Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2022-142-RC1, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on acp-2022-142

Anonymous Referee #1

Referee comment on "Measurement report: Large contribution of biomass burning and aqueous-phase processes to the wintertime secondary organic aerosol formation in Xi'an, Northwest China" by Jing Duan et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-142-RC1, 2022

General comments:

This paper uses an SP-L-ToF-AMS to study the OA sources and SOA formation in urban Xi'an during winter 2018. The authors used AMS source apportionment techniques to study the contribution of different types/sources to OA and performed correlational analysis to identify key factors on the observed trends of different OA. They further focus on OOA derived from biomass burning (OOA-BB) and aq-OOA from aqueous reactions. In particular, aq-OOA was found to be dependent on SIA content and ALWC. The use of AMS type instruments for source apportionment analysis and the identification of SOA is quite routine now. POAs (HOA, COA, CCOA, BBOA etc) have been regularly identified. The observations of different types of OOAs, their VK plot characteristics and OSc trends are also widely reported. While the paper is a robust AMS "Measurement Report", I am most interested in the interplay between sulfate and nitrate and SOA formation, especially under different environments, BBOA dominant vs. aqueous phase chemistry dominant. Below I highlight a few comments for the consideration of the authors.

Specific questions /issues:

- Page 4 line 135, what is the justification of using C2H3O+ as an identifier of OOA-BB? Also, the close relationship between BBOA and OOA-BB without much time lag can also

mean that the OOA-BB measured could be a result of oxidation occurring very close to the source of the burning instead of the result of atmospheric aging under "environmental conditions". Can the authors examine their data in more details or literature to address this issue? My main concern is that atmospheric aging does take time and a time lag is expected.

- Page 7 line 213, did the SOA concentration increase? An increase in SOA% could be due to lower POA.
- Page 7 line 221, what is the typical time scale for such BBOA reactions to form OOA-BB? Is it reflected in the correlation of time-lag profile of OOA-BB with time profile of BBOA?
- Page 10 Fig.4, why was the slope larger at low RH? It is interesting and the authors should explain. The size of the symbols is meant to show the SIA concentration. But what is the scale?
- Page 10, line 295, as sulfate and nitrate increased, the aqOOA also increased to the same high level at some different ALWs. So, within certain range of ALW, its increase correlates with aqOOA increase. But at a very large increase of ALW, it does not give much higher max of aqOOA. What are the reasons for this?
- Page 11 Fig. 5, the summer data are from Duan et al., (2021) should be stated in the captions. Also in Fig. 5, what are the scales for nitrate and sulfate concentrations?
- More discussions on the role of sulfate and nitrate in Aq-OOA formation are needed. Do you see sulfate and nitrate playing other roles, e.g. nitrate photolysis, beyond ALW in the formation of Aq-OOA? Do you see a difference between daytime and nighttime trend?
- Similarly, do you see any correlation between BBOA and sulfate formation? There are a lot of recent interesting work suggesting that BBOA can form sulfate during its atmospheric aging.