**Atmospheric
Chemistry
and Physics
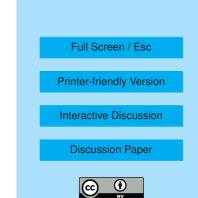Discussions**

Interactive
Comment

Interactive comment on "Seasonal features of ultrafine particle volatility in the coastal Antarctic troposphere" *by* K. Hara et al.

Anonymous Referee #1

Received and published: 4 July 2011

GENERAL

The paper presents aerosol volatility measurements at the Japanese Antarctic station Syowa and combines the obtained data with simultaneously measured aerosol chemical composition. The aerosol volatility measurements are probably the longest in Antarctica, are they? There are other aerosol volatility measurements, you could add some references. For instance, you don't mention that Ito has also made aerosol volatility measurements at Syowa, even though you have several references to Ito. You should write, how are your measurements different from these earlier ones. Anyway, the results make sense and being the longest volatility record the work is definitely worth publishing in ACP even though there is nothing drastically new in the conclusions.

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I consider that there is one point that should be analyzed more, however: the size ranges of the two SMPS systems. You are comparing the heated size distributions in the range 10 – 395 nm with the non-heated size distributions in the range 5 – 168 nm. This is somewhat confusing. In the larger particle size range (Dp > 168 or even larger than 395 nm) there may still be quite a lot of particles, especially when there are sea salt particles in air, you are close to the ocean. If these sea-salt particles are internally mixed with some more volatile material they may shrink into the size range < 168 nm and then the ratio that you call non-volatile constituent fraction gets overestimated because you calculate it from the ratio of the integrated number concentrations of the two SMPSes. There is also a small source of uncertainty in the lower range (Dp < 10 nm) during new particle formation episodes. But this latter problem is not significant in most cases.

I suggest you do following: compare the integrated number concentrations from the TSMPS with that obtained from the 3010 CPC (after cleaning out the local contamination), it gives the total number concentration without upper cutoff. You must have thought about this already yourself. Was there some technical problem with the CPC?

Actually, it would really be good if you could present also the total number concentrations from the 3010, the SMPS and the TSMPS at the lowest temperature 20°C in a time series and a scatter plot with N(3010) in the x axis and the other two in the y axis. This would come before you start presenting the actual volatility data, it would give information on the uncertainties involved.

The other way would be to calculate the ratios using only your TSMPS: the changes in Antarctica are generally not very fast, you could take the 20°C size distributions at every 280 minutes (or maybe interpolate two consecutive 20°C size distributions) as the reference size distribution and compare then the heated ones with that. In this way you would have the same size range – even if the uncertainty involved with shrinking particles from the size range > 395 would still be there.

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I don't think that the main results would change very much, but still these data processing ways would be more sensible than what you have done now.

DETAILED COMMENTS

P14780 Section "2 Measurements and analysis"

- Please add information about how far from the open ocean the station is in summer and winter even if this information is in other papers. This helps in the data interpretation in the later sections.

- Some additional information on the station: are there nunataks or bare soil nearby? This is important because soil dust is nonvolatile and you are making volatility measurements.

- How about your inlet, what is the D50, in other words, how big sea-salt particles get in?

- What were the CPCs of the SMPS and the TSMPS?

- What is the residence time of air in your oven of the TSMPS? This is also an important information because it has been shown that too short a residence time leads to underestimation of volatility.

- Did you ever calibrate your TSMPS by using ammonium sulphate or other volatile particles? This would be a good piece of information because all thermodenuders are somewhat prototypes, individuals.

P14784, L13: "Therefore, Cl− is identifiable as a sea-salt constituent". Sure. Where else could it come from, even in theory?

P14785, "The CN concentration increased remarkably under the storm conditions with strong winds." This is interesting. This observation is similar to what we found at Aboa, even though with a different instrument, see Virkkula et al. (2007) Bor.Env. Res. 12, 397-408, Figure 5. This is a suggestion for further analysis, I don't require this for

getting this paper published: You could make scatter plots with wind speed in the x axis and number concentration and non-volatile fractions in y axis. I would expect to see a good positive correlation between N and wind speed but not as good with non-volatile fraction and wind speed. Part of the increased number may come from the ocean as sea-salt particles but some may be wind-induced charged ions that grow to sizes detectable by the SMPS.

P14786 L19 – 24. You repeat the same thing in three sentences: sea-salt particles may act as CCN. This is true but one sentence is enough.

Finally: read carefully through the text, there are still some typing errors.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 14777, 2011.

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