

Interactive comment on “Virtual radar ice buoys – a method for measuring fine-scale dynamic properties of sea ice” by J. Karvonen

J. Karvonen

juha.karvonen@fmi.fi

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General comments:

This paper aims at developing the algorithm for estimating the ice drift from the successive radar images. For radar images, the author used the ship-borne radar and coastal radar as an example. To estimate the ice drift distribution, he selected the traceable objects (he calls them virtual buoys (VB's)) at some spatial intervals from the ice field using the method of local binary patterns, and then attempted to obtain the displacement of the individual objects using his algorithm. After applying this method for three cases (two: coastal radar, one: ship-borne radar), he concluded that this method is useful for obtaining the ice drift pattern and applicable to the estimation of dynamic properties of sea ice. I understand the importance of this topic and find that it will be

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useful to get the dynamical features of sea ice on a sub-grid scale in various regions on such an operational basis. And that must be what the author aims at in this paper. But the manuscript is a bit hard to follow at places to me and it would be possible to make it much more readable. I feel the English usage is not necessarily proper at places, so the proof-reading by native-English speaker might be helpful. Besides, an important thing is that the author should show more clearly what is a new concept and procedure in this method compared with the past ones, I think. My major concerns are as follows:

Dear reviewer,

Thank You for the comments, I have now updated the manuscripts trying to take the reviewer and discussion comments into account.

1) I feel that Abstract should be rewritten so as to show more clearly what is the purpose of this study in a wider sense and what is a new concept and finding in this method. Although the author described “ice dynamics estimation”, what he did is just to obtain the ice drift pattern. Thus I feel it would be better to replace “ice dynamics” by “ice drift pattern” throughout the manuscript and describe, say, “this result is applicable to estimate the dynamic features of the ice field such as divergence pattern or deformation”.

The abstract has been updated. I have changed ice dynamics to ice drift in most parts. However, parameters related to ice dynamics can be derived from the results of the algorithm, e.g. the area of VB triplets which can be used as a measure of local convergence or divergence has been computed for some sample cases in the manuscript. Computing shear and vorticity is also possible and rather straightforward.

2) Introduction might be too technically specific. If this study is intended for estimating the dynamical properties of sea ice, it should be stressed why the ice drift pattern on this spatial scale is needed to be clarified more. And the problems of the past techniques and how the author tried to improve them should be described more clearly.

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Introduction has been updated. The technical stuff on the radar has been moved to a section of its own.

3) Although the detailed weather conditions were described in section 3, there is no description about how they affected the experiments. It would be better to mention how the air temperature and wind are related with the result.

The section on experimental results has been updated and material on the relation to the weather conditions has been included.

4) In section 4, I could not see how this method is different from the past crosscorrelation method and why this method has a merit for detecting the edge and corner features of the ice field. It would be helpful to explain the detail of this method by comparing with the past cross-correlation method. Especially the explanation about how you solve the equation (8) would be needed to be more understandable because I think it might be the key of this method.

The advantage compared to Maximum cross-correlation (MCC) is that we inherently get sub-pixel resolution for the ice drift estimates. In MCC either an interpolated image (increasing the amount of computation) or non-linear interpolation in the CC-domain would be required for subpixel accuracy. this has been indicated in the manuscript. I tried to improve the description of the solution of eq. 8.

5) In section 5, overall I feel that the validation of this method is coarse. It would have been possible to estimate the error quantitatively by comparing the calculated ice drift speed and direction with the real ice displacement from the images. Another concern for the result obtained from the ship-borne radar is how they removed the ship motion. I think the removal of ship motion is vital to the estimation of real ice motion.

I have added a shortist subsection on estimation of the esitmaton errors. Direct comparison of visually estimated drift and algorithm results is very difficult because the accuracy of visual tracking is not very good (according to my experiance one pixel or

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more) and also requires a lot of work. I have included an alternative approach for roughly estimating the numeric error.

Specific comments:

*(P2L15-P3L2) I wonder if the range resolution might be affected by the sea ice topography. In the ridged ice field, it might become hard to detect the characteristic ice features from the slanted view. Does this effect not affect a significant influence on this method?

*(P3L9) What is satellite-borne EO data?

Satellite-borne earth observation data, EO has now been replaced with "earth observation".

*(P3L24) "temporal differences" should be "time intervals".

Changed.

*(P4L1) Please explain more about "Optical flow algorithm".

This has been explained in the subsection on optical flow but I added a sentence and a reference to the the section here.

*(P4L9-10) Please name "25 February 2011 from 03:00 to 16:58 UTC" Case A, "8 February 2012 from 00:00 to 23:59 UTC" Case B, and "21 January 2015, 11:30 UTC to 18 February 2015, 10:20 UTC" Case C. Then the manuscript would become more concise.

The suggested naming convention has now been used.

*(P6L10) Please explain more explicitly about how the author initialized the VB's based on ALBP's.

This is explained in Section on 4.1. A reference to this section has been added.

*(P7 Eq.2) Please explain what "x" stand for.

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x is just the argument of the given function.

*(P10Eq.11) In the successive images the intensity would deviate from the first image due to the ice motion. Therefore I wonder if combining the successive images in this way might reduce the accuracy. Please explain your intention about this.

This is the standard way of estimating the partial derivatives in optical flow. According to my experience the method is very accurate. It has been used in many (especially optical) application successfully.

*(P11L18) Please delete “of the radar indicated by the green dot and”.

Removed.

*(P12L2-3) Please change to, say, “However, just from trajectory plots the ice drift velocity is not shown as a function of time.”

Changed.

*(P12L7-8) Please replace “different parameters related to the ice dynamics” by other explicit words such as ice drift.

Replaced.

*(P13L16) “temporal difference” would be “time interval”.

Changed.

*(P14 first paragraph) It is not understandable to me. Please rewrite it so as to show the purpose and concept of this study more clearly. “information on the nature of the ice dynamics” should be replaced by more explicit words.

*(Fig.3-5) Please show the land area in the individual figures to show the ice area clearly.

Land masks added.

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*(Fig.8) Please write the unit of time step in the caption.

Changed from time steps to minutes, units have been indicated.

Sincerely, Juha Karvonen, FMI

Please also note the supplement to this comment:

<http://www.the-cryosphere-discuss.net/9/C2022/2015/tcd-9-C2022-2015-supplement.pdf>

Interactive comment on The Cryosphere Discuss., 9, 4701, 2015.

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