

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2022-575

Anonymous Referee #2

Referee comment on "Impact of aging on the sources, volatility, and viscosity of organic aerosols in Chinese outflows" by Tingting Feng et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-575-RC1>, 2022

General comments:

The manuscript by Tingting Feng et al. investigated the aging impact on sources, volatility, and viscosity of organic aerosols in Dongying. They found that the BBOA was the most volatile OA factor, followed by aged-HOA, transported-OOA and background-OOA. In addition, the estimated mixing time of OA varied dramatically from minutes at night to years in the afternoon, emphasizing the necessity to consider its dynamic kinetic limits when modeling OA. The topic fits well within the scope of Atmospheric Chemistry and Physics. This manuscript is generally well written. Before its publication, the following comments need to be addressed.

Specific Comments:

1 More information needs to be provided to support source apportionment results in the background-OOA and transported-OOA. For example, the background-OOA did show a relative flat diurnal variation. However, the elevated loading of background-OOA was occurred after excluding the impacts of PBL. In addition, the lower O/C of transported-OOA compared to background-OOA was observed. Is such difference in the oxidation state one of the reasons that you name background-OOA and transported-OOA? If so, please list the references and explain the reasons. In addition, is there any other evidences to support that factor 2 is related to the ageing of HOA (rather than COA, CCOA or other primary emissions)? What about the correlations of aged-HOA profiles in this study with the aged traffic emissions in laboratory studies?

2 Are there any specific reasons for using a constant CE (0.5), rather than CDCE? How about the neutralization in ambient air and each TD temperature? Please mention it here.

3 How did you measure the organic nitrate? Did you exclude the impacts of organic nitrates on the measured/predicated NH_4 in Fig. 6(f). I am also curious the lower mass concentrations of organic nitrate in the polluted periods compared to entire periods. Please elaborate.

4 The discussion regarding oligomers should be backed by the evidence rather than speculated upon at all throughout the manuscript.

5 Did you assess how long it takes to reach stability after switching? In my viewpoint, there might be a significant uncertainty using the switched time of 4 min. The authors need to address such uncertainties in the revised manuscript.

6 Are there any other metal containing constituents measurements to support your assumptions (e.g., line 365-367)? Looking into the HR data(e.g., Na^+ , K^+ and Pb^+) would be helpful.

7 The author have not discussed the inlet used for dilution fully. An experimental design for dilution needs to be included in Section 2. In addition, a discussion about the additional mass losses of four OA factors as functions of the dilution factor is irrelevant for the section 3.4.2.

8 Line 116: Inaccurate " PM_1 ". Do you mean " NR-PM_1 "?