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Interactive comment on "Sea ice drift from Sentinel-1 SAR imagery using open source feature tracking" *by* S. Muckenhuber et al.

Anonymous Referee #1

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The paper by S. Muckenhuber and co-authors entitled "Sea ice drift from Sentinel-1 SAR imagery using open source feature tracking" investigates the adequacy of a previously published motion tracking algorithm based on feature-tracking (the ORB) to the purpose of sea ice drift retrieval from pairs of Sentinel-1 SAR images.

The paper is generally of good quality, well written, and with a sound scientific approach. But a clear weakness is that it does not go far enough in its analysis and discussions of the results to be published as-is in TC. It is striking that section 5 "Discussion and Outlook" has only 17 lines and is more a Conclusion than a Discussion.

Here are four aspects for the authors to take into account when revising the manuscript.

A) Take full account of the availability of operational processing of Sentinel-1 SAR ice

drift as processed at the Danish Technical University (DTU) and distributed through the Copernicus Marine Environment Core Service (CMEMS, former MyOcean). On page 6939 line 20 you write "This introduces a new era in SAR Earth Observation, but no sea ice drift algorithm unsing Sentinel-1 data has been published so far." Although this is correct (it is not published in peer-reviewed litterature), the fact that this product exists and is the current de-facto standard for sea ice drift from Sentinel-1 SAR should reflect on the discussions of this paper. In addition to mentionning this product, the authors could use the estimates from the CMEMS product as comparison to their vectors in Figure 4 (in place or in addition to the manually drawn vectors). This product and its archive are freely accessible from http://marine.copernicus.eu

B) The justification for a feature-based approach (as opposed to the more commonly adopted pattern-matching strategies) is only quickly discussed, mainly arguing that "Feature-tracking provide vectors, which are independent from each other, whereas pattern recognition includes the surrounding drift information"). Here "independent" refers to the fact that neighbouring drift vectors do not re-use the same image pixels. However, pattern-matching methods can be designed to retrieve independent vectors (varying the extent of the correlation area and the spacing between vectors). Please investigate in your revised study the spatial sampling (distance to neighbour) of your (matched) features, and relate this density to the size used for matching the features. This should lead to a discussion on these aspects of “independence of motion vectors” with your set of parameters in feature tracking.

C) The TC reader is curious to learn more about the much higher number of features detected (and matched?) with HV than HH imagery. What is the reason for this? Where are these additional features located in an image? Are HH and HV features mostly at the same location or do they complement each others? Can the authors illustrate the distribution of HV and HH features on a (zoomed) Sentinel-1 SAR image? Are your findings the same as those of Komarov and Barber (2014), or not?

D) Make a clear distinction between Section 4 "Results" and Section 5 "Discussions".

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In addition to the above, here are a series of remarks and typos that are raised to the authors attention:

*) Title: although it is obvious to many, including the authors, "open source" could be specified further as "open source [feature tracking] software".

*) Abstract: Please delete the sentence "A new quality measure for feature tracking algorithms is introduced utilizing the resulting distribution of the vector field". The proposed quality measure is not specific to feature tracking, and is not fully convincing (it might help detecting obviously wrong vectors, but is limited by true discontinuities of the motion fields). It is relevant to use it in your study, but not to highlight it as a new finding.

*) Page 6938 line 20: could you add a sentence discussing the role of sea ice drift in the Southern Hemisphere?

*) Same page line 23-24: are you known with Komarov and Barber (2014)? The resolution they document, I believe, is 3 to 5 km. And RGPS (Kwok et al. 1990) has 5km spatial resolution. So you should probably be more specific about "few kms" (and cite Komarov and Barber 2014). Komarov, A.S.; Barber, D.G., "Sea Ice Motion Tracking From Sequential Dual-Polarization RADARSAT-2 Images," in Geoscience and Remote Sensing, IEEE Transactions on , vol.52, no.1, pp.121-136, Jan. 2014 doi: 10.1109/TGRS.2012.2236845

*) Same page line 25: Use 'seas' instead of 'oceans'?

*) Citation Kwok 2010 and Kwok and Sulsky 2010: these are reviews. They 'demonstrated' these capablities earlier (e.g. in Kwok 1990). Same for Kwok 1998 few lines below. This is where Komarov and Barber (2014) is missing.

*) Page 6939 line 18: although it is not published in peer-review litterature, the operational sea ice drift product from Sentinel-1 SAR of the CMEMS cannot be ignored here. Please mention it, for example refer-

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ring to the Copernicus website (http://marine.copernicus.eu/web/69-interactive-catalogue.php?option=com_csw&view=details&product_id=SEAICE_GLO_SEAICE_L4_NRT_OBSERVATIONS_011_006

*) same page line 26: see discussion item above: the notion of independence is not well-defined here, and you cannot say that pattern-matching methods automatically lead to dependency, nor that feature-tracking implies the vectors are independent. Please discuss these aspects in the manuscript.

*) It is customary to use the introduction to give a short layout of the rest of the paper. Please add this.

*) section 2: Data. Is the Sentinel-1 mission an initiative of the sole ESA? The Sentinels are part of the boarder Copernicus, which is an EU programme. A joint initiative of ESA and EU?

*) page 6940, lines 10-15. Mentions what is actually measured: radar backscatter sigma0.

*) same page, line 24: capitalize “Earth”.

*) page 6940 and 6941 The last paragraph were the Nansat tool is discussed and its handling of the GCPs is not part of “data”, and could be moved to “methodology”.

*) Same paragraph. It sounds like you did little pre-processing of the data. Did you applying calibration, de-banding, transformation to dB scale, etc. . . ? If yes, please specify these steps. If not, state clearly that the images are used as found in the product files.

*) Page 6941 line 12: “major” -> “main”

*) Same page, line 23: mention that the parameters are the “best” for your region of interest. This is elsewhere in the text. Your parameters are optimized for your region of interest. They are also optimized for winter/spring cases only.

*) Page 6943, line 4: 9 contiguous pixelS. Next line, fix typo in “recognized”.

*) Page 6943: while describing the algorithm, you seemingly use “keypoints” and “features” for the same thing. Are they the same things and if not can you add a sentence to state the difference?

*) Pages 6943 6944: the reader might be lost by your steady pace at describing the ORB. Could you add a figure/sketch showing the location of the 256 pairs designed by Rublee et al. 2011? Or are they at random? Could you also give an example of computing the Hamming distance of 2 features? What is the “ratio test from Lowe 2004” can it be described here in few sentences, including its purpose in your setting? Is it for discarding rogue vectors?

*) Page 6945: here some information about the ratio test. Please move this to the description of the method and only keep the discussion of the 0.75 value if still relevant.

*) Page 6945 lines 8-9: “, if the motion vectors are too different” (instead of “if the matches are not correlated”).

*) Page 6945 line 10: The reader can probably do the math, but it looks wrong to add km and km/h. Please fix the equation by introducing the time separation between the images.

*) Page 6945 line 15: “computationally”. Later: “best suitable” -> “optimized”

*) Page 6945: The RMSD as a quality measure is dubious as it would favour algorithms that smooth a true discontinuous motion field (shear, divergences,...) It seems you are using it as a gross error check to detect outliers more than a ranking. Maybe discuss this?

*) Page 6946 line 6: “velocities” are not in “km”. You could use “displacement” or “motion”.

*) Page 6946 “Patch size”. Is Patch Size the n from Eq 3? Please make sure the reader knows what you are varying and how it relates to the description of the algorithm.

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*) Section 4.1.2 : Refer the reader back to Eq 1. Are the values you find here expected to work during summer? Please briefly discuss the expected adequacy of your values to all-year-round (in this case: summer) applications. This discussion could be moved to Section 5 “Discussion and Outlook”.

*) Section 4.1.3 : It seems features are tracked at all pyramid levels. So adding a pyramid level logically brings more features. Is it what we see in Figure ?

*) section 4.1.4 : Again, please specify that the coefficients in table 2 are optimized for your region of interest.

*) section 4.2 : This is where this paper should try and give more insights as to why HV channel gives so many more vectors than the HH channel. Are they located at the same locations? A figure showing a (zoomed) Sentinel-1 SAR image and the location of HV and HH features is suggested (if it serves its purpose).

*) section 4.3 : It seems that the authors only tuned the parameters of the ORB algorithm, while the other two (SURF and SIFT) are used "out-of-the-box", without optimizing. If the case, it is not surprising to find better results for ORB (after optimization). Please modify the text (also abstract and conclusion) so that it is clear to the reader which algorithms were optimized (and why your choice is still ORB - for example license issues and computational efficiency).

*) section 4.5 validation. The RMSD to the manual vectors is not impressive (600m) considering that they are from the same images. Komarov and Barber (2014) find RMSD of about 400m against GPS drifters, and similar values are reported for the CMEMS/DTU Sentinel-1 based product. To be more conclusive, please load the CMEMS 10-km Sentinel-1 SAR product (from DTU) computed from this pair of Sentinel-1 images, and do the validation against the compute the RMSD to this product. The validation will then not suffer from the uncertainty due to manual matching.

*) page 6949: Discussion and Outlook. This section is too short and more discussions

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are needed for this manuscript to be more than a report about your investigations. In the revised manuscript, it should be extended with the discussion of the differing HV/HH performances, and the discussion of the distance between features compared to the size of the features (are the motion vectors from two neighbouring features independent?). This will then be a very good contribution to this journal.

*) Your Outlook is too vague. Rather state what your next steps will be in your investigations of Sentinel-1 sea ice drift.

*) Figures 1, 2, 3: it is puzzling to see the HH+HV (red) curve in-between the HH and HV (black) curves. Should not the red curve be the sum of the other two?

Interactive comment on The Cryosphere Discuss., 9, 6937, 2015.