

## ***Interactive comment on “Summertime aerosol volatility measurements in Beijing, China” by Weiqi Xu et al.***

**Anonymous Referee #2**

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This paper presents aerosol volatility measurements in Beijing using a TD-AMS setup. Measurements are inverted into volatility distributions using an evaporation kinetic model. Volatility distributions are reported for various PMF-resolved OA factors. An improved understanding and characterization of atmospheric aerosol volatility is a topic of interest to many atmospheric researchers. The topic fits well within the scope of ACP. I do have some major comments about presentation, analysis, and discussions in this paper, which should be addressed before acceptance for publication.

Major Comments: 1. Overall, I found the discussions in this paper are limited and incomplete in many cases. It is difficult to identify what are the novel and interesting findings from this study. There are several similar studies exist in the literature. This paper seems another ambient volatility measurement in a different location. Most of

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the reported results are also similar to existing studies. It would be nice if the authors can focus a bit more on their novel findings and expand the discussion on it. For example, I found the volatility comparison with WRF-Chem simulation is an interesting part of this paper- since not many studies have done this type of model-measurement comparison. However, the discussion on this comparison is very limited. The authors should consider discussing this result under a separate section. Detail discussion on model-measurement comparisons such as model simulations/inputs, possible reasons for the discrepancy and their implications should be discussed. Also, the implications of their findings in terms of local and regional context should be discussed.

2. It seems the reported volatility distributions may not be well-constrained. They have collected TD data with three temperature steps (50, 120, and 250 degC) with a very low residence time (1.9s in 2017 and 7.4s in 2018). They have used the TD data during the temperature ramp period in their fittings, which seems problematic to me. Because the temperature profile inside the TD during the ramp period may not be in equilibrium. They reported that they had used the fitting method of Karnezi et al. (2014). Details on this should be provided. It is possible to derive hundreds of different volatility distributions by fitting the TD data. The effects of mass accommodation and vaporization enthalpies on the fitted results should be discussed. Ultimately, if their fitted distributions are not well constrained, then all subsequent comparisons among different OA factors and with earlier studies will not be meaningful. In Fig 6, considering the uncertainty, it is difficult to distinguish the difference between the volatility distributions of different OA components.

3. Throughout the paper (especially in Sec. 3.1, 3.2) they have used MFR as a basis for volatility comparison with other studies and/or different OA components in this study. Volatility comparison should not be made based on MFR or T50.

A few specific/minor comments: 1. Page 3, L25: Was the bypass measurement performed after drying? What was RH after drying? Did they characterize and consider particle loss through the dryer?

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2. Page 4, L5: Only about one week of data were collected in 2017. Given the different measurement setup and data collection duration, I found a comparison between two-year is a bit problematic. Authors should discuss these limitations. My concern is that they may not be able to resolve the "true difference (if any)" due to measurements limitations and fitting uncertainties and the reported results could be overstated.
3. Page 4, L7: Are the reported residence times plug-flow RT? It should be clarified.
4. A CE of 0.5 is used. Can the author show a mass closure using SMPS measurements (e.g., AMS+BC~ SMPS)?
5. Page 4, L20: Did they consider size-dependent particle loss in the TD? How do the size distribution of calibration particle (NaCl) and ambient particle compare?
6. Page 5, L18: What fraction of OA was BC-containing OA?
7. Page 5, L22: What particle size information was used for fitting? How did they measure it? Details should be given.
8. Page 7, L10: SOA= LO-OOA+MO-OOA. This may not be always true. They have used SOA and POA in many places, which is sometimes confusing. It is better to use the derived factor.

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