

## ***Interactive comment on “Vertically-resolved Characteristics of Air Pollution during Two Severe Winter Haze Episodes in Urban Beijing, China” by Qingqing Wang et al.***

### **Anonymous Referee #2**

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#### General comments:

This manuscript reports results obtained during two haze episodes at Beijing in November 2014 and January 2015. The authors deployed a set of instruments at ground level and on the top of the Beijing Meteorological Tower (260 m) to measure the vertical profile of a few selected parameters (light extinction coefficient, NO<sub>2</sub>, black carbon, non-refractory PM<sub>1</sub>, meteorological data).

This manuscript is very well written, and is totally relevant for the readers of Atmospheric Chemistry and Physics. I think that the manuscript in its current version is already in a very good shape. However, I have a few minor comments that the authors

C1

may consider before final publication.

#### Specific comments:

1) Given that the authors are comparing concentrations between ground level and 260 m altitude, I'm wondering whether they need to convert the concentrations in standard temperature and pressure (STP) before doing the comparisons. I know that when people compare aircraft measurements to ground level data, the conversion to STP volumes is very important. Here, between ground level and 260 m, I'm not sure whether the difference of pressure requires this conversion. Among all the parameters that were measured at both altitudes, pressure is the only one for which the vertical profile is not given in this manuscript. I would suggest that the authors include it in the supplementary material (for instance in Figures S1 and S2), and check whether it's worth adjusting the concentrations to standard conditions.

2) Given that the chemical composition of non-refractory PM<sub>1</sub> was measured with an HR-ToF-AMS at ground site and an ACSM on the top of the tower, I would suggest that the authors say a few words on their uncertainties. They can refer to the work of Crenn et al. (2015), who compared a set of 13 ACSMs with an HR-ToF-AMS. Then, the authors can check whether the differences in terms of concentrations and compositions observed during their study are significant, or whether they are within the uncertainties of the instruments.

3) Still concerning these two instruments, I'm curious to know how the authors performed the PMF analysis for the ACSM. Did they use the results obtained with the HR-ToF-AMS to choose the final PMF result for the ACSM (number of factors and fPeak)? I think that the authors need to add some evaluation plots for the 4-, 5-, and 6-factor solutions in the supplementary material, in order to justify the choice of the 5-factor solution for the two instruments. Here also, the authors can refer to the same inter-comparison between the HR-ToF-AMS and ACSMs (Fröhlich et al., 2015). In that work, the authors had noticed that some PMF factors can be quite difficult to separate

C2

in some ACSM datasets (especially the separation of COA from HOA).

4) Among the five PMF factors identified with the HR-ToF-AMS and ACSM, three factors correspond to primary particles directly emitted at ground level (FOA, COA, BBOA). I'm wondering whether the authors can do a comparison between their mass spectra (Figure S3), and check whether some specific signals changed significantly between ground level and 260 m (following photo-oxidation, for instance). I guess this comparison should be quite difficult, given that the instrument on the top of the tower was the ACSM (mass spectra in unit mass resolution).

5) When I take a look at the vertical profiles of temperature during the 36 periods (Figures S1 and S2), I notice a strong decrease of the temperature at high altitude for at least 23 of these periods. This kind of vertical profile can have an incidence on the gas-particle partitioning of a few semi-volatile species (I'm especially thinking about nitrate), which can condense more at high altitude. The authors can include a discussion on this in the manuscript, for instance on page 11, lines 5-6, where the authors mention a higher contribution of nitrate at 260 m.

Technical comments:

6) Page 10, line 28: The SSA values are given in a certain range (i.e. min-max) for Fall 2014 and  $\text{avg} \pm \text{std dev}$  for Winter 2015. Please choose one of the formats and use the same for the two periods, just to be consistent.

7) Caption of Figure 5: "260 m (top panel). The".

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C3

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C4