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Atmospheric Chemistry and Physics Discussions

Interactive comment on "New particle formation and its effect on CCN abundance in the summer Arctic: a case study during PS106 cruise" *by* Simonas Kecorius et al.

Anonymous Referee #2

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This manuscript describes an interesting and valuable set of measurements made aboard Polarstern in the region near Svalbard, during May to July 2017. The authors report measurements of neutral and charged clusters particle size (together covering a size range on <1 - 800nm), particle hygroscopicity and cloud condensation nuclei. Measurements of nucleation mode particle size and charged/neutral cluster abundance have been rarely made in Arctic regions, making this a unique and valuable data set that indirectly furthers our knowledge on the identity of species responsible for Arctic nucleation and initial growth of particles to Aitken mode sizes. The manuscript is well written and largely well organized. The following comments are intended to improve an already very good manuscript.

General Comments:

(1) L65-73: This is an important section of the introduction, and at present is missing several of the relatively small amount of studies that exist. Since so few data sets exist, it is reasonable to cite them even if they are not all discussed in detail. Some that should be included are listed here: Freud et al, doi:10.5194/acp-17-8101-2017; Nguyen et al, doi: 10.5194/acp-16-11319-2016; Dall'Osto et al, doi: 10.5194/acp-19-7377-2019; Burkart et al, doi: 10.5194/acp-17-5515-2017 and 10.1002/2017gl075671; Collins et al., doi: 10.5194/acp-17-13119-2017; Leaitch et al., doi: 10.12952/journal.elementa.000017; Tremblay et al., doi: doi.org/10.5194/acp-19-5589-2019; These and others were recently reviewed in doi: 10.1029/2018rg000602.

(2) The discussion of ship deck observations in Section 2.1.1 may serve the reader better if they were incorporated into the larger discussion of NPF events in Section 3.1

(3) A detailed description of how pollution influence from the ship was reproducibly removed from the data set needs to be included in the Methods section. Was the influence quantified with a specific measurement (e.g., rapid variability in particle concentrations in a certain size range?)? Or, was a specific wind sector removed from the data? "Abrupt and short increases in particle number concentration recorded by a CPC" as cited in L286 as the method for filtering ship stack pollution; this should be elaborated in the methods section and detail is needed on how the authors determined that "most of the cases when the particle number increased tenfold" could be attributed to NPF. I strongly discourage the authors from including periods of ship pollution in Figure 2 (noted in L374-375). I disagree that "better representation of particle growth" (L1031-1032) warrants this.

(4) Further to comment (3) above, for the offline chemical analysis the sampling time was 72-144 hours (L223); how was sampling pollution from the ship minimized or avoided (e.g., by a wind switch)? The presence or absence of any such precautions should be stated, and if they were absent the possible implications should be

discussed.

(5) A slightly more descriptive title might be helpful. For example, “during PS106 cruise” could be replaced by a few words describing the region of measurements. This would be helpful for readers not familiar with the region covered by the cruise.

Specific Comments:

(1) L44: This statement might be best attributed to a paper such as Croft et al., Nature Communications, 2016, that makes a more direct connection to radiative forcing

(2) L60-62: This statement is more attributable to Willis et al., Reviews of Geophysics, 2018 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018RG000602) and Abbatt et al., ACP, 2019 (https://www.atmos-chem-phys.net/19/2527/2019/acp-19-2527-2019.html)

(3) L63: Dall’Osto et al., Scientific Reports, 2018 corroborates these results for different regions and multiple stations (https://www.nature.com/articles/s41598-017-17343-9 and https://www.nature.com/articles/s41598-018-24426-8)

(4) L206: I assume that the CCN measurements were made on poly disperse aerosol, but this is not explicitly stated.

(5) L218-219: A large fraction of the organic aerosol may be semivolatile (e.g., Burkart et al, GRL, 2017). How might the semivolatile fraction be impacted by both heating of the inlet and long sampling times? Possibly biases introduced into these measurements should be discussed.

(6) L390-391: This is a very interesting observation and is in agreement with Tremblay et al., 2018 (doi above), could these data be included in the Supplement?

(7) L425-429: Burkart et al., 2017 (doi above) came to similar conclusions

(8) L431-441 and Figure 4: Rather than referring to the stage number here I suggest the authors refer to the corresponding size range.

(9) Related to comment (8), are any accumulation mode particle present during growth events? Do these larger modes also grow during NPF? Is sea salt present in accumulation mode sizes? Collins et al., 2017 (doi above) and Burkart et al., 2017 (doi above) observed growth of multiple modes at different rates showing that the species responsible for growing the larger mode was more semi volatile. If this is the case can the composition of the larger modes be connected to what is growing the smallest particles?

(10) L444-446: This sentence described a very unique aspect of this study which could be highlighted more, for example in the abstract or at the end of the introduction.

(11) L450-460: Comparison to other Arctic studies that report growth rates might be more appropriate here, though I do not dispute the value of comparing to Antarctic studies. Collins et al., 2017 (doi above) report growth rates for events in the Canadian Arctic during two summers. Nieminen et al., ACP 2018 (doi: 10.5194/acp-18-14737-2018) include growth rates from Alert. Available observations were reviewed by Willis et al., 2018 (doi above)

(12) L567-569: Describing the experiments (i.e., the information in brackets) might be more useful than using experiment numbers for those readers not closely familiar with the CLOUD experiments

(13) L572-573: If the measured Aitken mode particles were all organic, how much OM mass would you expect and how does that compare with the measured masses? Also, if the material is largely semivolatile how much do you expect that losses during sampling would impact this assessment?

(14) L619-620: The authors of Willis et al., 2016 provide reasonable evidence for a marine source. For example, that the organic and MSA driven growth was only observed in a shallow marine inversion layer and not aloft. Burkart et al., ACP, 2017 corroborate a marine source of NPF precursors.

(15) L634-636: The cited observations, as well as Collins et al., 2017, do demonstrate some growth into sizes above 50nm. The same work also demonstrates frequent simultaneous growth of multiple modes, and a resulting strong impact on CCN.

(16): L660-670: This is a useful analysis, and I don't suggest that the authors make substantial changes here. However, I do wonder if updraft the most appropriate way to assess this for summer Arctic low level clouds directly impacted by marine sources? Advection from warmer to colder surfaces in a shallow boundary layer might be another mechanism for CCN to active in low altitude clouds, suggested by Leaitch et al., 2016. Marine influence was significantly less for the upper level clouds observed in Leaitch 2016 (see Bozem et al., ACPD, 2019 doi: 10.5194/acp-2019-70)

(17): L706: Will these data be made publicly available in the future?

Technical Corrections:

(1) L144: "0 am" typo? (2) L471: "is" to "are" (3) Supplement Figure S1: typo in the y-axis label? [m] to [nm]?

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