

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2021-1033

Anonymous Referee #2

Referee comment on "Highly time-resolved chemical speciation and source apportionment of organic aerosol components in Delhi, India, using extractive electrospray ionization mass spectrometry" by Varun Kumar et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-1033-RC2>, 2022

The paper by Kumar et al. is a summary of in-situ organic aerosol measurements in Delhi using different mass spectrometry techniques- one using hard ionization by electron impact (HR-ToF-AMS) and one soft ionization by extractive electrospray (EESI-ToF-MS). Measurements were carried out over ~ 1 month in winter to determine sources of organic aerosols in Delhi. Although recently there's been other papers on aerosol measurements in Delhi, this work is the first one with an EESI-ToF-MS deployment, allowing the authors to expand on source apportionment of the SOA fraction. The results summarized in Figure 4 are very interesting and critical for designing strategies to improve air quality in Delhi. The analysis related to estimating daytime production fractions of the different factors is also very interesting. The paper overall is very well organized, and the data are nicely presented. I support its publication after the following minor concerns are addressed:

Technical:

Page 8, L25: what is the reason behind removing signals related to CO_2^+ , rather than down-weighting them?

Page 8, L36: When taking the HOA profile from the 8-factor solution set, do you mean you used SoFi to constrain the HOA factor? Similarly, on page 9, L 25, do you mean SoFi was used? It may be better to directly mention SoFi in these instances as well.

Page 9, L26: how do we know the 5 factors from the 10-factor solution set were not mixed and reasonable to be assumed as pure reference profiles?

Page 11, L7: Equation 7 does not assume that activity coefficient is 1. Do you mean in your calculation of C^* , you assumed it's one?

Figure S2 and page 16, L 4-5: Not having worked with EESI-ToF, I'm not sure how much fragmentation one gets. My understanding has been that it's a pretty soft ionization technique. If that's the case, I don't know how to interpret seeing signal at similar ions for both aromatic and biogenic SOA. To reconcile this, do you mean that the common signals are due to fragmentation?

Page 16, L 28: My first thought after reading that BSOA was high at night was that it's NO₃-driven SOA. Later on, you mention that because of the high NO levels in Delhi you don't expect much of NO₃ formation. Are NO levels so high that they titrate O₃ completely such that NO₂ conversion to NO₃ is not possible? Even if that's the case, I think the potential for NO₃+BVOC oxidation at night should be mentioned here.

Page 18, L 17: it is not clear to me what you mean by GBRP-based sensitivities in a relative sense. Please explain more.

Page 18, L 31: case 1 still excludes the data from Jan 3-4, right? If so please indicate that clearly here too.

Editorial:

Page 5, L2: do you mean PM₁₀ and PM_{2.5}, separately rather than respectively?

Page 11, L34: Please change to $C_xH_y^+$. Similarly, Page 13, L 11, please change to SO_4^{2-} and on Page 18, L 14, change to $C_xH_yO_z^+$

Figure S1: Consider changing SVOOA to LO-OOA to be consistent with other parts of the paper

Page 19, L 34: For completeness, please define ROS

Page 21, Line 5: Either change to "...enabled apportioning the" Or "...enabled apportionment of the"

Figure 5. Figure caption indicates colors of the background that are different than the real colors for secondary and primary factors.