



EGUsphere, referee comment RC2
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Comment on egusphere-2022-1318

Anonymous Referee #2

Referee comment on "Chemical identification of new particle formation and growth precursors through positive matrix factorization of ambient ion measurements" by Daniel John Katz et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-1318-RC2>, 2023

This study used APi-TOF to measure the ambient ion chemical composition with the newly developed bin-PMF method and scaled Kendrick mass defect. In general, the manuscript is well-written and within the scope of ACP. I would recommend it be published as a measurement report after some minor revisions.

Major aspects

- In the introduction section, the authors focused too much on the background of atmospheric ion chemistry. Some discussions related to the gaseous vapors from agricultural regions in previous literature are needed. The authors should clearly state the remaining questions related to that field. I noticed some of such discussions are in the results sections but they should also be summarized in the introduction section.
- It is interesting to see that HSO₄⁻ signal is much lower than (H₂SO₄)HSO₄⁻ signal at the measurement site, which is quite different from the existing atmospheric ion studies (e.g., Ehn et al., 2010, Bianchi et al., 2016, and Frege et al., 2017). The HSO₄⁻ and NO₃⁻ are typically the predominant ions in the negative ion spectra determined from APi-TOF observation in the aforementioned studies because of their relatively abundant parent neutral species (HNO₃ and H₂SO₄ in the day). Thus, a detailed explanation/discussion may be needed here, especially since the authors mention that fairly abundant HNO₃ concentrations are expected (line 233) based on the PMF results.
- The authors observed evident levels of C₁₅ compounds (CHOs and CHONs) and attribute a substantial fraction of them to sesquiterpene-related HOM in the "high m/z nitrate factor". While a substantial level of sesquiterpene can be emitted from the herbaceous crops around the measurement site, direct observational evidence (e.g., sesquiterpene measurements) may be needed to support such a conclusion.
- Similarly, the authors conclude that more insight into NPF precursors can be provided by applying bin-PMF based on APi-TOF dataset. While this may be true, no NPF event has been observed/reported in this study. More evidence is required to support such a conclusion. Some background information such as pre-existing particles and NO_x can also be added in SI.
- As the authors suggested, the bin-PMF could be a very useful tool in analyzing APi-TOF

data in the future. The authors should explain more clearly how it is conducted with ToFware, which could be quite important information for potential ToFware users. Also, the peak list of the ions, the corresponding neutral species, their potential sources, and the average mass spectra from different trajectory sectors would also be needed in SI.

Other comments:

Line 229: What is the reason for the nighttime HSO₄⁻ signal?

Line 357: What is the diurnal variation of the total ion counts of the observed negative ions?

Figure 5: The difference between the signal fraction of each m/z is not evidently shown in the figure. A different color code in the log scale is recommended here.

Line 482: Why m/z 240 is removed before doing bin-PMF? A further discussion is expected here (Section 3.4) according to the method section (Line 131).