced algorithm can utilise both HH and HV channel. However, the focus of this paper is put on using HV polarisation, since this channel provides on average four times more feature tracking vectors than HH (Muckenhuber et al., 2016), representing a better initial drift estimate for the combined algorithm.

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.

We updated the algorithm description taking this comment into account. The parameters and their effect on the drift result are now explained more in detail for a better understanding of eventual changes from the recommended setting. The possible range of the parameters t_{s1}, t_{s2}, β, $\Delta\beta$, d_{min} and d_{max} is not limited.

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.

We removed the term 'real sea ice velocity' and changed it to *sea ice displacements with higher temporal resolution*, that *reveal more details e.g. rotational motion due to tides*. Section 5 has been updated accordingly.

Thanks again for your comments. We are looking forward to your reply!

Best regards,

S. Muckenhuber and S. Sandven