Response to reviewer #2

We first would like to sincerely thank the reviewer for his throughout review and his numerous useful comments that, we believe, contribute to significantly improve our manuscript. Each answer/comment of the reviewer below is followed by a "response" and when applicable by a more specific description of the "change(s)" we made to the manuscript.

General comments

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. 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.

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.

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.

<u>Response to main comment (1):</u>
Sea ice deformation is defined at a given scale. The deformation computed at the scale of the data (10 km) is therefore not the same as the one computed at lower scales. The aim of this paper is to increase the accuracy of the deformation fields computed at the resolution of the

data (e.g., at 10 km when using the SAR-derived ice drift like in the RGPS data). We think that the term "true divergence" is ambiguous, as it does not refer to a specific spatial scale. We think that the remark of reviewer could be reformulated as: "What valid information can we derive from the deformation at 10 km on the deformation at smaller scales, for example at the field scale, i.e. ~1m?". The answer would be that it is possible to extrapolate the distribution of the deformation at smaller scales by using the information retrieved from the multifractal scaling analysis. It has been done in Marsan et al. 2004 to determine the distribution of the total deformation at a spatial scale of 1m. We have not done this extrapolation in the present paper but it would bring valuable information on the opening/closing (in other words, the absolute divergence) experienced by sea ice at smaller scales. It should be noted that we provide a more accurate estimate of the power-law exponents of the spatial scaling as the corresponding uncertainty associated to the power law fit significantly decreases after removing the noise following the presented method. As a result, the extrapolation at smaller scales would be more accurate. However, as we have not yet investigated this idea, we will not include any statement on the extrapolation to smaller scales in the paper.

Once this ambiguity about the definition of the deformation is cleared, we can answer to the specific remarks of the reviewer:
In the "real-world" the "little jogs" described by the reviewer are observed on a wide range of scales (see the description of the secondary cracks in Schulson (2004)). The presence of these features at all scales is partly responsible for the spatial scaling of the deformation. However, as explained by the reviewer, the resolution of the data limits the representation of these features. A dataset having a resolution of 10 km is only able to "see" "little jogs" longer than 10 km. In consequence, the deformation values, but also integrated quantities , for example the total opening/closing, will not be the same when computed at a resolution of 10km or at higher resolution.

The reviewer suggests using data at higher resolution, but, as shown in section 2.1 with the idealized test cases, the opening/closing error generated by the artificial noise does not depend on the resolution. In the idealized test cases, unfiltered deformation fields at higher resolution have the same error as unfiltered deformation fields at lower resolution. To apply the suggestion of using higher resolution, one should therefore also filter the deformation computed from high-resolution data. The suggestion of using independent dataset has been added to the conclusion. We are already working on that topic.

Concerning the example with a "little jog" added to the single crack experiment, we agree on the figures computed by the reviewer. As explained by the reviewer, the presence of a "little jog" "… gives rise to an unmeasured opening (or closing) … within the triangle where the jog occurs". The unmeasured opening or closing is then of the same order of magnitude as the artificial opening/closing in the unfiltered field. Therefore, in the worst case, this unmeasured opening or closing may double the local error in one cell. However, in other cases, it could also reduce the local error when the artificial and unresolved opening/closing has the same magnitude but opposite sign. On average, it will then likely have no impact on the total opening/closing error obtained from a large set of experiments.

Concerning the second example with a unique triangle in which a "little jog", we also agree with the description made by the reviewer but we think that his conclusion: "In this case, the unfiltered ice motion field produces an UNDERestimate of the true opening." is ambiguous. First of all, this sentence is only true when looking at one specific triangle. When looking at the total opening/closing over the square domain, the local underestimation of the opening is much lower (one order of magnitude) than the total overestimation of the total opening/closing generated by the artificial noise. Secondly, this analysis is only true for a cell having a specific orientation relative to the crack (a triangle or a square with an edge parallel to the crack). When looking at a large number of experiments (with random orientation of the cracks and of the direction of the little jog), the opening/closing in the triangle with the "little jog" will equivalently be OVER and UNDERestimated. The median error will likely not be affected by the presence of a little jog. **The conclusion of the reviewer: "the unfiltered fields can give rise to UNDERestimates of the true divergence as well as OVERestimates" only**

reflects the presence of noise in the unfiltered deformation field. It does not apply to the constant overestimation of the opening and closing rate caused by the artificial noise. We think that our definition of the error is clear enough, and does not necessitate any change. We always compute the error over the analyzed domain and not the local error that is actually equivalent to noise.

Concerning the suggestion of the reviewer, we cannot agree to say that: “artificial noise” may actually contain some valid “signal” in real-world situations. In particular, we think that the “artificial noise” perturbs the “signal” so strongly that it actually makes it impossible to retrieve valid information about the deformation experienced by the sea ice. We showed that the “artificial noise” generates an overestimation of the deformation at 10 km and an overestimation of the scaling exponents. This deviation from the power-law scaling when the spatial scale reaches the data resolution clearly suggests that this noise is artificial and not linked to real properties of sea ice deformation.

Changes for main comment (1):
We add to the conclusion:
“A complete validation using independent datasets should also be done.”

Response to main comment (2):
We agree.

Changes for main comment (2):
As suggested by the reviewer, we renamed the section “Validation” into “Consistency check” .

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).
Response: Yes, we agree.
Changes: We have added those references.

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.
Response: Yes, we agree.
Changes: We now use this reference.

Page 5106 lines 20-21. "Central Arctic" should be changed to "Western Arctic". Look at the coverage map in Figure 7.
Response: Yes, we agree.
Changes: It has been corrected.

Page 5108, 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.

Response :
We agree with the reviewer that the problem partly concerns the identification of individual objects or of the boundaries between individual objects. As stated in the conclusion, the inclusion in tracking algorithms of strategies to detect discontinuities (as in Thomas (2008)) could solve this problem by providing much better definition of these boundaries.
We appreciate that reviewer fully understand the spirit of our method which is actually a combination of a detection method and a smoothing method.

Concerning the error introduced by using triangles, please read our answer to General comment 1 of reviewer #1.

Changes: No change linked to this remark.

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".
Response: Yes, we agree.
Changes: The number of vertexes is now indicated by letter “m”. The subscripts for epsilon are now “div” and “shear”. “n” is kept for the subscript on the normal displacement. Italic “*n*” is kept for the number of triangles that can be crossed to create the smoothing kernel.

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.
Response: Yes, we agree.
Changes: We add this sentence: “For application to real data, the choice of this parameter is critical and is detailed in Section 2.2.”

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".

Response: We agree that the uncertainty (here defined as the opening/closing error) is slightly higher with triangles than with quadrangles. It is now explicitly shown in Section 2.1.

However, using triangles has another great advantage. Triangulations can be applied to any set of points, which is not the case for quadrangulations.

Changes: We now have a full paragraph to justify the choice of using triangular meshes: “Using triangles instead of quadrangles roughly doubles the number of deformation estimates, and increases the resolution of the deformation product up to 7 km. Another advantage is that triangulations can be made on any set of points (if they are not all aligned), which is not the case with quadrangulation (Bremner, 2001). If the tracked points are given on a regular grid, quadrangulation could be easily performed and could be preferred to start with a reduced uncertainty on the initial unfiltered fields. However for most of the available datasets (for example GlobICE and EGPS), the data are not given on a grid but as a list of points. The method presented here based on triangles is then very flexible and can be applied to many different sources of data. In the next section, the unfiltered and filtered deformation fields obtained on triangular meshes are compared to the RGPS deformation fields. The smoothing procedure is not applied to the RGPS deformation fields because it requires to know the neighbors of each cell and this information is not present in the RGPS Lagrangian deformation dataset.”

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

Response: We agree that the end of the sentence was not clear.

Changes: As this part of the sentence was also not necessary, we removed it.

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

Response: We now specifically introduce a notation for the size of the kernel and we explain with the test-cases how we determine that the size of the kernel IK_sl could be as low as n+1 for single cracks when the center of the kernel is at the boundary of the mesh and as high as 4n+1 for two intersecting cracks. This is now explained in the text and we also add several sentences to explain how we use this quality index to determine the reference value for the threshold parameter.

Changes: Here are the sentences added to the text:
“Based on this quality index, the threshold values 0.01 and 0.02 per days are the best. The value of 0.02 per day is chosen as the reference value for the deformation threshold. To quantify the range of the quality index obtained with this reference value, we look at the percentage of pairs of images for the entire winter 2006-2007 for which the quality index is lower than 50% and we found that only 14\% of the pairs of images have a quality index lower than 50\%. To further validate the choice of the model parameters, a consistency check based on a multi-fractal scaling analysis of the deformation fields is proposed in Sect. 3.”

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.

Response: We agree that our statement was too general to be always true. We change it into a more specific description of how the noise in the deformation field modifies the distributions of absolute divergence and shear, and how it impacts the spatial scaling. We also specifically indicate that it is the absolute divergence on which the scaling analysis is performed. This point was maybe not stressed enough and this could explain the question raised by the reviewer.

Changes: “The spurious noise in the deformation fields corresponds to high values of deformation and is potentially present for any active linear kinematic features. This noise may then impact the distributions of absolute divergence and shear and modify their mean (1st-order moment) but even more their standard deviation (2nd-order moment) and skewness (3rd-order moment). Moreover this noise is the highest at the resolution of the data but rapidly decreases for larger spatial scales. We then expect that the presence of noise in the deformation fields will have a strong impact on the result of the scaling analysis, especially for the smallest scales and the highest-order moments of the distribution.”

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).

Response: For the filtered data all the circles from 7 to 200 km are well aligned with the dashed line and this is clearly not the case for the unfiltered data. We agree that some of the black circles for the unfiltered data are close to the dotted line but not all of them, meaning that the unfiltered data do not follow the power-law model.

Changes:
We now more precisely describe the results of the scaling analysis for the filtered and unfiltered data. The following text has been added and is followed by the description of how the scaling of the unfiltered data deviates from the power-law scaling for the scales smaller than 50 km.
“The filtered shear and absolute divergence closely follows the power-law model for the spatial scaling as their first order moments are well aligned with the power-law fit for the spatial scales ranging from 7 to 200 km (see right panel of Fig. 10 for the absolute divergence). This is not the case for the unfiltered deformation fields (see left panel of Fig.10) and we explain this strong departure from the power-law model by the presence of artificial noise.”

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?

Response: The choice of the method (i.e., the definition of the error by the min-max error) is motivated by the fact that we want to quantify the deviation from the power-law scaling. By definition, if a scaling holds for a given range of scales, it should be respected for any pair of scales within this range. We perfectly understand that our measure of the error is very sensitive to “outliers” and this is precisely what we want. We want to be sure that when we define a power-law exponent for the scaling over a given range of spatial scales, this exponent will be the same (or very close) for any pairs of spatial scale within this range. This information will not be given by squared correlation. It should be noted that the term “outliers” is here applied to the mean values of the shear or absolute divergence (black circles). These mean values are obtained as the average of a very large number of points, meaning that any small deviation from the power-law scaling cannot be caused by only a few outliers in the deformation datasets but by the presence of a large number of artificially high values of deformation.

Changes: We add a new paragraph in this section to present the definition of our error bars: “By definition, if a scaling holds for a given range of scales, it should be respected for any pair of scales within this range. To evaluate the deviation from the power law scaling, we compute the power-law exponents for each pair of successive spatial scales (i.e. from 7 to 14 km, from 14 to 25 km, and so on) and we take the minimum and maximum values of these exponents. Those values as well as the exponents previously obtained with the whole range from 7 to 20 km are reported as a function of the moment order q in Fig. 12. The relationship between the power-law exponents and the moment order q is called the structure function beta(q) and is defined by $<\dot{\epsilon}_L^q> L^{-\beta(q)}$. The minimum and maximum exponents define the bars around beta(q).”

See also the response to the question 5117-18 of reviewer #1 on the choice of the model parameters.

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.
Response: We agree.
Changes: We now use the plural in the title. We leave the editor decide if we should use the term “data set” instead of “dataset”.

Page 5106 line 26. Kwok and Stern 1995 is actually Kwok, Rothrock, Cunningham, and Stern 1995.
Response: Thank you.
Changes: Reference corrected.

Page 5112 line 21. "position" should be plural: positions.
Response: Thank you.
Changes: It has been corrected.

Page 5112 line 24. Put the word "negative" before "y axis": the negative y axis is aligned with the 45W meridian.
Response: Thank you.
Changes: It has been corrected.

Page 5113 line2. "first ... secondly" should probably be "first ... second"
Response: Thank you.
Changes: This sentence has been modified and does not contains these terms anymore.

Page 5115 lines 8 and 9. Maybe the word "large" should be "long"?
Response: Thank you.
Changes: It has been corrected.

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".
Response: Thank you.
Changes: It has been corrected, we use now “larger in magnitude.

Page 5119 line 7. "drops" should be "drop"
Response: Thank you.
Changes: It has been corrected.

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

Response: Thank you.
Changes: It has been corrected.

Page 5120 line 13. "buoys trajectories" should be "buoy trajectories"
Response: Thank you.
Changes: It has been corrected.

Figure 10. What do the colors mean? They should be explained in the caption.
Response: Each color corresponds to a different box size used for the coarse graining procedure.
Changes: It is now indicates in the caption.