Characterization and source apportionment of organic aerosol at 260 m on a meteorological tower in Beijing, China

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General comments:

This manuscript reports results obtained during a measurement campaign undertaken at Beijing between October 2014 and January 2015. The authors deployed an Aerodyne ACSM and a few co-located instruments (SO_2 and CO analyzers, meteorological data) to measure the concentration and chemical composition of NR-PM_1 on the top of the Beijing meteorological tower (260 m). This 3-months field campaign was divided into 3 distinct periods: the APEC summit in the middle of the campaign, during which the Chinese Government implemented strict emission control at Beijing and the surrounding regions, a non-heating period before the summit, and a heating period after the summit.

I think that the authors have a very interesting dataset in the hands, and that the measurements performed simultaneously at ground level and at 260 m height can help to better understand the evolution, dispersion, and transport of different kind of particles as a function of meteorological conditions. I would recommend the publication of this manuscript in *Atmospheric Chemistry and Physics* after the authors address the following comments.

Specific comments:

1) Page 4, lines 14-15: Can the authors give some clarifications about the ACSM vs. HR-ToF-AMS inter-comparison they performed during two weeks? Why did they use variable scaling factors for the correction of the ACSM data? Did they also compare PMF analysis between the two instruments? It would be also interesting if the authors mention whether the bias between their two instruments are consistent with those reported by Crenn et al. (2015) and Fröhlich et al. (2015).

2) Page 4, lines 23-24: The FFOA factor is also characterized by a high *m/z* 57/55 ratio. Moreover, given that the PMF analysis has been performed with unit mass resolution data, how do the authors know that signals at high *m/z* correspond to PAH fragments?

3) Page 8, lines 11-19: The authors mention that the diurnal pattern of FFOA was very pronounced at ground level, and much more flat at 260 m (Figure 7). Is it possible that this result is due to frequent temperature inversions which prevent vertical dispersion of FFOA, exactly like for COA (page 8, lines 28-29)?

4) Page 9, line 30 to page 10, line 1: I don't think that a high f43/f44 ratio is an argument supporting that LO-OOA corresponds to combustion-related SOA. Indeed, previous studies showed that OOA factors with high f43/f44 ratios can also correspond to factors with biogenic influences (Fig. 4 in Ng et al., 2010; Setyan et al., 2012).

Technical comments:

5) Page 2, line 20: "from direct~~ion~~ emissions and secondary OA".

6) Section 2.3 (Positive matrix factorization): the authors named the two oxygenated OA factors "OOA" and "LO-OOA". Just to be consistent, I would suggest naming the first factor "MO-OOA" (more oxidized OOA) throughout the manuscript.

7) Page 5, line 2: I believe the authors wanted to say "a clear decrease of the ratio**s** of *m/z* 41/43 ~~to~~ **and** *m/z* 55/57 as a-value increases".

8) Page 8, lines 13-14: I would suggest saying "the FFOA concentrations at ground level ~~dropped~~ **started to drop** rapidly at ~3:00–4:00".

9) Table 1: The sum of the 5 PMF factors do not match the total organic concentration reported above in the table. I'm wondering whether the authors should scale the PMF factors to the total organics.

10) Figure 1: I would suggest using a darker grey to highlight the different periods of interest.

11) Figure 4: Is it possible to include the diurnal patterns of the wind direction and CO?

12) Figure 5: I'm not sure whether it makes sense to compare the time series of BBOA and LO-OOA with *m/z* 60 and *m/z* 43, respectively. They correspond to the most representative signals of these two factors, and thus cannot be considered as external tracers. For LO-OOA, I would suggest to do the comparison with the time series of Chl, given that they presumably come from the same source (coal combustion).

13) Figure 8: Is there a reason for which the pie charts for 260 m are bigger than those for the ground site? The sizes do not seem to be related to the average concentrations.

14) Figure S3: For each time series (right panels), I would suggest to add the average concentration obtained with each a-value, as well as the correlation coefficient (r^2) and slope vs. the time series obtained for a-value = 0. With this information, the reader will better see how the time series deviate from the unconstrained factors when the a-value increases from 0.1 to 0.5.

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