We would like to thank the reviewer for his/her insightful feedback regarding our manuscript. We have revised based on his/her commentary and believe the manuscript is much stronger as a result.

Review of "Marine and terrestrial influences on ice nucleating particles during continuous springtime measurements in an Arctic oilfield location" by Creamean et al. The authors report measurements of ice nucleating particles (INPs) over the period of 3 months in the Arctic during spring. Data from 17 days are presented. Since there are few measurements of INPs in this region and INPs are important for predicting climate, the measurements are certainly valuable. However, most of the conclusions reached by the authors are not well supported by the current analysis. The authors have a nice data set, but a more rigorous analysis is needed to support the conclusions in this manuscript. Specific comments are included below.

Comments:

Abstract. The authors state that the concentrations of coarse mode INP at -15 C were low during the first part of the campaign but then increased by nearly 2 orders during late May. To better illustrate these results, please use a log scale in Fig. 4 to represent the INP concentrations. Currently, the changes in INP concentrations at -15 C are not clear in Fig. 4 due to the use of a linear scale to represent all freezing temperatures.

Thank you for the suggestion. We have changed Figure 4 so that log scale is shown.

Abstract. The authors state that the higher concentrations were attributed to air masses originating from over sea ice leads and tundra surfaces. This conclusion was mainly based on back trajectories and information on snow and ice coverage in the Arctic. Although reasonable, the analysis was not very rigorous. The authors did not rule out anthropogenic pollution from local sources. This should be done in a rigorous and quantitative manner if the authors want to claim the INPs are from natural sources.

The air mass trajectory analysis was used for context for the chemical analysis conducted, both of single particle and bulk compositional information, in addition to other supporting information. First and foremost, we know that the aerosol composition in general in the size ranges relevant to the INP measurements were predominantly sea spray aerosol and dust based on the chemical analyses. Very little influences from soot or fly ash (i.e., local industrial pollution) were observed (4% and 16% of the particles that were > 1.15 µm on 23 May and 28 May, respectively; Figure 7). Second, based on size alone, we would not expect pollution sourced from Prudhoe Bay (which a majority by number are sub-100 nm; Creamean et al. (2018a); Maahn et al (2017)) to overlap with the sizes of the INPs observed (i.e., > 2.96 µm) at Oliktok Point. Third, INPs measured at the warmer end of the temperatures we focus on during our case study are likely biological or dust in origin (e.g., Kanji et al. (2017), Murray et al. (2012)). Fly ash and soot generally form ice at much colder temperatures. We have revised Figure 7 to show wind direction and speed separately, and now discuss this in more detail at the end of section 3.3. We note that although winds were easterly on 28 and 29 May, wind prior to those days were variable and can help explain the aerosol sources during our higher marine and terrestrial INP concentration periods. Thus, based on the combination of freezing temperatures, size, single-particle composition, bulk composition, local meteorology, and air mass transport, we demonstrate that there was indeed little influence from local anthropogenic pollution. We added several sentences discussing these points at the end of section 3.3—that it is possible but unlikely that local pollution largely influenced the INP concentrations during late May.

Section 1.2 (Sample collection). For INP analysis, particles were deposited on Vaseline-coated Mylar. Could the Vaseline influence the INPs and shut off freezing of some of the INPs? Were all INPs extracted from the Vaseline coating? Since this is a new approach and Vaseline could cause artifacts, the influence of Vaseline on INP concentrations should be discussed.

The fact that Vaseline is used in preparation of the copper plates and contributes very little artifacts to the INPs during testing indicates it likely does not contribute significant artifacts to the samples containing aerosols impacted on Vaseline. The use of the Vaseline coating has already been tested and discussed in Creamean et al. (2018b) as we note in the methods section. Additionally, Tobo et al. (2016) presented drop freezing results using Vaseline with little to no contaminant influences from the Vaseline itself, and observed blank water samples freezing starting at –30 ℃. However, we cannot determine if all the INPs were extracted with certainty. For clarity, we have added, "It is possible not all particles were removed during the extraction process; however, previous control testing indicates sufficient aerosol loading is resuspended (Creamean et al., 2018b)." to section 1.2.

Page 8, lines 19-21. The authors refer to previous studies to suggest that they had relatively clean conditions around late May. But the references are from different years. Hence, these references may not precisely apply to the current study. The authors need to prove that late May was associated with relatively clean conditions. This is not obvious from Figure 2. This is especially important if the authors want to claim the INPs are from natural sources.

The purpose of providing the average values of particle number concentrations in the text, specifically from the CPCs which are indicative of Prudhoe Bay pollution (Creamean et al. (2018a); Maahn et al. (2017)) was to demonstrate how May was relatively clean compared to March and April. To make this clearer, we have revised Figure 2 to show hourly averages and log scale for the particles concentrations to demonstrate the differences between Mar and Apr versus May. We also changed the average values provided in the text to those calculated from hourly concentrations to reflect the values in the figure. One interesting feature that is more prominent is the high variability with very low to concentrations comparable to the previous two months in May. We now discuss this in the text, in addition to highlight such variability as part of the "interesting aerosol events". Please note that we have also removed the wind parameters panel as that was redundant to Figure 7 and we do not discuss the local winds until later when describing the particle compositions.

Page 9, line 11. The larger spread in INP concentrations could also be due to just a larger variation in source strength.

Thank you for pointing this out. We have added this possibility.

Page 9, line 28-29. "particles that are theoretically thought to be too small to serve as efficient INPs." I do not think these references presented a theory that suggested that small particles are not efficient INPs. There are also several laboratory studies that show INPs can have sizes similar to the ranges isolated by stages C and D.

We have revised the wording to instead state that particles of these sizes are relatively less efficient INPs as compared to their larger counterparts.

Page 10, line 6-8. The authors suggest that the back trajectories (Figure 6) illustrate that the transport was slow and remained near the surface. However, there is no information on trajectory height or time in Figure 6. I suggest that Figure 6 be modified to include height information and time information to support the author's claims. Also, the trajectories go outside the plotting area in Figure 6. I suggest that the plotting area be increased so the full trajectories can be observed.

We have revised Figure 6 to include height and time information. We also increased plot size for the full trajectories and included zoomed in panels to show more local scale transport.

Page 10, line 13-15. “Air reaching Oliktok point originated from over a large area of open leads within 30 km off the coast of Utqiagvik (Figure 6d).” This point is not clear from Figure 6d. Where are the open leads in Figure 6d?

We have revised Figure 6 (see response to previous comment) to include an inset of the areas of open water within 30 km and describe this feature more in section 3.3. Open leads are evidenced by the regions that are < 100% sea ice percentage, indicating the presence of open water in the 4 km grid cells. However, we realize that the term “leads” may not accurately represent the area of < 100% sea ice concentrations (i.e. light blue colors directly north of Oliktok Point). The width of leads varies from a couple of meters to over a kilometer, thus, they are difficult to resolve in the 4-km sea ice data we use. It is possible the open water is simply small or larger ice floes that have broken off the pack ice near the ice edge. Thus, we have changed this to “marginal ice zone” (MIZ) as that is a more accurate description of the transition between open water and sea ice during the melt season, but that leads may be a feature within this zone.

Page 10, line 15-16. “sources were generally regional in nature. . .” This statement is not well supported. As the authors point out, mineral dust can be transported to the Arctic from long distances [Stone et al., 2007]. Perhaps the INPs measured by the authors were mineral dust containing biological material transported from long distances? I do not think this was ruled out. If the authors want to rule out long-range transport than more information and discussion is required.

It is possible that the INPs we observed could have originated from mineral dust from distant sources. However, we provide several reasons as to why the INPs were likely from more local or regional sources. Recent work by Kylling et al. (2018) demonstrates surface dust impacts from several midlatitude sources to the entire Arctic is predominant during May. Yet, they also show that the major source of dust at the surface is from North American north of 60 °N, indicating regional source influences. Specific to our region, supermicron mineral dust has been shown to be transported during Arctic Haze as demonstrated by Quinn et al. (2002) at Barrow, but at very low mass concentrations relative to other aerosol species (i.e., < 0.005 μg m^{-3} based on nss-Ca^{2+}). Additionally, given the size in which we observed the INPs (> 2.96 μm), it is unlikely such large particles were transported from very distant sources, especially considering the air mass transport pathways were near the surface in the boundary layer for several days prior to arrival at the site (evidenced by the revised Figure 6). We have added a few sentences ruling this possible source out in section 3.3.

Kylling, A., Zwaaftink, C. D. G., and Stohl, A.: Mineral Dust Instantaneous Radiative Forcing in the Arctic, Geophys Res Lett, 45, 4290-4298, 10.1029/2018gl077346, 2018.

Quinn, P. K., Miller, T. L., Bates, T. S., Ogren, J. A., Andrews, E., and Shaw, G. E.: A 3-year record of simultaneously measured aerosol chemical and optical properties at Barrow, Alaska, J Geophys Res-Atmos, 107, Artn 4130 10.1029/2001jd001248, 2002.

Page 12, lines 1-5. “We present the first INP measurements in an Arctic oilfield location and demonstrate how local and regional transport from marine and terrestrial sources to an industrial region can introduce high concentrations of coarse, warm temperature INPs that are possibly of biological origin.” The authors have not ruled out adequately local industrial emissions as the source of INPs. If they want to claim that the sources were local marine and terrestrial, more analysis is required to rule out local industrial emissions. For example, does the INP concentrations correlate with tracers of industrial emissions?

Please see response to the second comment.

Figure 4. As mentioned above, I suggest using a log scale to illustrate the INP concentrations. Currently it is very difficult to follow the trend at -10 C and -15 C due to the linear scale. Also, what is the onset 99th percentile, how was this calculated, and why is it different from the measured onset temperature? Is the

measured onset temperature or the onset 99th percentile more relevant? Also, Figure 4 doesn't include error bars. Error bars should be included, otherwise it is hard to tell if the trends are statistically significant.

Please see response to the first comment. The 99th percentile was removed from the figure to avoid confusion, especially since we do not discuss this value in the results and discussion. We have also added error bars to the INP concentrations and onset temperatures.

Figure 27, a, b, c. Why only include SEM-EDX results from the end of May? Results from the first part of May would be very useful to interpret the INP data.

We agree that results from early May would be useful; however, given limited resources and samples, we were not able to analyze any more samples other than those presented. The bulk composition was measured during this time period, and we do compare to the case study time period.

Figure 7, panel e. I suggest a separate panel for the wind speed and direction. Currently the panel is congested.

Done.

Figure caption 8. "Additionally, the bars and whiskers represent INP concentrations at temperatures in Table 2 and at all temperatures, respectively." What temperatures from Table 2 are plotted? What was plotted in the case of "none" in Table 2? Please include this information in the figure caption for clarity.

We have clarified that the bars show the ranges of INP concentrations (i.e., minimum and maximum at the four temperatures shown in Table 2) in the caption.