Summertime aerosol volatility measurements in Beijing, China

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Anonymous referee's comments

General comments:

This manuscript reports results obtained during two field campaigns conducted in Beijing, China, in summer 2017 and 2018. The authors deployed a thermodenuder and a HR-AMS (plus an additional SP-AMS in 2017) to study the volatility distributions of NR-PM_1 species, as well as those of OA factors.

This is an important and well conducted study. Even if the manuscript is very descriptive, it will certainly be of interest for the readers of ACP. I would recommend publication of this manuscript after the authors address the following comments.

Specific comments:

1) Section 2.1 Sampling and instrumentation: The authors mention that measurements in 2018 were conducted at the Institute of Atmospheric Physics, but nothing is said about the sampling site in 2017. I assume it was at the same location.

2) Section 2.3 Source apportionment of OA: In addition to the description of the PMF analysis, the authors include already here a discussion on the main PMF results. I think that a part of this discussion should be moved into the section 3 Results and discussion.

3) Section 2.3 Source apportionment of OA: did the authors try to perform a PMF analysis by just using TD data from high temperature?

4) Section 3.1 Thermograms of aerosol species: I'm wondering whether the authors identified significant amounts of PAHs in the AMS mass spectra (at least if they measured mass spectra beyond *m/z* 200). These compounds can also have an impact of the volatility of OA factors.

5) Page 6, lines 21-22: Is there a correlation between K^+ and Cl^- signals in W-mode? It would also be quite interesting to know the evolution of NH_4 measured vs. NH_4 predicted as a function of the TD temperature.

6) Page 6, lines 22-29: The thermogram of sulfate for 2018 shows an increase of the MRF at 70-80°C, followed by a fast decrease. Is it possible that a part of sulfate was initially present in particles larger than 1 µm (so not transmitted in the AMS) at ambient temperature, then with

the evaporation, the size of these particles shrank until reaching a size measurable by the instrument? Do the authors have PToF data at different temperatures to check this hypothesis?

7) Page 7, lines 15-16: The authors claim that OA composition had considerable differences between 2017 and 2018. I do not agree with this statement. As shown by the authors just before, the OA composition looks quite similar between the two years.

8) Figure S1: It looks like the legend of the y-axis is wrong. This axis corresponds to the particle loss within the thermodenuder. “Mass fraction remaining” refers to the particle mass which remains after evaporation in the thermodenuder.

9) Figure S6: It does not really make sense to show the thermogram of $C_xH_y^+$ alone in a separate panel. I would suggest including it in the first panel, so that we see the evolution of $C_xH_y^+$, $C_xH_yO^+$, and $C_xH_yO_{gt1}^+$.