Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-256-RC1, 2016
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Growth of nucleation mode particles in the summertime Arctic: a case study" *by* M. D. Willis et al.

Anonymous Referee #1

Received and published: 12 April 2016

This paper describes airborne observations of new particle formation in the Canadian Arctic. Increases in small particle number concentrations were observed during low level flights to the east of Resolute Bay on the edge of the main area of sea ice. As the flight continued in open water the particle size distribution evolved and particles increased in size to 50 nm and greater. As these measurements were taken in very clean conditions, in light winds and when the shallow marine boundary layer was capped with a strong inversion the particle evolution can be linked to new particle formation and growth. Chemical measurements of the larger particles show the presence of methyl sulfonic acid (MSA), organic matter and trimethylamine. The organic matter was shown to have a rather different mass spectral chemical signature during the period of particle growth compared to other regions during the study and points to the role of secondary organic matter in growing new particles into particles that may be active as CCN. There

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may be some evidence that iodine is involved in the new particle formation but the authors, rightly, are tentative in their conclusions on this point. The paper is certainly worthy of publication in ACP in my view if some points are considered.

Page 144-165: Given that a comparison of aerosol number concentration across a range of sizes derived from size distributions and total number concentrations is the central theme to the paper I find it strange that a characteristic size distribution from the UHSAS and SMS and the average integrated number comparison with the CPC is not provided as a figure. This could also be used to illustrate how the integrated number concentrations were derived as well as show the agreement between the different instruments the authors refer to. Size distributions up to 100 nm are shown in figure 3 but I suspect that these are only from the SMS.

In the supplementary material (Figure S5), it appears from the profile of Ntotal that there was an initial descent to around 80-100 m and then an ascent to 300 m before the aircraft descended again to 70 m. In this second period of surface layer sampling, Ntotal was not enhanced as it was in the early sampling period. I don't see how this relates to the straight and level runs shown in figure 3 and needs clarifying.

Lines 352 to 353 and Figure 3b: "Particle number size distributions illustrate that particles below 20 nm (Figure 3b) grow to form a mode centred at 30 – 40 nm (Figure 3c–e)." Size distributions in figures 3b, c and d only show size distributions between 20-100 nm. This needs to be explained clearly in the figure caption and text. I don't think that the authors can say that the size distributions on their own show growth from below 20 nm to form a mode at 30-40 nm. I do not dispute the claim but I would like to see a clearer summary of the evidence presented by the authors in this section to support what is at present simply an assertion. This can be done given the distances and timescales. The advection timescale from 85W to 82W is between 3 and 6 hours at windspeeds 4-8 m/s and the sample time of the aircraft is around 10 minutes depending on the aircraft speed. Given the changes in aerosol concentrations and sizes this excludes a wider aerosol source region and implies that the source is to the west

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of the sample region and the aerosol distribution develops as the air moves to the east. It would very informative to the reader to include such a discussion at this point in the text in my view and to discount other possibilities.

In addition to the above, can the authors say anything about the growth rates of the particles and the size of the condensation sink?

Lines 368-372: Elevated concentrations of larger particles and aerosol component mass are observed at the east of the sample region at altitudes up to 900 m. The authors suggest that some mixing has occurred. The thermodynamic profiles of potential temperature in figure S5 show an increase with height from the surface to around 300 m, no change to 500 m, and a further increase aloft. This suggest the lowest layer remains stable to 300 m and there is little thermodynamic forcing of mixing throughout the column. Was any cloud present through the column, the RH profile suggest not, but without cloud it is rather difficult to see how mixing of the surface layer could be responsible for the profiles observed.

Minor comments: Line 121: isn't the inner diameter the most important?

Lines 155-157: The authors state that the agreement between different aerosol instruments was generally within a factor of two. What was within a factor of two, total number, size or something else? The comment needs to be more precise.

Line 156: The CCNC is not mentioned at all in the text up to this point. I assume that this is a cloud condensation nucleus counter but the model and operating mode is not mentioned, was it run at one or more supersaturations or was it scanned, if so over what timescale and over what supersaturation range?

Line 269-270: why two laser beams? are these separated or is the sample volume of the two co-located? Page 28: Figure 2 caption: "The location of the aircraft during sampling is noted by the grey triangle" I assume the grey triangle refers to the position of the aircraft at the time of the start of the FLEXPART release?

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Line 305 and following: It would be useful to provide some detail on the time of take-off and the air speed and/or time of the profiles and manoeuvres.

Table S1: The ion m/z are incorrect for CH3SO+ and CH3SO2+

Figure S5: caption, should read “below the inversion” and not “in the inversion”

Line 350: “of” not “on”

Page 11-12: Figure 6: Given the very low particle numbers the ALABAMA instrument is limited by counting statistics. It is rather disingenuous to provide the cluster abundances as a fraction when the total numbers in each cluster are less than the fractional amount. It is better to show the total numbers of particles counted. By my calculation, only the TMA containing cluster includes more than 10 particles in the cluster and many of the “clusters” are only 1, 2 or 3 particles.

Figure S6: It is probably best to present the 4 point smoothing and the uncertainty based on the Poisson counting stats which I suspect will show there is little that is statistically significant above 200 nm.

Lines 451-453: I am unconvinced that the ammonium shows a smooth decrease from west to east similar to the sulfate. There is a marked reduction in ammonium in the western boundary layer that is not matched by the sulfate.

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