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Atmospheric Chemistry and Physics
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Interactive comment on "Insights on Atmospheric Oxidation Processes by Performing Factor Analyses on Sub-ranges of Mass Spectra" *by* Yanjun Zhang et al.

Anonymous Referee #2

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This manuscript presents binPFM (Positive matrix factorization) analysis results of sub-ranges of mass spectra and combined ranges of mass spectra, respectively. The authors compared the results from three sub-ranges and the combined three, and concluded that the PFM results depended on the volatility of the species that is assumed to be identical among species, the chemistry or source that contributes to a particulate range of species, and the relative abundance of different species. The authors also discussed the potential formation mechanisms of observed species, especially dimers formed from peroxy radicals. Generally this is a very interesting study that clearly shows the potential issue when applying the PMF methods to measurements of volatile organic compound with different volatilities, which is of interest to the at-

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mospheric chemistry community. On the other hand, the manuscript is a little bit too technical for Atmospheric chemistry and physics, but can be revised to fit. I would recommend publication of this manuscript after the following concerns have been addressed.

1. Overall, this manuscript focuses too much on the method itself but does not put enough weight on the science they have obtained by analyzing the dataset. The current organization is more like an AMT paper instead of an ACP one. The authors are advised to move a certain fraction of the technical part, e.g., the contamination session, into the supplement and expand the scientific findings.

2. The texts in the conclusion part are quite redundant and just a repeat of the issues with applying traditional PMF to CIMS data, especially in the first two paragraphs. This part certainly can be rewritten to be more concise and to deliver key conclusions only.

3. (Line 121), “six different pathways” would not be the best word, since OH and NO3 chemistry would not generally happen at the same time. Although OH radicals can be generated from pinene+ ozone chemistry at night, the chances for cross reactions of dimers between peroxy radicals formed from OH chemistry and those from nitrate chemistry are just low, in my mind.

4. (Line 220-232), a couple of statements should be clarified. There is a statement of a bin width of 0.02 Th (Line 221). On the other hand, authors state “25 bins per unit mass” for Ranges 1 and 2 and “30 bins per unit mass” for Range 3. What caused the difference? Also, I assume that a larger range of signal region for Range 3 in further analysis was due to a worse shift in mass-to-charge? Lastly, what is the setup for the combined range analysis?

5. (Sessions 3.2-3.5), since the authors started from two factor analysis to more factors, point out the sequence of factors presented does not necessarily correspond that in the figures in each session.

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6. (Figures 2-5), state the time period for the factor contribution.

7. (Line 361-362, 455-456), isn't it true that the ultimate source of NO during daytime is still emission?

8. (Line 435), the termination of one peroxy radicals with another does not necessarily have to lead to the formation of dimers.

9. (Line 497-499), rephrase the sentence.

10. (Session 4.2.1) Although there might not be a daytime factor previously, it is not surprising that dimers are formed in the day. NO channel competes with the self reactions of peroxy radicals but which level of NO will really dominate is still an open question.

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