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Interactive comment

Interactive comment on "Organic aerosol volatility and viscosity in North China Plain: Contrast between summer and winter" *by* Weiqi Xu et al.

Anonymous Referee #2

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This manuscript used TD-AMS system to study the volatility of organic aerosol at three sites in North China Plain. Further, the inferred volatility distribution together with literature parameterization is used to infer the aerosol viscosity and glass transition temperature. While the data analysis is solid and the discussions are thorough, I find it challenging to grasp the key messages from the manuscript. As the volatilities of several OA factors measured at two sites (rural vs urban) in two seasons (summer vs winter) are contrasted in the manuscript, the discussions are rather scattered. I appreciate the authors' efforts to present such a comprehensive dataset, but I humbly suggest the authors to organize the discussions/conclusions in a more coherent and systematic way, or better emphasize the significant findings from this study, which will better convey the crucial findings and increase the impact of this study.

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Discussion paper

Major Comments 1. Besides re-organizing the manuscript, another suggestion I have which may distinguish this study from other similar TD-AMS studies is regarding the RH-dependent volatility of MO-OOA, which I find to be one of the most interesting findings in this study. It is intriguing why the volatilities MO-OOA and LO-OOA exhibit such RH-dependence. Although possible reasons are discussed, the authors are biased to "chemical composition" as stated in Conclusion section (Page 12 Line 13-16). However, the RH-driven particle diffusivity is another highly possible explanation. In fact, this hypothesis can be experimentally tested by humidifying ambient aerosol before sending it through the TD. If possible, this test should be included in the manuscript. Without providing further evidence, the conclusion that the composition and formation mechanisms of MO-OOA are different under different RH is not supported. 2. The fitting of OA volatility distribution based on MFR should be elaborated. I list several questions that confuse me. (1) Are the vaporization enthalpies and accommodation coefficients values fixed or treated as tuning parameters? If the former, how sensitive are the VBS to these parameters? Also, please elaborate why these parameters derived from another study are applicable. (2) Do the authors use the MFR under different T of the whole dataset to fit a campaign-average VBS, as shown in Figure 3? (3) How is C* of OA or OA factors calculated? For example, Page 7 Line 22 mentioned that the C* of OA in summer was 0.55 ug/m3. Is this volatility-bin weighted C*? Similarly, how are the effective vaporization enthalpies of OA factors calculated (Page 9 Line 2)? 3. Page 8 Line 2,3, etc. 0.75 vs 0.93 ug/m3. Please provide the uncertainty range of the estimated C* to justify if the comparison is significant or not.

Minor Comments 1. Page 12 Line 16. Typo. Replace "combing" with "combining".

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1105, 2020.