Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-1076-RC2, 2020


Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Shipborne observations reveal contrasting Arctic marine, Arctic terrestrial and Pacific marine aerosol properties" *by* Jiyeon Park et al.

**Anonymous Referee #2**

Received and published: 15 January 2020

This manuscript describes approximately one month of measurements of aerosol physical properties and CCN concentrations taken aboard ship in the Pacific region of the Arctic, and Northern Pacific Ocean. This work demonstrates that terrestrial inputs in coastal areas can be an important driver of the particle size distribution in Arctic regions, and can drive strong new particle formation relative to open ocean areas of the Pacific. This is an important and interesting conclusion that has not been made in the past. Also, notably, this work presents measurements of particle size distributions starting at 3nm, which has not been done in many Arctic regions. This manuscript is well written, and presents the main results clearly. I support publication in ACP once the following issues can be addressed.



**Major comments:**

(1) I am somewhat surprised that the authors have not demonstrated agreement between the two SMPS systems in their overlapping size range (i.e., 10-80nm). This is a crucially important size range for understanding particle growth and adequate quality control of these measurements should be addressed. How does N10-80 measured by the two instrument compare across the measurement period? Were any size calibrations of the two instruments made in the field? A clearer description of how the two measurements of particle size were aggregated over the overlapping range to produce Figure 7 is needed.

(2) How does the condensation sink and average growth rate compare (1) across the different types of air masses sampled, and (2) to previous Arctic measurements? Was the condensation sink significantly higher in the Pacific air masses? Figure 7 would suggest that is the case. I suggest that the authors make full use of their unique data set by calculating the above two quantities wherever possible, and including them in relevant figures. Statistics of these quantities could be included in Table 1. This would facilitate improved comparison with other studies in similar coastal Arctic regions (e.g., Collins et al., ACP, 2017 https://doi.org/10.5194/acp-17-13119-2017 and Burkart et al., GRL, 2017 doi: 10.1002/2017gl075671). In particular, Collins et al 2017 provides a detailed comparison of the condensation sink in their observations with that sampled globally (their Figure 8). Placing the author's results in this broader context would strengthen the paper significantly.

(3) What other environmental variables were different between the three air mass source regions? e.g., wind speeds, cloud cover?

(4) It appears as though multiple modes were present in some cases (e.g., during 9/13-9/17), and based on Figure 7 appear in average size distributions. As discussed in Burkart et al., GRL, 2017 (doi: 10.1002/2017gl075671), comparing the growth rates of different particles modes can provide significant insight into the physical chemical

properties of the condensing/partitioning species. I do not suggest that the authors undertake a detailed modelling exercise as in Burkart et al, rather that the authors calculate growth rates for the different modes and discuss what these quantities might mean in the context of the Burkart et al analysis.

**Minor comments:**

L67-68: I agree with this statement in general; however, the authors miss two very relevant studies of ship-based coastal Arctic measurements: Collins et al., ACP, 2017 https://doi.org/10.5194/acp-17-13119-2017 and Burkart et al., GRL, 2017 doi: 10.1002/2017gl075671

L69-82: This paragraph focuses on studies that have linked NPF events to increases in measured CCN; however, the authors focus not only on studies from Arctic regions and seem to focus on global observations as the expense of being exhaustive in discussing all of the very few relevant Arctic studies. The two studies mentioned in the comment above should be included here, and are more relevant to the authors' discussion than Kalivitis 2015 and Rose 2017. Burkart et al., ACP, 2017 (doi: 10.5194/acp-17-5515-2017) also connect NPF with CCN in coastal Arctic environments.

L123-126: Time resolution of the aethelometer?

L141: Can the authors be more quantitative in describing "particularly high (spike) and varied dramatically in a short time"?

L235: Tremblay et al, ACP, 2019 (https://doi.org/10.5194/acp-19-5589-2019) demonstrated regional growth events taking place over the Northern Canadian Arctic Archipelago, and their results are relevant to this discussion.

L262-263: Could a satellite-based measurement of cloud fraction (e.g., from Aqua MODIS that is openly available) be used to assess any impact of cloudiness on the observations of NPF?

L422-435: Burkart et al., ACP, 2017 (doi: 10.5194/acp-17-5515-2017) also estimated



the CCN activation diameter to be approximately 80nm in a coastal Arctic environment.

Table 1: Do NStandardSMPS and NnanoSMPS include the overlapping size range? I suggest that the authors be explicit about the size range here, rather than referring to the instrument. Presenting mean concentrations in specific size ranges from aggregated size distribution data may be more useful (e.g., as shown in figure 2)

Figures 3-6: Could more than one tick label be added to the size scale on the nanoSMPS size distributions?

Figure 7: Specify which SMPS is used in the size range from 10-80nm

**Specific comments:**

L61: Ice and snow-covered regions

L264: change 'trends' to 'tends'

L286: missing 'were'

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