

Comments on Aerosol Characterization in Canadian high Arctic, by Kuhn et al.

I believe the manuscript is well written and summarizes first set of results from measurements made in Canadian high Arctic, at a site that is mostly in the free troposphere.

However, I see one fundamental problem with the measurements of semi-volatile aerosol species (i.e., organic aerosols and ammonium nitrate) after heating the samples up to 20 degrees C. With ambient temperature at the site being only slightly above freezing, if at all, in Aug-Oct, heating of the sample by at least 15-20 degrees C can very well drive off most of the aerosol phase ammonium nitrate and most likely organics to the gas phase. Therefore, I'm not sure the measurements truly reflect the amount of organics and ammonium nitrate transported to the site as they are described currently in the paper. Since the authors have done a very good analysis on particle transmission through the inlet, I suggest they expand the modeling exercise to estimate how much of the semi-volatiles could have been evaporated in the sampling line with this temperature increase. Then, they should very clearly explain this artifact in the manuscript and reflect the effect of this artifact in the conclusions drawn from measurements of nitrate and organics (Currently the authors discuss this only briefly on P13418). Measurements of sulfate on the other hand should be robust and artifact-free.

I also think the manuscript lacks support in 2 other areas:

First, I would like to see some comparison of these measurements with other Arctic aerosol measurements in the US or Canada (for similar times of the year) just to put the current measurements in perspective.

Second, I would like to see more discussion on meteorological differences of the two highlighted time periods which resulted in transport of pollution to the site- but with very different footprints according to FLEXPART.

Overall, I think the measurements are important because the site has the potential to become a long-term measurement site in the high Arctic in free troposphere and help understand the transport of pollutants to the arctic.

I recommend publishing the manuscript after the revisions suggested above and below are addressed.

1. P13408. line 18, "...with 'an' average"
2. Abstract- specifically when reporting average values of organics and nitrate- explain the sampling artifact discussed above and note that these average values are lower limits by ***%.
3. P13412, line 25- note that 80 and 700 nm are in vacuum aerodynamic space.
4. What is the chopper duty cycle? Indicate this in the manuscript. Since a window within the poly-dispersed distribution of particles is chosen for IE calibrations, what do you think the uncertainty in size of particles in that window is and how

- does that affect the estimated IE? Do you imply that the fact that the poly method gives 10-15% lower IE values is because of this uncertainty in size of particles?
5. P13418- about nitrate signal- the authors hint here and also on P13420 that the nitrate signal is not really inorganic nitrate. I suggest using the observed ratio of 46/30 from mass calibrations to estimate amount of inorganic nitrate (left) on the aerosols and then assigning the rest of the signal to organics (either organic nitrate or other organics with interferences at $m/z=30$). This procedure was previously explained in Bae et al. (Interference of organic signals in highly time resolved nitrate measurements by low mass resolution aerosol mass spectrometry, JGR, Vol. 112, D22305, doi:10.1029/2007JD008614, 2007).
 6. P13419, Because it's likely that most of the 'nitrate' signal is from organics, I don't see an advantage of discussing correlation of sulfate and 'nitrate'. I suggest revisiting the correlations after calculating the 'excess nitrate' as suggested in (5). Furthermore, again because of the heating of the sample line, I'm not sure if good or bad correlation of organics or sulfate with inorganic nitrate will be any conclusive if no correction for evaporation of semivolatile species is considered.
 7. P13420, what collection efficiency was used to estimate mass of species by AMS? Since there are definitely episodes of highly acidic sulfate (or sulfuric acid) particles, the nominal CE=0.5 can not be applied.
 8. P13421, line 5, was the pToF nitrate mode based on $m/z=30$ or 46? If $m/z=30$ has a lot of interference from organics, then this 'nitrate' mode is again not inorganic nitrate.
 9. P13425, Ohio river valley is also a big contributor to SO_2 in eastern US. It is hard to see specifically in Fig 10 if the Ohio region has also influenced the trajectories, but it is worth zooming onto the map and verifying that (in this case, add it to line 19).
 10. P13425, I am confused about the oil refinery numbers presented here. If Siberia puts out ~9.8 Mbbbl of oil/day, how can the total Russian output be about half of that?! Are these two numbers mis-typed?
 11. P13425, Consider adding "...so losses to the atmosphere are larger than 'what would' occur with more ..." to line 24-25.
 12. Are there any gas phase measurements (like CO_2 , CO, NO_y , O_3 , etc...) that can be added to the current analysis when looking at back trajectories? That will greatly strengthen the conclusions drawn from FLEXPART in terms of anthropogenic influence on the transported pollution.
 13. Fig 2-3: I recommend adding measurements of ambient temperature, RH (or dew Pt) and sampling inlet temperature to the time series.
 14. I would remove Fig 5 and 6.
 15. Fig 9-11: It's very hard to see anything in the bottom panels. Consider having these in larger sizes.