



EGUsphere, referee comment RC3  
<https://doi.org/10.5194/egusphere-2022-1318-RC3>, 2023  
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## **Comment on egusphere-2022-1318**

Anonymous Referee #3

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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-RC3>, 2023

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### General comments

This manuscript presents ambient ion composition measurements from the Southern Great Plains. The analysis does not lead to very far-reaching conclusions, but ambient ion composition is a result of a multitude of factors, and as such analyses tend to be more qualitative than quantitative. That being said, the analytical methods are very appropriate, the results are presented in a clear and logical fashion, and the conclusions are to the point. I enjoyed reading the manuscript and only have a few minor comments for the authors to consider before final publication.

### Specific comments

- Line 32: Does one of the references discuss examples of how ions can “affect neutral gas-phase chemistry through ion-neutral interactions”? It does not sound like a very common reaction mechanism, and therefore it would be interesting to hear more explicitly what kind of reactions are referred to here.
- Line 102: Would be useful to state also the length of the 10mm tube, though I expect it to be very short.
- Line 105: The clog of the inlet is acknowledged, and the impacts are discussed in different parts of the manuscript. While I don’t expect that there will be any impact on any of the conclusions in this work, it would be useful to add a bit more discussion about the topic. In particular, since on line 131 in the SI it is stated that the primary effect would be lowered signal, I feel that there is a risk that readers might underestimate the potential severity of such a change. For one part, if the SSQ pressure is not controlled to be constant, a change in inlet flow would change the

pressure in this (and subsequent) chambers, potentially causing large changes in mass-dependent transmission and declustering strength. The authors should state whether this pressure was controlled or not. Even in the case of an unchanged pressure, the dynamics of the expansion from the pinhole will change, and potentially again affecting the same properties (in addition to the absolute signal strength, which the authors already mention).

- Lines 122-125: While I am quite familiar with these subjects, I still have a hard time understanding the message of either of these two sentences, in particular the second one. Please try to reformulate. Is "stable isotopes" the correct term here? Why exactly is redundant information minimized?
- Line 136 onward: Since the authors clearly know the peak shape and resolution functions from Tofware, it is not clear to me why these are not used, instead of the method outlined in this paragraph. I expect the results would be nearly identical, but I feel it would be good state the reason for this approach.
- Lines 149-150: This is stated as a general truth here, but there is no a priori reason why this should always be the case. If this sentence was a conclusion based on the authors own data, it should be said more clearly. I also think there is a word missing in the sentence.
- Section 3: Concerning the naming of factors, I believe the "nitrates" factors are named based on them being clusters with NO<sub>3</sub><sup>-</sup> (?), but "nitrates" is also used in relation to organic nitrates. I suggest to go through the manuscript to make sure that every usage of "nitrate" is unambiguous concerning which type of nitrate compound is meant. I also suggest to avoid using terms of the type "negative/positive binPMF factors". Since the "P" already stands for positive, there is the risk for confusion. I suggest to always include the word "ion" or "mode" after positive/negative. This includes the SI, where e.g. "positive binPMF" is used several times.
- Line 257: "Formulas are presented as clusters with the nitrate anion". Do you mean that you did not remove the (potential) NO<sub>3</sub><sup>-</sup> from the given formulas?
- Lines 292-293: This sentence confuses me. Organic nitrates can also be HOM, so what is actually being compared?
- Lines 296-297: This conclusion may be true, but it may also be due to the ON not clustering as efficiently with bisulfate, or that bisulfate just prefers to cluster with other species.
- Line 302: The lack of dimers is very intriguing, and I would like to see some speculation on the reasons. To my knowledge only NO has been shown to kill dimer formation, but since the authors suggest NO<sub>3</sub> oxidation to be important, the NO should be very low.
- Lines 400-402: If pyridine has a lifetime of 40 days, it should be quite well mixed vertically as well, so why would BL dynamics deplete the signal?
- Lines 455-459: I don't understand why this scaling will result in differences between odd and even masses.

#### Technical corrections

- "SGP" is used in the abstract before it is defined.
- Line 128: The use of "either" seems misplaced?