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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.

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Reply to reviewer 1

1 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 are some points to address to make sure that this result is real. We will rewrite the sectors of conclusions, abstract and results, and make them more focusing. We will highlight the space-time

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Discussion paper

coupling for kinematics and ice deformation changes over the transition from free drift to a consolidated ice pack.

2 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 work by Stern and Lindsay (2009). We will further compare our results with theirs (Stern and Lindsay, 2009), and highlight the spatial gradient of ice kinematics and ice deformation in response to wind forcing in the conclusions and abstract.

3 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. Inertial oscillations (in the northern hemisphere) are clockwise oscillations, in contrast to tidal oscillation, which can rotate clockwise or counter-clockwise. Amplitudes shown in Figure 9 are that at the local negative inertial frequency (about -2.01 $\sim$ -1.94) after Fourier transformation of monthly time series of normalized ice velocity. At this frequency, there are also some energy caused by tidal forcing and high-frequent parts of wind and current forcing. In the revision, we will also show the amplitudes at the positive tidal frequency (+2), which includes the energy from tidal forcing and background noise of high-frequent parts of wind and current forcing. From the amplitudes at the positive tidal frequency, we cannot identify the obvious seasonal and spatial variations because all the buoys were deployed over the deep waters and the tidal forcing is relatively weak. In addition, both tidal forcing and high-frequent parts of wind and current forcing are not expected to have seasonal changes. Thus, we will further explain the spatiotemporal change patterns shown in Figure 9 are majorly attributed to the changes caused by inertial oscillations.

3. 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? To estimate the area fraction of top 15% ice deformation, we use the data obtained from the relatively dense buoy array deployed in the north of Pacific sector of Arctic Ocean, but not from all buoys. We will test the reliability for using the area fraction of 15% to estimate the area localization through using various fractions. The trend in Fig. 14 is statically significant at 0.01 level.

4 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. The spatial scaling exponent is strongly dependent on the ice cohesiveness and temporal sampling rate. In the Fig. 12, the results include the results obtained from September and at the 1-h temporal resolution, thus including some relatively large value. The value obtained in Jan-Feb. at the 3-h temporal resolution shown in Fig. 2 was 0.35–0.37, which was slightly smaller than that obtained in Beaufort Sea during March-May 2007 (0.40), and comparable with that obtained in the Central Artic Ocean during May 2007 (Itkin et al., 2017). As our known, the Arctic sea ice in the Pacific Sector may reach to its annual maximum thickness in May or early Jun. (e.g., Perovich et al., 2003). Thus, the strongest ice cohesiveness would occur during the latter winter and early spring, but not in the mid-winter when the air temperature is coldest but the ice thickness still doesn't reach the annual maximum. In the revision, we can enhance the comparison with the results from other studies.

Perovich, D. K., T. C. Grenfell, J. A. Richter-Menge, B. Light, W. B. Tucker III, and H. Eicken, Thin and thinner: Sea ice mass balance measurements during SHEBA, J. Geophys. Res., 108(C3), 8050, doi:10.1029/2001JC001079, 2003.

5 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. As mentioned above, we will highlight the new findings for the space-time coupling of ice deformation. By combining the findings obtained from Hutchings et al. (2018), we will add some discussions on the annual circle of the space-time coupling regime of ice deformation. To estimate the beta, we use the strain rate obtained from all triangles consisting of any three buoys, which can guarantee the magnitude of statistical samples. This method has been used by Itkin et al. (2017), who also estimate the beta using the data obtained from one month. To test if our results are robust, we will further estimate the seasonal beta, i.e., those obtained in autumn (September-November) and winter (December-February).

6 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 cause one another, but perhaps they could be related. Consider being careful you're your language throughout. Thanks for the suggestions. We will check the language through the manuscript and make sure that the expression is rigorous and clear.

7 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 details distract from the main points. Thanks for the suggestions. In the revision, we will focus on the seasonal changes in the space-time coupling of ice deformation.

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

9 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 ..." We will check the grammatical errors through the manuscript.

10 line 43: The first sentence is hanging here, I think you need to clarify what you mean by deformation. We will correct this mistake in expression.

11 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. We will add the discussions on the inertial oscillation of the ice-ocean boundary layer in response to impulses imparted by sudden changes in wind direction.

12 line 108, using semi-colons will help separate items in the list. line 116: "From" should be "Of" line 129: remove "have" We will correct these mistakes in expression.

13 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. We will give detail on the changes in the geographical distance according our use of 1 standard deviation of latitude and longitude.

14 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. It's just supporting data, but we will try to discuss with Mark Tshudi.

15 Regarding the inertial motion index. How do you ensure this is actually a peak and not background noise? In fact, the peak value of inertial oscillation will be affected by the high frequency variations of wind or current, but the influence is very small. We select the peak value manually in the range of 0.5h near the inertial period. In the reversion, we will further explain the method.

16 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. We will specify that beta and alpha are the scaling exponents for the mean deformation.

17 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? Yes, we want to say we calculate the empirical orthogonal functions for the sea level pressure. We will make the expression clearer. We have expanded SLP already in Line 146.

18 line 498-490, and line 28-29: This seams to be conjecture. The ice in this region is already mostly seasonally any way so I think it is moot point that there will be further losses in these regions. Yes, the ice in these regions is already mostly seasonally. However, the further lengthened ice melting period, even the length of free-ice waters occupation, will shorten the growth season of sea ice and reduce the ice thickness, thus enhancing the response of sea ice kinematics and dynamic deformation to atmospheric forcing. We will add some discussions on this feedback regiem.

19 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. We will improve these figures.

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