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Interactive comment on "On producing sea ice deformation dataset from SAR-derived sea ice motion" *by* S. Bouillon and P. Rampal

Anonymous Referee #2

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The Radarsat Geophysical Processor System (RGPS) uses synthetic aperture radar (SAR) satellite images to track thousands of grid points on the sea-ice cover of the Arctic Ocean, with approximately 10-km spacing. From the resulting ice motion fields, the spatial derivatives of the ice motion can be computed, and from them the deformation of the ice cover (divergence and shear). The authors of this paper contend that the straightforward calculation of the deformation in this way contains "artificial noise" that arises from discontinuities in the ice motion field. Depending on the angle of the discontinuity (or lead, or crack) with respect to the geometry of the grid points, some of the grid cells along the discontinuity may experience "opening" (divergence) while other grid cells experience "closing" (convergence). This is deemed to be unphysical, and in need of correction. The authors propose a smoothing scheme in which the spatial derivatives of the ice motion in the grid cells along the discontinuity are averaged

together before the deformation invariants are calculated. The authors "validate" their method by showing that the moments of the deformation vs. the spatial scale over which they are calculated approximate power laws more closely for the "filtered" fields than for the unfiltered fields.

General Comments

In general the paper is clearly written and will be suitable for publication with minor revisions.

My main comment is: how do the authors know that the observed openings and closings along a lead are actually "noise" and not "signal"? In the real world, a lead is not necessarily a perfectly straight line with smooth sides. Little "jogs" along the main direction of the lead will give rise to openings and closings even under pure shear deformation. These little jogs cannot necessarily be resolved by the 10-km spacing of the grid points.

Consider, for example, the situation in Figure 1(a). Suppose the crack, depicted as a thick black line, actually has a small "jog" in it – a section of length W = 0.01 perpendicular to the main direction of the crack. (Note that the spacing of the points is about 10*W). The points above the crack have a relative displacement parallel to the crack of U = 0.01. This gives rise to an unmeasured opening (or closing) of area U*W within the triangle where the jog occurs. Since there are about 200 triangles in the figure, each one has an area of about 0.005. Therefore the relative area change (divergence) due to the jog would be U*W/0.005 = 0.02, which is roughly the magnitude of many of the red triangles along the crack. The point is: small unresolved jogs along the length of a crack will in fact give rise to small amounts of opening and closing under pure sliding deformation. In an idealized situation where the crack is assumed to be a straight line, the openings and closings as in Figure 1(a) are certainly noise. But in a real-world situation, it is not necessarily unphysical to have openings and closings along a crack.

Consider another case where a horizontal crack with a small vertical jog passes through the middle of a square cell, and the ice slides along the direction of the crack. As above, this gives rise to an actual opening (or closing) within the cell, but the shape of the cell changes from a square to a rhombus with no change in area. In this case, the unfiltered ice motion field produces an UNDERestimate of the true opening. The point is: the unfiltered fields can give rise to UNDERestimates of the true divergence as well as OVERestimates.

The only way to really figure out what's going on is to go back to the original SAR imagery and track more points along the boundaries of the cells, in order to get better representations of the true material boundaries of the ice. But I am not suggesting that the authors need to do this – it would be far too much work.

What the authors have actually done is to show that their filtered fields lead to better agreement with power-law scaling behavior than the unfiltered fields. This is not exactly "validation", it's really a consistency check with the assumption that the deformation should have power-law scaling. Real validation would involve finer-scale ice tracking (as noted above) or an independent data set. However, the consistency check does lend credibility to the authors' method of filtering the data.

In summary, my main comments are: (1) the "artificial noise" may actually contain some valid "signal" in real-world situations, and (2) the "validation" is really a consistency check. The authors need not re-do any of their analysis, but I think they should acknowledge these points.

Specific Comments

Page 5106 line 17. Cross correlation and feature tracking in SAR images go back decades to the work of Fily and Rothrock (JGR-Oceans 1990) and Kwok et al (IEEE J Ocean Engr 1990).

Page 5106 line 20. A better reference for RGPS is: Kwok, R., The RADARSAT Geophysical Processor System. in Analysis of SAR data of the Polar Oceans: Recent

Advances, Tsatsoulis, C. and R. Kwok, Eds.: 235-257, Springer Verlag, 1998.

Page 5106 lines 20-21. "Central Arctic" should be changed to "Western Arctic". Look at the coverage map in Figure 7.

Page 5108, Method. The method involves a triangulation of a set of tracked points. The deformation is calculated for each triangle. Note that a triangle is the least accurate shape one can possibly use for calculating sea ice deformation. The problem is this: in estimating the deformation of a region using a discrete set of boundary points, the implicit assumption is that the points adequately resolve the material boundary of the region. In other words, as the shape evolves over time, there should not be a flux of ice into or out of the region. But the sides of a triangle will almost certainly not be material boundaries unless all three vertexes are on the same rigid piece of ice (in which case the deformation is zero). The more points that are used to define the boundary, the more accurate the estimate of the deformation becomes. Thorndike (Kinematics of Sea Ice, Chapter 7 in The Geophysics of Sea Ice, NATO ASI Series, vol 146, 1986) found that the ratio of estimation error variance to signal variance is about 0.7 when using 3 points to estimate divergence (see Fig 23b and the discussion at the top of page 536). This ratio drops significantly for 4 points and 5 points. As the authors point out later, triangles give the best spatial resolution, but it should be noted that they also give the worst accuracy. However, the filtering scheme used by the authors effectively increases the number of boundary points that define the material element containing a crack, thereby improving the accuracy of the deformation estimate in that element. When the spatial ice motion derivatives within two or more adjacent cells (triangles) are averaged together, the contributions from the internal cell boundaries cancel one another, leaving only the contributions from the external cell boundaries. This effectively creates one large cell in which the derivatives are the same as if they had been calculated by a contour integral around the outer boundary, as in equations 1-4. Thus the cell-by-cell averaging can also be viewed as a way to combine the cells into one larger cell for which the material boundary is defined by many points. I think this is worth noting.

Page 5110 and following. The letter "n" is used to mean 4 different things: - The number of vertexes of a grid cell. See page 5110 line 3 (n=3) - A subscript on epsilon to indicate divergence. See page 5110 equation 7. - A subscript on u to indicate the velocity component normal to the crack. See page 5110 line 10. - The number of triangle edges that are crossed in order to construct the smoothing kernel. See page 5110 line 25 (n=7). It is possible that the reader may become confused about the multiple uses of "n".

Page 5111, line 12 and following. The 2 parameters in the filtering method are the deformation threshold and the size of the kernel. "the threshold value is chosen to be small enough to select all the deforming cells" (lines 14-15). That's all we're told (here) about how to select the threshold. Later, at the top of page 5114, the authors explain more about the choice of threshold. It would be helpful to say on page 5111 that more details about choosing the threshold are presented later.

Page 5113 lines 18-19. Yes, using triangles instead of quadrilaterals increases the number of deformation estimates and increases the resolution, but it decreases the accuracy of the estimates. See the previous comments about "Method".

Page 5113 line 29. I don't understand what this means: "while keeping an important weight for the shear deformation"

Page 5114 lines 9-16. I don't understand the quality index – neither how it's defined nor how it's used.

Page 5116 lines 8-9. The authors state that the scaling analysis is very sensitive to the presence of noise in the analyzed field. But the scaling analysis is done with the MEAN deformation, which is the result of averaging over many values, which greatly reduces the noise. See line 23: the mean value <epsilon-L> is used. See also Figure 10: the huge cloud of points at each spatial scale is averaged to produce a single mean value

(the black circle), which surely is not very sensitive to noise in the analyzed field, even at the 200-km scale, where the sample size is still reasonably large.

Page 5117 lines 3-4. "The artificial noise particularly induces a strong departure from the power-law model at the smallest scales (see Fig. 10)" I don't see the strong departure. In Fig 10 (left panel, unfiltered), at the smallest scales, it looks to me like the dotted line (power law model) is quite close to the black circles (mean deformation).

Page 5117 lines 11-18. My understanding is that the authors are trying to find the set of method parameters that gives the best linear relationship between deformation and spatial scale (in log-log space). This could be done using standard least-squares fits, with standard measures of the goodness-of-fit such as the squared correlation Rˆ2, for each set of method parameters. Instead, the authors invent their own procedure for finding the best set of parameters, by calculating slopes for each successive pair of spatial scales and then using max(slope)-min(slope) as the "error" to be minimized. Why not use a standard method like least squares? The authors' method would appear to be very sensitive to outliers. Can they give assurances that their method is "reasonable", or cite a reference for it?

Technical Corrections

Title of paper. The word "dataset" should be plural: datasets. Also, the editors of the journal need to decide whether "datasets" is in fact one word or whether it should be "data sets". I use two words, but the journal might have a different convention.

Page 5106 line 26. Kwok and Stern 1995 is actually Kwok, Rothrock, Cunningham, and Stern 1995.

Page 5112 line 21. "position" should be plural: positions.

Page 5112 line 24. Put the word "negative" before "y axis": the negative y axis is aligned with the 45W meridian.

Page 5113 line2. "first ... secondly" should probably be "first ... second"

Page 5115 lines 8 and 9. Maybe the word "large" should be "long"?

Page 5118 line 8. The scaling exponents are "systematically higher". The word "higher" is ambiguous because the quantities in question are negative. "higher" could mean "higher in magnitude" (more negative) or "higher in value" (less negative). I'd suggest either "larger in magnitude" or "more negative".

Page 5119 line 7. "drops" should be "drop"

Page 5119 line 10. "cumulated" should be "cumulative"

Page 5120 line 13. "buoys trajectories" should be "buoy trajectories"

Figure 10. What do the colors mean? They should be explained in the caption.

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