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The paper *Characterization of aerosol growth events over Ellesmere Island during summers of 2015 and 2016* by Samantha Tremblay et al. investigates formation of new particles and their growth in the Canadian Arctic providing valuable information about the chemical composition of Aitken mode particles in this region. The number of studies on fine particles in the Arctic is increasing but our understanding is far from being exhaustive and measurements are generally sparse and incomplete. Upon investigation of size resolved chemical composition the authors claim that for aerosol smaller than about 100 nm the growth is dominated by organics. This would certainly be a high impact finding if is proven to be true, but the data presented here are not convincing enough to support this hypothesis and some major integrations are needed. Concluding, the paper is nicely written and addresses an important topic but there is some more work that needs to be done before publication in ACP.

Major comments

- **Influence of meteorology on measurements performed at PEARL:** In the paper is said that "growth events (and presumably nucleation) are more commonly observed when the inversion is weaker or not present" however only vertical profiles during event days are analysed. The authors should look at vertical profiles during non-event days and compare them with those presented in the paper. For example it would be useful to plot the histogram shown in fig. 6 both for event and non-event days and check if the difference is significant or not. Moreover, for certain days, it seems like growth is happening with a relatively strong temperature inversion. I would suggest to check the diurnal profile of water vapour mixing ratio (I assume this to be measured at PEARL), this should show an increase if the airmass is coming from the boundary layer compared with the free troposphere. In case no evidences for boundary layer air contribution are found it would be interesting to check if the particle chemical composition during the growth is different from other events.
- **Methanesulfonic acid contribution:** Mass spectra for 2 aerosol growth events are compared with lab measurements of MSA particles to prove that there are organics contributing to the growth besides MSA. The relative differences and the small correlation between ambient and lab spectra provides a qualitative argument in favour of this hypothesis but the authors should try to quantify the contribution of MSA to the total organic mass. I'm aware of the fact that, due to the

low resolution of a Q-AMS, this could be tricky but there are previous studies [1] reporting MSA concentration with the same instrument (the same paper is also cited by *Tremblay et al.*), so with the available data it should be possible to calculate at least an upper bound on the organic mass fraction attributable to MSA.

- **Size resolved aerosol chemical composition:** In figure 7, the authors show chemical composition down to 10 nm, whereas the sizing calibration is done until 80 nm. This extrapolation is beyond AMS capabilities (it would be incredibly good if an AMS could really measure 10 nm particles) and needs to be corrected, a non-AMS user could easily be fooled by these figures.
- **Aerosol composition during growth events:** the high organic-to-sulfate ratio reported in this paper for particles below 100 nm is surprising, in particular considering the marine sources, unfortunately the authors provide only a *snapshot* for 2 events while a more complete analysis should be performed to support their findings. The first thing that is not clear from figure 7 is why sulfate is present only in the larger particles, is this due to a second mode of aerosols that have a different history? The authors should comment on this. There are no indications about the period considered for the calculation of the size resolved chemical composition: is this an average over the whole growth event or a selection of a defined period? I would appreciate if the authors could show the size resolved chemical composition at 2/3 different stages of the event, this could tell a little bit more about the mechanisms beyond the growth. Moreover, it would be really useful if the authors could plot the total organic and sulfate time series concentration during the growth event. If the growth is mostly due to organics then I would expect to see an increase of organic concentration whereas sulfate should stay more or less constant. In addition, uncertainties on aerosol composition should be estimated and added to figure 7.

Finally, the authors conclude saying that "particles smaller than 100 nm in diameter are predominately organic with the organic-to-sulphate ratio increasing for smaller particle sizes", this is a general conclusion but is based on the analysis of only 2 events that does not provide any statistical basis for such kind of conclusion. For this reason the authors should look at the size resolved organic-to-sulfate ratio for all the events to check whether this statement is verified or not. It would be useful also to compare the size resolved chemical composition with non event days.

Minor comments

- The authors often speak of particle nucleation, I would avoid using this terminology in the paper because there are no measurement for particles below 10 nm. For this reason there are no proof that nucleation is really happening at the measuring sites (in particular considering the small growth rates reported in this paper).
- I would suggest to add a couple of sentences about iodine nucleation in the introduction. This has been proven to be a very effective mechanism in certain coastal regions [2] and a recent paper in the Arctic also showed evidence of this [3]. Later in the manuscript (chapter 3.2) the authors should also mention whether they see or not any evidence for iodine particles in their spectra.
- In chapter 3.1.2 growth rates for 5 selected events are reported. However, there is no mention to the method used to estimate the growth, nor to the size range considered for the calculation. The authors should add this information to the manuscript that is really important in particular when comparing with results from other studies.
- In chapter 3.2 a detailed analysis of fragments m/z 43 and 44 is provided, I wonder whether the authors can exclude any contamination from combustion or other sources (e.g. the generator) on fragment m/z 43. Moreover, I agree with referee #1 in saying that figure 8 doesn't show any clear trend and the authors should reconsider their conclusions here.
- Figure 3 shows a nice agreement between the PM1 as measured by the AMS and SMPS+OPC, however the authors should mention which density values were used to calculate the total mass for this comparison.
- Figure 4 reports the total particle number concentration at the 2 measurement sites, I would suggest to add a second box and whisker plot showing only the concentration of Aitken mode particles.

References

- [1] Lisa Phinney et al. “Characterization of the aerosol over the sub-arctic north east Pacific Ocean”. In: *Deep Sea Research Part II: Topical Studies in Oceanography* 53.20 (2006). Canadian SOLAS: Subarctic Ecosystem Response to Iron Enrichment (SERIES), pp. 2410 –2433. ISSN: 0967-0645. DOI: <https://doi.org/10.1016/j.dsr2.2006.05.044>. URL: <http://www.sciencedirect.com/science/article/pii/S0967064506002025> (cit. on p. 2).

- [2] Mikko Sipilä et al. “Molecular-scale evidence of aerosol particle formation via sequential addition of HIO₃”. In: *Nature* 537.7621 (2016), pp. 4–6. ISSN: 0028-0836. DOI: 10 . 1038 / nature19314. URL: <http://www.nature.com/doifinder/10.1038/nature19314> (cit. on p. 3).
- [3] J. D. Allan et al. “Iodine observed in new particle formation events in the Arctic atmosphere during ACCACIA”. In: *Atmospheric Chemistry and Physics* 15.10 (2015), pp. 5599–5609. ISSN: 16807324. DOI: 10 . 5194/acp-15-5599-2015 (cit. on p. 3).