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

Dear authors,
thanks a lot for all your work on your paper. It is a pleasure to see a paper on such an interesting topic evolving. I hope my comments help you to improve your paper even further (even though I’m aware that they are numerous :- ( )

Comments to the answers to Referee 1

Most of the comments are fully satisfying but not all. They might double with later comments on the revised version of the article.

Page 2 Line 42-46: The problem with the backscatter values is not solved by correcting a typo and claiming that the values were shown in Bell (a very uncommon unit for backscatter by the way). The correction of the typo shifts the backscatter values into the right magnitude. However, given a noise level of 22 dB for Sentinel-1 I would still like to question the calibration routines used. Using scaling suggested by the authors on correctly calibrated S1 data would mean to focus on a value range mainly containing noise (-32.5 dB - -22 dB) and only partly containing potentially meaningful backscatter values in the range -22 dB to -18.86 dB. The authors might want to check again there calibration routines.

Page 8 Line 245: the use of logarithmic scaling is the standard for all SAR based sea ice research and professional monitoring by universities, ice services and research institutes. But it is good to see that this promising approach uses dB scaling now as well.

Page 8 Line 255: I did not want to vote against parameter tuning or the validation using displacement vectors derived from satellite image pairs. Both instruments are extremely useful. I just thought that a validation using the same dataset originally used for tuning is not the best approach for an independent validation. It is a pity that the authors decided to skip both parts completely but then it is for sure the most effective approach to deal with my remark.

Page 9 Line 278 – 301 I like this new paragraph you added into section5, but as far as I can sayit does not consider the specific problem, I outlined in my question and they suggest to handle with this paragraph (the same search window size means different degrees of freedom depending on the time span between the two considered image acquisitions) - It does however discuss the general aspect of time span regarding the “hard” 0.5 m/s maximum speed threshold but I guess, that’s o.k.

The article itself

General comments

The article improved a lot since the last time and the authors handled the comments of the reviewers in a very efficient manner. It reads like a completely new manuscript. Personally I think that the authors tend to use superlatives like “optimize” or “most meaningful” a bit too often given the flexibility of their approach (various changes between both versions) but nevertheless it seems to be on a right track now and worth being published in “The Cryosphere” after major revisions. Within the frame of the revision it would however be necessary to discuss the differences of this paper to

the recently published paper by Anton Korosov and Pierre Rampal for the same institute on the same topic: *A.Korosov and P. Rampal (2017): "A Combination of Feature Tracking and Pattern Matching with Optimal Parametrization for Sea Ice Drift Retrieval from SAR Data", Remote Sens. 2017, 9(3), 258; doi:10.3390/rs9030258,* which looks quite similar to me. Personally, I think that this parallel publishing from the same institute is at least a bit unfortunate (even though I'm aware, that the manuscript at hand was originally submitted earlier than the now published article by Korosov and Rampal).

Specific comments

Page 4 Line 113-115 I'm not sure how much of the vector independence from the feature tracking actually exists, given the overlap of features from the various resolution levels, the filtering and polynomial least square fitting to identify outliers and the linear interpolation to get first guess estimates for the subsequent pattern matching. What do the authors think?

Page 6 Line 173 – 192 I would like to repeat my statement from the last review: there seems to be something wrong with your calibration routine and it is not only a typo. It would be great for sea ice research if S1 had a sensitivity of -32.5 of even -25dB only but its noise level is at -22dB, which means that especially your lower boundaries for the scaling cause a problem for correctly calibrated S1 images.

Page 6 Line 186 The scaling to 8 bit / 256 grey level is an input requirement for feature tracking using the ORB algorithm but reduces the signal variation. This reduces the effectiveness of the NCC which can handle any numerical precision you like. It is not a problem for medium to larger resolutions (like CMES service provided by the DTU) since there are still enough pixel in each correlation window but as soon as you go to higher resolutions, even slight variations in the signal become more important (since there might be not so many clear variations left in a smaller window). Have the authors tried to use the original dB backscatter values for the pattern matching instead of the scaled 256 greyscale image?

Page 9, Line 223-224 As I mentioned before, the use of logarithmic scaling for SAR is common and I'm actually surprised that the authors did not do it from the start.

II Filters: I have various questions:

1. For the polynomial fit the authors use all vectors (those from coarser resolution levels and those from finer resolution levels – a single pixel on the coarser resolution level covers about 3.6 pixel on the finest resolution level: subpixel uncertainty of +/-145 m vs a subpixel uncertainty of +/- 40 m?) – isn't this a bit of a problem if you merge it all in one polynomial surface and then try calculate if the predicted vector is 100 px away from your polynomial surface?
2. You are applying a least square fit to an angle and calculate a polynomial surface from it. How do you handle the zero-crossing problem and all the other problems you are facing once you start averaging and interpolating angle values?
3. It seems that your angular polynomial surface has a peak at about 130°. Just for curiosity: Is this the rotation between both images?

Page 11 Line 272-273: least square fit and linear function for angles? How do the authors handle the specific problems related with this (see my comments on II Filters)

Page 11 Line 285-287: “The uncertainty … vector” I would claim that it is not the uncertainty of the estimate but the representativeness of the estimate and that it does not only depends on the distance to the next feature tracking based vector but as well on the heterogeneity of the other surrounding feature tracking based drift vectors.

Page 12 Line 307-308: As far as I know the idea to restrict the smaller correlation template to a circle has been strongly propagated by Roberto Saldo from the DTU who uses it for his drift algorithm. I’m not sure if this is what the authors mean. While I’m aware of the advantages to limit the smaller correlation template to a circle, I have no idea why you would want to restrict your search window to a circle (if not only for the reason to safe computation) but may be the authors want to outline the advantages of a circled search window while having a rectangular correlation template. If they meant to describe a circled correlation window the authors want to refer to Roberto Saldo (if there is no publication regarding this matter may be as personal communication).

Page 13 Line 318-319: “In order to be consistent with the resolution of the feature tracking”. What is the advantage of being consistent with the resolution of the feature tracking?

Page 14 Line 344-352 A higher MCC value seems to indicate a higher chance of a better match for your algorithm compared to buoy data. This is an interesting finding since most pattern matching algorithms have lower MCC_min values or none at all and since it was shown, that there is no direct relation between error and correlation coefficient.
This could mean that the author’s initial guess and search strategy are less effective than those of pure pattern matching algorithms (and thereby needing a higher correlation to find a suitable match) especially since the maximum shift from the feature tracking based first guess is 1.6 (d=10) to 16 km (d=100) at maximum. Every difference above that is a hint that the first guess did not work or that the buoy drift has nothing to do with the surrounding ice conditions (Figure 11 a). Additionally I suggest checking the spatial distribution of the vectors the authors rejected due to their low MCC and high spatial distance (e.g. a shearing zone within the correlation window would for example cause as well a drop in the correlation value and potentially a larger difference in displacement relatively to an originally close drift buoy in another drift regime) and study the behavior compared to manually tracked vectors from the image pair ensure that the difference comes really from the algorithm and not from the limitations of the comparison with buoys, the authors are well aware of (page 20 line 439 – 450).

Page 15 3.3 Comparison with buoy data: Just for my understanding: Did the authors center a 34x34 pixel window at the original buoy position? The original buoy position has been acquired every hour or has been interpolate to hourly intervals

Page 15 Line 356 “Each pair yielded more than … three days” I’m not sure but it would probably more interesting to mention the number of vectors, which the recent version of the feature tracking component provides, using HV only and dB scaling, if at all …

Page 17 Figure 9 and 10: Based on the Figures it is difficult to see an advantage for the variation of the search window size or the rotation angle aside from computational efficiency. Would the authors agree?

Page 18 Fig 11 a: something seems to be wrong with the legend

Page 18 Fig 11 b: how would the fit look like if the authors would include the values you rejected due to their low correlation value?

Page 18 Line 407 / Page 20 Line 441 “median and 341.9 m” What’s the significance? Obviously it means that 50% of your data has an error of less than 340 m with respect to the buoy data but it means as well that 50 % have an error, that is larger (and based on the distribution: 1% < 20m, 5% < 43m, 10% < 68m, 68% (1 sigma criterion) < 620 m, 95% (2 sigma criterion) < 2700 m and 99% (3 sigma criterion) < 6400m). But could the authors give a hint what it could mean regarding the quality of the algorithm, given the various influences they identified which could have biased the result? May be a solution would again be to use the manually derived vectors from the original draft for it to account for the algorithm “accuracy” only (well with an “uncertainty” hidden in the manually collected drift vectors of course)?

Page 20 Line 458-460 “The parameters can … strong rotation).” I have a fair idea which parameters to vary to meet changes in computational power, area of interest and expected ice conditions but I have no idea why availability of time, computational power and number of image pairs are mentioned individually since the only parameter behind it is computation time or rather the selection of a finer or coarser resolution. Additionally I would be interested what parameter to change to influence the accuracy!

Page 20 Line 470-484 I guess this paragraph is based on information you received from Nuno Miranda. It would be useful to refer to it as Personal Communication in the text since otherwise the readers might wonder where this information comes from.

Page 21 Line 487-490 “Our next step … less than 5 km” That’s really nice! I’m really looking forward to such a dataset with a good quality. However, given that most of the S1-data in the Arctic and Antarctic is in HH polarization (in Antarctica completely) wouldn’t it been better to perform this study in HH from the beginning, even if there seems to be an advantage for HV in the feature tracking?

Page 21 Line 503 “better coverage in HH pol” – that’s nicely put – the S1 observation scenario does not plan any HV acquisition Antarctic part of the southern ocean and none in the central Arctic ocean but it could of be a reason to demonstrate the need for HV data in these regions as well.

Page 21 Line 507 - 510 “Therefore …future work” A good idea! Just a thought I would like to add. Feature tracking needs linear features which tend to depend a bit more on incidence angle, orbit, and changes in ice condition in HH than in HV and depending on the robustness of your feature detector it might even be quite robust to the quite strong noise in the S1-HV data (it is actually impressive how many feature tracking based vectors you were able to retrieve from the HV scene compared to the HH one). This could explain the better performance of the feature tracking part for the HV component. Cross correlation based pattern matching however is less sensitive to changes in linear feature and more sensitive to areal pattern changes which might potentially favor the HH channel – but as I said: just a thought.

Technical comments

Page 1 Line 9 “pre-processing of S1 data … has been optimized” – Are the authors refereeing to their use of the logarithmic scale for the backscatter again? As far as I’m aware that is all you did and it is a

bit of disappointing if the abstract promises an optimized preprocessing while the article offers the conversion to dB only.

Page 1 Line 10-13 I'm not sure if computational efficiency is necessary in the abstract, the authors might want to give it a second thought but I'm fine if you want to keep it there.

Introduction: I think the introduction improved a lot but especially of page 3 I miss something running like a common thread through it. It reads like an accumulation of independent facts.

Page 3 Line 69 If I'm not mistaken, Hollands and Dierking (2011) implemented their own modified version of the algorithm and did not just continue the work on the algorithm but the authors might want to check that.

Page 3 Line 86 -88 "Muckenhuber … Sentinel-1 data" I suggest adding at the end of the sentence something like: "as a frontend to the ORB algorithm from Rublee included in the OpenCV package" just to give the reader an impression of the used technique, but I might be mistaken.

Page 5 Line 139 "HV polarization" – given that most of the Arctic and the whole Antarctic is only covered by HH data this limits the conclusions for the application of the presented algorithm to the European Part of the Arctic and the Baltic sea.

Page 5 Line 165 "Nansat … gdal.org)." It reads like a commercial – may be the authors want to give this line a second thought.

Page 9, Line 215 I suggest to add :"Given a suitable threshold [and unique features]…"

Page 9, Line 217 I suggest to change: "Muckenhuber … found the most suitable …" to "Muckenhuber … found a suitable …" given the flexibility of your approach and your suggested parameters

Page 10 Line 260-261: I suggest adding something like: "The quality of this "first guess", however depends on the density of the feature vector field and the local ice conditions"