

## ***Interactive comment on “Characterizing sudden changes in Arctic sea ice drift and deformation on synoptic timescales” by Jennifer V. Lukovich et al.***

**Jennifer V. Lukovich et al.**

Jennifer.Lukovich@umanitoba.ca

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Please find below a list of responses to reviewers' comments regarding the submitted manuscript, “Characterizing sudden changes in Arctic sea ice drift and deformation on synoptic timescales”, by J.V. Lukovich, C.A. Geiger, and D.G. Barber. General comments are presented, followed by responses to reviewers' specific comments and suggestions. Please note that changes implemented may also be found in the attached supplementary material in response to reviewers' suggestions.

### **General comments:**

The authors would like to thank both reviewers for their constructive comments and suggestions. In consideration of reviewer recommendations, and the initial motivation for the triplet analysis, the authors have in the revised manuscript presented a frame-

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work based on Lagrangian dispersion, and single-, two-, and three-particle dispersion statistics in particular to provide a quantitative analysis and more focused narrative of sudden changes in ice drift paths as well as associated changes in sea ice deformation, based on distance from the coastline.

Please find below specific comments to suggestions and recommendations.

### **Specific comments:**

Anonymous Referee #1 Received and published: 31 October 2016

General comments: This paper investigated the deformation processes of sea ice in the southern Beaufort Sea from the analysis of several buoy data with special attention to the sudden changes in sea ice drift and its relevance to atmospheric forcing, ice conditions, and the effect of shore. The goal of this study is placed in developing a framework for understanding sudden changes in ice drift trajectories. For this purpose, firstly the authors set 4 triplet areas composed of three buoys for each, and then traced the temporal evolution of each triple. As a result of analysis during the period from September to November in 2009, they detected eight “sudden change” events and examined the kinetic deformation parameters in relevance to the atmospheric forcing, ice conditions, and the effect of shore. They used triplet area, perimeter-to-area, the Okubo-Weiss criterion as diagnostic parameters. From their analysis, they concluded that sudden change occurred reflecting sea ice deformation, associated with the transition of atmospheric forcing and the interaction with the coastal line, and so on. I understand the importance of this topic and find that it may be useful to understand the dynamical features of sea ice near the shore on a daily time scale and on a sub-grid spatial scale. And that must be what the author aims at in this paper. I see that the authors attempted to make the most of available several buoy data to reveal them, and their efforts should be appreciated. However, I feel the manuscript is a bit more descriptive and the conclusion does not necessarily seem clear. My opinion is that the paper would be improved much if the author show more clearly what is a new

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finding of this study in the context of the research history on sea ice dynamics. Thus my evaluation is somewhat reserved at this stage. My major concerns are as follows:

1) Discussion and conclusion seems to me a bit qualitative. I mean that to correlate the sudden change events with the change in atmospheric forcing or the interaction with coastal line, it would be helpful to show how much forcing (e.g. change in wind speed, or shear of sea ice drift) was needed and examine if the result can be explained in the present framework of sea ice dynamics. It might be difficult to draw quantitative conclusion just from the available datasets. Even so, it would be possible to make additional figures which explain the thought of the authors more clearly, such as scatter plots as a function of the distance from the coast or a schematic picture. The figures in the present manuscript are only time series of physical parameters. It is not necessarily easy to understand the essence of this paper just from the time series.

Thank you for your comments and suggestions. The authors agree that the initial version of the manuscript was qualitative, and have addressed this through investigation of single-, two-, and three-particle dispersion statistics. Since the initial motivation of this manuscript was to develop a diagnostic that built upon previous Lagrangian dispersion statistical analyses, it was decided that investigation of each using this dataset would provide a comprehensive framework with which to assess the regional-scale changes in sea ice drift (single-particle; Figures 3 and 4 in supplementary material), deformation (two-particle; Figure 9 in supplementary material), and deformation components (three-particle) using the available data, and as motivation for future observational/modeling studies focused on sea ice drift and deformation. In light of the present reviewer's comments, the authors have also included analyses that capture relative contributions of wind and deformation using scatter plots, as suggested, based on distance from coastline, in order to provide additional insight into sea ice dynamics in the Beaufort Sea region based on distance from the coastline (Figure 15 in supplementary material). This work further builds upon oceanographic studies illustrating the benefits of Lagrangian dispersion statistics in capturing structure in the oceanic (flow) field,

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through investigation of single-, two-, and three-particle dispersion in the context of changing sea ice and atmospheric conditions.

2) Sorry, but I am a bit skeptical about the analytical method of deformation parameters using triplets. Although I agree that this method would be useful if the side lengths of the triplet are of almost similar magnitude, I feel it is questionable if the triplet becomes so distorted that the lengths of base and height have significantly different magnitude, as shown in Figure 6a. This is because the divergence or deformation parameters would take different values, depending on the horizontal scales. In such situation as the side lengths of the triplet are different by more than one-order, I wonder if the obtained values are representative of the region and therefore this method is really applicable. If the authors are convinced about this matter, it would be helpful to add some explanation.

The triangle base in the present analysis is defined as the triangle side with the longest length, in keeping with past studies that have characterized evolution in the triangle configuration (LaCasce, 2008). In a comparison of the least squares method derived according to (local) spatial derivatives in particle velocities, and the triangle area rate of change approach (Molinari and Kirwan, 1975; LaCasce, 2008), it was shown that both methods yield comparable results for drifters separated by distances of similar magnitude. Both methods were also shown to agree for large spatial gradients in the flow field (Molinari and Kirwan, 1975). As is described in both studies, the least squares approach resembles a Taylor series expansion about a centre of mass for a finite fluid element, and is applicable at local scales, namely when the distances between drifters/beacons are of comparable size. It was also shown that uncertainty associated with the least squares approach is reduced with an increase in the number of particles considered. A description of the triplet area rate of change approach in Saucier (1955) highlights that the scale factor associated with map projections cancels since the triangle area exists in the numerator and denominator of the area-weighted approach for calculating DKPs. Similarly, distortion in the triangle area is captured in the numerator

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and denominator so that it is the time rate of change in area that is captured by the DKPs. Diagnostics such as the aspect ratio and Okubo-Weiss criterion are also used to demonstrate relative DKP contributions based on distance from the coastline.

Additional clarification of this approach is provided in the revised manuscript and supplementary material, and methods section in particular, in lines 6 to 11 on page 11, as follows:

“Previous studies of DKPs using the triangle area approach have shown that the role of triplet areas in describing DKPs resides in the evolution in the time rate of change in the triangle area (Saucier, 1955; Molinari and Kirwan, 1975). If the lengths of the base (defined as the longest triangle side) and height (defined as the perpendicular distance and  $2A/b$ ) differ by an order of magnitude so that the triangle is significantly distorted, a decrease in area will occur. If in addition the change in area exceeds its uncertainty, the DKP associated with the relevant rotation of coordinates will increase, providing a signature of strong deformation. If, however, little change in triplet area is observed (less than the area uncertainty  $\sim 0.12 \text{ km}^2$ ), the DKP in question will essentially vanish. In the present study, as is noted below, minimum values for the triplet area amongst all triplets are on the order of  $10 \text{ km}^2$ .”

3) About the terminology. Several expressions about “sudden change” events might be a bit confusing. The authors used “sudden changes”, “shear shock events”, or “shock-response” and whatever for the similar events. Do they mean the same phenomena? If so, it would make the manuscript more readable to unify the expression after defining them at the beginning.

The authors agree that multiple terms for the same expression detracted from the narrative in the original version of the manuscript. “Sudden changes” in the initial manuscript referred to minima in the centroid variances as a signature of interruptions to the ice drift path. By contrast, the “shear shock event” referred to a specific event in October characterized by strong shear, following which a loss of coherence

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in ocean-sea-ice-atmosphere interactions occurred. Finally, “shock-response” referred to sea ice drift response to atmospheric forcing. The authors have attempted in the revised manuscript to ensure consistency in the terminology referring to shock events in the context of sea ice drift and deformation in response to atmospheric forcing.

In the revised version of the manuscript, we define sudden changes at the beginning of the manuscript as changes in the ice drift path (p. 2, line 20), and quantify sudden changes in terms of minima in ice drift variance and inflection points in single-particle dispersion.. Reference is now made to the 8 October 2008 SLP high that contributed to Ekman convergence in the sea ice cover, rather than the shear-shock event, since the SLP event reflects not only the emergence of shear in triplet B, and in triplet C two days later, but also the shift in sea ice dynamical regimes. In addition the 8 October SLP high depicts a transition in the dynamical state of the ice cover, as captured in single-particle dispersion statistics (Figure 3). Furthermore, the authors have removed reference to “shock-response” mechanisms in order to focus the content and improve clarity of the manuscript.

4) I would like to know more about the motivation of investigating the “sudden change” events. I also consider that this is a very important issue in the sea ice dynamics because it can induce the crack or formation of leads which would affect a large scale dynamics of sea ice area. So it would make the paper more impressive if the authors show some pictures which show how cracks were formed associated with the “sudden change” events. Personally, I think it might be interesting to discuss it from aspect of the yielding mechanics, namely the transition from viscous to plastic behavior in the VP rheology in the numerical sea ice model. To do so, separate the events which appeared in the persistent atmospheric forcing from those that occurred corresponding to the change in the atmospheric forcing. This might be one idea to the manuscript more quantitative.

The investigation of sudden change events is motivated by the development of a Lagrangian framework that can be used in both observational and modeling studies to

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characterize sea ice drift and deformation over a range of spatial and temporal scales. Sudden change events provide a means to characterize and quantify changes in the ice drift paths and associated deformation in response to atmospheric forcing and based on distance from the coastline, relevant for ice hazard detection and contaminant transport. In light of the both reviewers' comments and in order to provide a more quantitative analysis, the authors have described sudden change events in the context of single-, two-, and three-particle dispersion. In assessing sea ice drift using single-particle dispersion, a shift in dynamical regimes associated with the SLP high on 8 October was observed, and the poleward retreat in the sea ice edge due to convergence in the ice cover in response to the SLP high is presented in Figure 8 of the revised manuscript showing Canadian Ice Service ice charts prior to, during, and following the 8 October SLP high event.

Thank you also for the suggestion of focusing on yielding mechanics – the authors have attempted to address this through an assessment of the shear-to-divergence ratios as a signature of relative contributions in the yield curve. Although the derivation and assessment of yield curves is beyond the scope of the present study, the authors provide a preliminary assessment of shear to divergence ratios during persistent northeasterly wind events (e2, e5, and e7), where along- and cross-shear transport as captured by two-particle dispersion gives rise to local compression and convergence in the ice cover, evident in S/D values on the order of 180.

Specific points: \*(P3L24) “(Kwok, 2006)” is missing in the reference lists.

Thank you for this suggestion. This citation is now included in the reference section.

\*(P9L6) “Figure 2b” seems missing.

This is now corrected.

\*(P11L5) “Results show reduced total deformation with increasing distance from the coastal line (figure 8)” For me it is not so clear just from Fig.8. Especially the difference

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between A and B cannot be explained so well.

This statement has been modified to describe enhanced deformation in triplet B near the ice edge following the SLP high, as captured by the total deformation  $\sqrt{(D^2+N^2+S^2)}$ .

\*(P11L9-10) “Noteworthy is the existence of vorticity-dominated flow. . .” Please explain why this is noteworthy.

This statement has been removed from the revised manuscript as it does not contribute to its content.

\*(P12L19) “indicating the impact of ice interactions with coastline” Please explain more about the reason.

This statement was initially intended to demonstrate that coherence in turning angles occurs within a high ice concentration regime. However, the authors agree that high turning angles for all triplets do not indicate the impact of interactions with the coastline, and have thus removed this text from the revised manuscript.

\*(P13L28-29) “a continued increase in temperature. . .” I wonder that the reason for this interpretation is not enough because other factors such as the change in synoptic atmospheric circulation might have affected the temperature. Please add some more explanation.

The authors agree with the present reviewer that factors other than a crack in the ice cover may be responsible for increasing surface air temperature in the vicinity of triplets B, C, and D (Figure 7). The local SAT maximum does coincide with a SLP minimum, which could have advected warm air into the region from lower latitudes. Local warming may also be connected to coastal upwelling events associated with the SLP high, Ekman convergence, downwelling in the central basin and corresponding upwelling near the coast. In the absence of a more rigorous analysis explaining the physical mechanisms responsible for this particular feature, which is beyond the scope

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of the present study, this statement is also removed in the revised manuscript.

\*(P14L5) "Noteworthy also is increasing SAT" Please specify the period of this phenomena.

The statement is now expressed as "Noteworthy also is increasing SAT in the vicinity of triplet B and to a lesser extent C, relative to triplet D in early November."

\*(P16L13) How did you estimate "ice strength".

The shear-to-divergence ratio is used in the present study as a measure of ice strength, with theta values of 0 (180) characteristic of divergence (convergence) indicating reduced (increased) ice strength. This sentence is clarified in the revised text such that

"Changes in ice strength monitored by shear-to-divergence ratios are associated with the transition from an on or offshore to along-shore ice drift regime. "

In light of the present reviewer's comments, following the findings of Richter-Menge et al. (2002) showing correspondence between ice deformation and stress measurements, and since ice strength is dependent on ice concentrations and thickness, the phenomenon whereby the ice cover fails due to internal ice stress comparable to ice strength is also examined in the context of Figure 14 and understanding the rheological characteristics of sea ice.

\*(P16L19) "vorticity superimposed on shear weakens ice strength" I could not understand this. Please explain more.

The authors agree that this statement did not provide much clarity or information regarding the correspondence between DKPs and the S/D ratio as a measure of ice strength. The initial intent was to describe the role of external forcing captured by vorticity in disrupting shear flow and subsequently reducing ice strength. This sentence has however been removed given the speculative nature of this assertion.

Technical corrections: \*(Figure 1b) I recommend to have the edge of each circle colored

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in black because some circles are hard to see.

This has been corrected in Figure 1b of the supplementary material.

\*(Figure 3) It would be helpful if the "sudden change" events are shown by arrows in the figure.

Thank you for this suggestion. Arrows indicating sudden change events are now included in most figures.

(Figure 6a) Please magnify the numbers of latitude and longitude. And please designate which color corresponds to A – D.

This has also been corrected in Figure 10 of the supplementary material.

(Figure 6b, 7, 8) Please magnify the scales of the figures. They are hard to see.

The scales of the figures have been magnified, and the x- and y-axis labels in particular.

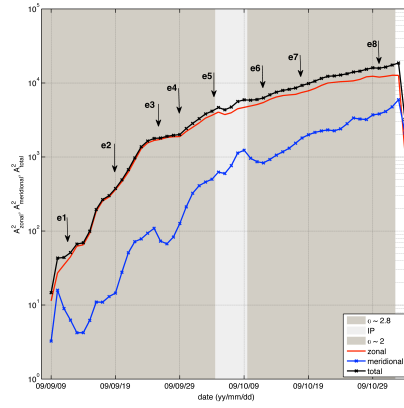
That is all. Faithfully yours.

Thank you once again for helpful comments and suggestions.

Please also note the supplement to this comment:

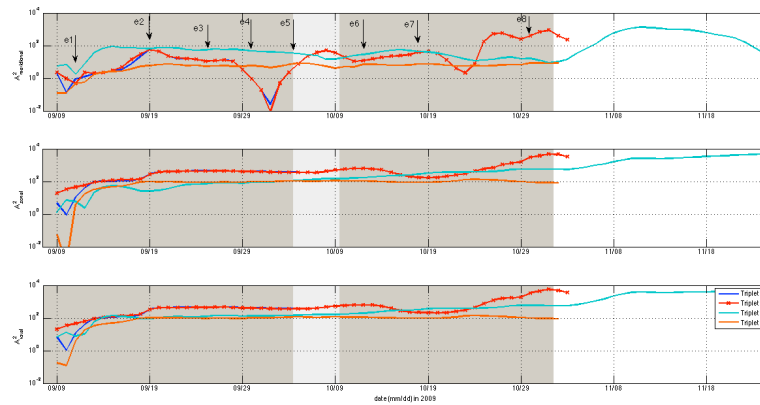
<http://www.the-cryosphere-discuss.net/tc-2016-219/tc-2016-219-AC1-supplement.pdf>

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-219, 2016.



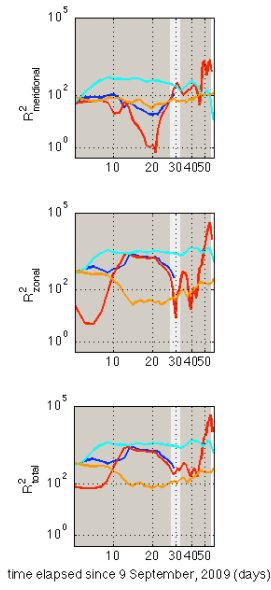
**Fig. 1.** Figure 3 (revised version): Absolute (single-particle) dispersion statistics for triplets A to D, depicting zonal (red), meridional (blue), and total (black) dispersion

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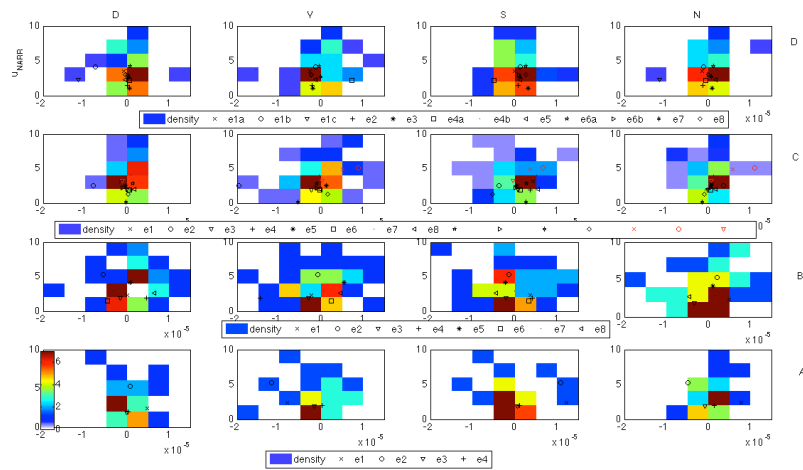
**Fig. 2.** Figure 4 (revised version): Absolute (single-particle) dispersion statistics depicting meridional (top), zonal (middle), and total (lower) dispersion to characterize local changes in ice drift.

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**Fig. 3.** Figure 9 (revised version): Relative (two-particle) dispersion showing meridional (top), zonal (middle), and total (lower panel) relative dispersion as a function of elapsed time.

C13



**Fig. 4.** Figure 15 (revised version): Scatter plots of NARR winds versus DKPs for triplets A to D showing density of values in wind and DKP bins. Symbols depict sudden change events.

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