This manuscript presents 7 months of observations of aerosol precursor gases at the Finnish sub-Arctic field station SMEAR I. The authors combine their ambient pressure nitrate chemical ionization mass spectrometry measurements of sulfuric acid, methanesulfonic acid, iodic acid and the sum of highly oxygenated molecules with publicly available meteorological and aerosol data from the SMEAR I station. As the authors note, very few measurements of this nature and over such a long timescale are available to gain information on seasonal cycles of aerosol precursors in near-polar regions. For this reason, the present data set is quite interesting and the general analysis provide some interesting insights into aerosol formation chemistry. My main concern is that this analysis is somewhat cursory, and stops short of delving into detailed mechanistic analysis that such a data set can provide. However, I believe that this manuscript is deserving of publication in ACP given that the following mostly minor comments can be addressed.

We kindly thank you for your time to review our manuscript and we give you our detailed reply in the following sections in blue text.

Minor Comments:

(1) L104-105: what is meant by this statement? perhaps something is missing from the latter part of this sentence?

What we meant to say is that the chemistry behind NPF is never simple, even in pristine air. The sentences have been corrected now to clarify the meaning and says: However, the chemistry behind NPF is not simple, even in the pristine Arctic air.

(2) L143: what is the magnitude of the loss parameter in the 1 m stainless steel inlet line?

We completed the calibrations with the inlet tube so for this particular study we did not directly measure the loss parameter. From previous calibrations 1 m inlet line causes ~50% losses to sulphuric acid signals.

(3) L146-147 & L159-160: is the -50%/+100% uncertainty quoted only for sulfuric acid? what is the approximate uncertainty for non-sulfuric acid compounds for which the sulfuric acid sensitivity has been applied?

The uncertainty is given to represent all compounds mentioned in this study. HOM charging has been theoretically studied and it has been found that HOM charges in the nitrate CI-APi-TOF at kinetic frequency with NO_3^- (Ehn et al., 2014), similarly to SA (Viggiano et al., 1982). In the case of iodic acid,the proton affinity of IO_3^- is significantly lower than the charger ions and thus collision limit charging is again assumed (Sipilä et al., 2016). MSA has been observed to have the same calibration factor as SA in the nitrate-CIMS (Berresheim et al., 2002). If IA, MSA and HOMs would not charge at the kinetic frequency or close to it, it would lead to underestimation of concentrations in our manuscript. Thus the values reported in here are the lower limit estimations. This has now been clarified in the text.

L144-149 added a sentence:

If MSA, IA or HOMs do not ionize at the kinetic limit these concentrations could be underestimated and thus, the concentrations reported in here should be taken as lower limit values.

(4) L231: can the authors be more specific about what is meant by 'saturate'?

By saturation we mean that the concentration rises up very quickly and then stays stable for the rest of the day, almost like a flat line (= saturation), and then drops back in the evening. I used the word saturation because increasing concentration of a compound doesn't always lead to increasing signal in your instruments but the detectors saturate and give a flat line even when increasing concentrations of the compounds. In this light the wording seems incorrect and I changed “saturation” to steady state.

(5) L290-304: as the authors note in the latter part of this paragraph, does it make sense to speculate about long range transport of HIO3? What is the approximate lifetime of HIO$_3$? Presumably its lifetime is short, and long range transport appears a very unlikely possibility.

You are correct, the lifetimes of iodine species are short, in this case they are reciprocal of the condensation sink (tau = 1 / CS). Since IA is only a bit smaller molecule than SA, the same can be estimated for IA. In Värriö, where CS is around 0.001 s^{-1} (NPF days), the lifetime of IA should be around 15 minutes.

We did not speculate the long-range transport of IA, but its precursors, but the reviewer is in fact correct that the text isn’t well written to explain this in detail. The precursor of IA — e.g., CH$_3$I can be transported from the Arctic. This species has a lifetime of around 1 week and has both marine sources and terrestrial sources (Bell et al., 2002).

The text has been modified between L300-304 to:

“…suggests fast on-site chemistry, which is not consistent with long-range transport of iodic acid, but its precursor such as CH$_3$I (Bell et al., 2002). Also, iodic acid life time against condensational loss is expected to be short, in the range of ~15 minutes, this suggests that HIO3 is formed close to or at the site of measurements.”

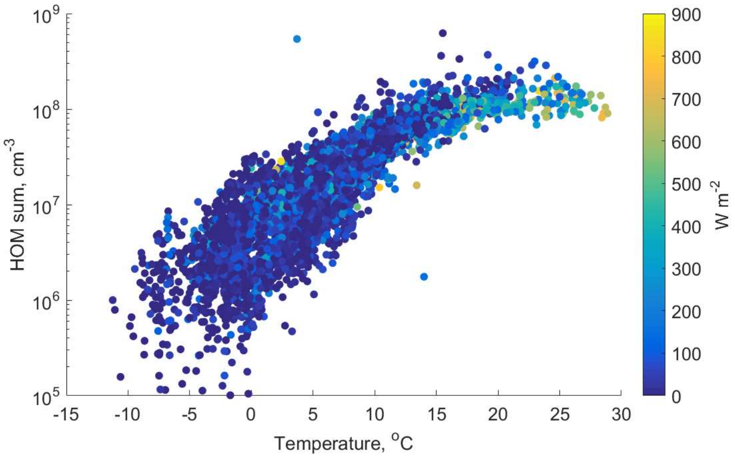
Bell, N., Hsu, L., Jacob, D. J., Schultz, M. G., Blake, D. R., Butler, J. H., King, D. B., Lobert, J. M., and Maier-Reimer, E., Methyl iodide: Atmospheric budget and use as a tracer of marine convection in global models, J. Geophys. Res., 107(D17), 4340, doi:10.1029/2001JD001151, 2002.

(6) L305-313: why focus only on the melting of snowpack? Isn't photochemistry in the snow pack a more likely source? (e.g., Raso 2017, as cited by the authors)

The photochemistry in the snowpack was mentioned right after the melting snowpack: “This is possible due to the deposition of sea salts on snow particularly during dark periods that activate during the spring and are re-emitted to the atmosphere through heterogeneous photochemistry of iodide, and iodate ions” but we modified the “melting snowpack” to just “snowpack” for clarity.

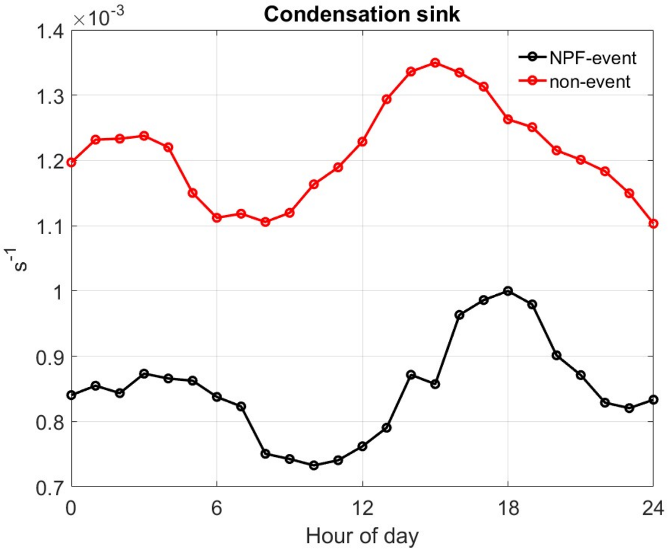
(7) Figure 5: if the relationship between HOM concentration and temperature is most significant here, why not plot temperature on the x-axis and color by radiation?

We did plot the requested figure, but since the solar radiation is mostly <400 W m^{-2} it doesn’t really show the effect of solar radiation. However, it does nicely emphasize the correlation between HOMs and temperature and this we added it in the manuscript as panel B) next to the original picture.



(8) Figure 7: a diurnal profile of the condensation sink calculated from DMPS measurements would also be informative here, rather than only plotting total aerosol number concentration. (i.e., are the polar marine air masses also cleaner in terms of existing particles before NPF events occur, or does the very large concentration of aerosol precursors overwhelm any differences in condensation sink between event and non-event days?)

With the total number concentration, we want to show that NPF is producing a significant number of particles at the site. However, condensation sink is also an important parameter to be considered when trying to understand NPF and thus, we calculated the condensation sink from the DMPS data and added the figure in the manuscript:



The figure shows that the condensation sink is about 30% higher during non-event days than the event days.

(9) Figure 8/10: why do the authors think the O_3 is higher when air masses arrive from more northern polar marine regions?

Since ozone is photochemically produced in the presence of NOx (and VOC), we think that it relates more to the abundant solar radiation and availability of these trace gases than the airmasses arriving from North.

(10) Figures in general: many of the figures have very small axes labels, and are at a resolution that is difficult to read, please enlarge axes labels and legends and improve the figure resolution in the final version.

Thanks for the feedback, we increased the font size from 14 → 16 in all figures. Hopefully that is sufficient.