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Interactive comment on "Seasonal changes in sea ice kinematics and deformation in the Pacific Sector of the Arctic Ocean in 2018/19" *by* Ruibo Lei et al.

Anonymous Referee #1

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It is not clear from the conclusions, abstract and results where the emphasis is in this paper, with too much attention paid on the synoptic conditions over the key finding. I think the key point is that the space-time coupling for ice deformation changes over the transition from free drift to a consolidated ice pack. This point is worth reporting, as I believe it has not been shown with clarity before. However there is some points to address to make sure that this result is real.

The study also shows a gradient in response to wind forcing across the Canada Basin that might be attributed to the different ice ages. It is shown that there is increasing localization of deformation as the ice pack become more consolidated, which is echoing

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work by Stern and Lindsay (2009).

I have some concerns with the methodology as presented.

1. If you just consider the amplitude of semi-diurnal peak in the velocity you are mixing measurement noise and background energy cascade (typically red noise for ice drift) with the inertial motion. How can you be sure that you are actually not aliasing the inertial power due to weather changes? Are you really sure the peak is apparent for all months? You need to consider how high above the background the inertial peak sits. In some parts of the Arctic this peaks is tidal as well as inertial. You should comment on the roll of tides in the study region.

2. Can you comment on how accurately you can estimate the area localization, delta_15%, given the sparse nature of the buoy array? Is the trend in figure 14 statistically significant?

3. Regarding the results, some are not consistent with previous studies. However there is insufficient information in the manuscript to identify if the results are reasonable based on the data. Your beta values, the spatial scaling exponent, are somewhat higher than values found in previous studies. I am referring to figure 12.

A similar decrease in beta with sampling interval, the space-time coupling, was found by Hutchings et al. 2018, who only had data for March through May. It is interesting that you find c (the gradient in log space) increases from a time the pack is in free drift to a time it is more consolidated pack. I have one suggestion to make sure your results are robust: Is there sufficient data to identify beta in only one month? I have looked at this myself and find the results to be quite messy when I split time series of buoys array deformation by month.

Incidentally there are many places in the paper where the language is implying something causes the other, such as more consolidated ice pack causes lower beta and higher c. I would suggest you consider that patterns that covary do not indicate they

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cause one another, but perhaps they could be related. Consider being careful with your language throughout.

The paper could be refocused in the abstract, discussion and conclusion to focus attention on the main findings. While the synoptic situation is important and it needs to be mentioned how the ice pack responded dynamically to seasonal synoptic changes, these detail distract from the main points.

Specific points

line 21: It is not clear what "Areal localization index" is in the abstract. Perhaps use plain language here rather than jargon.

Please check for small grammatical errors. For example line 28 in the abstract "ore pronounced in the future as sea ice losses at higher rates in the". I think "as ... " should be "as sea ice losses are at higher ..."

line 35: "the Arctic Amplification". the not needed, and elsewhere.

line 43: The first sentence is hanging here, I think you need to clarify what you mean by deformation.

line 68/69: "inertial signal". You need a better description of the inertial oscillation of the ice-ocean boundary layer in response to impulses imparted by sudden changes in wind direction.

line 108, using semi-colons will help separate items in the list.

line 116: "From" should be "Of"

line 129: remove "have"

line 136: I do not understand what you are calculating over the buoys that are 1 standard deviation from mean latitude or longitude. Why choose one standard deviation? This seams arbitrary and whether there are distortion effects related to the spherical

coordinates depends on the array size, and 1 standard deviation probably changes over the time the buoy array exists.

line 156: "Because of the delayed release of NSIDC data ..". I suspect you might be able to get more recent data if you ask Mark Tshudi personally.

Regarding the inertial motion index. How do you ensure this is actually a peak and not background noise?

equations 6 and 7: I think you need to specify that beta and alpha are the scaling exponents for the mean deformation. As sea ice deformation is multifractal, the exponents vary for the different moments of the deformation distribution.

line 209, this sentence is a little clunky. I think you want to say you calculate the empirical orthogonal functions for the sea level pressure. Also, did you expand SLP earlier?

line 498-490, and line 28-29: This seams to be conjecture. The ice in this region is already mostly seasonaly any way so I think it is moot point that there will be further losses in these regions.

Finally some of the figures are overly cramped in their use of space. e.g. figure 9 almost has labels for sub panels overlapping. The month lables are hidden inside the figures and a little bit of space below the color bar would help readability. Figures 10, 15 have similar issues.

References Hutchings, J. K., Roberts, A., Geiger, C. A., & Richter-Menge, J. (2018). Corrigendum: Spatial and temporal characterization of sea-ice deformation. Journal of Glaciology, 64(244), 343-346. Stern, H. L., & Lindsay, R. W. (2009). Spatial scaling of Arctic sea ice deformation. Journal of Geophysical Research: Oceans, 114(C10).

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