Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC4
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Comment on acp-2021-280

Anonymous Referee #4

Referee comment on "Measurement report: High contributions of halocarbon and aromatic compounds to atmospheric volatile organic compounds in an industrial area" by Ahsan Mozaffar et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-280-RC4, 2021

Approximately 100 speciated VOCs, measured by GC/FID/MS, are reported from a Nanjing industrial area in China from July 2018 – May 2020. The non-continuous measurement periods include field data from summer, autumn, winter, and spring. This measurement report focuses on the inclusion of select halocarbons and oxygenated VOCs to a "total" VOC (TVOC) measurement, which is then compared to past TVOC studies in Nanjing and other Chinese cities. The authors performed data analysis techniques including PMF, PSCF, and photochemical box modeling on this data set to assess potential VOC sources and the impact of these VOCs on local ozone production.

General Comments

I note that the authors have already heavily revised their manuscript in response to previous Referee Comments (e.g. PMF results and discussion, exclusion of VOC ratio and ozone formation potential discussions) and this review is primarily based on the revised version.

The observations reported here, speciated and quantified VOC composition in Nanjing, wuold be a useful resource for the atmospheric research community. While the measurement techniques (described in Mozaffar et al. 2020, Atmospheric Research) employed for this study are appropriate and appear well done, some of the analysis and interpretations included in this measurement report could be refined. The following are some areas for concern that they authors might consider revising:

- Throughout the manuscript there is continuous discussion and comparison of VOC compositions measured in various cities from different studies. Here the authors

compare observations of certain classes of VOCs by describing them as "%" (e.g. lines 75 – 91). I find this to not only be confusing, but also not a useful metric, as each study did not measure the same suite of VOCs. For example, comparing this studies % contribution of alkanes to the total VOC measurement to another studies is not useful if the other study was not measuring the same total VOC list. I would recommend that the authors revise the manuscript throughout. If they would like to directly compare their measurements to previous reports, they should do so on a concentration basis. Until this comparision is revised it is difficult to gauge how the measurements reported here compare to other areas.

- Along with the above comment, the use of the term "TVOCs" throughout this manuscript I find to be troubling. The TVOC measurement here is a sum of the suite of VOCs measured, but it is not a total VOC measurement. This term is especially frustrating in section 3.2, lines 242 – 263, where the authors compare their TVOC with that from other cities where entire groups of VOCs (e.g. halocarbons) were not included. The manuscript should be revised with care to make sure that any quantitative comparison with previous studies is "apples to apples" (even if that means that not all of the VOCs reported in this study are included in a specific comparison in the discussion).

Specific Comments

- In section 2.4, which VOCs were used to model the impact of their reduction on ozone? The text (line 175) says 11 VOCs but does not list which ones or the authors' reasoning for those choices.
- In section 3.2, lines 224 – 228, the authors attribute a higher contribution of OVOCs in summer/spring to enhanced biogenic emissions. However, the concentration of the reported OVOCs (figure 2c) are fairly constant throughout the year (or even reduced in the summer). It appears the higher contribution of OVOCs in spring/summer is due to the reduction of other classes of VOCs (e.g. halocarbons).
- The spring portion of the data set is from April 2020, could the authors provide comment on whether they view this measurement period to be representative of a typical spring in Nanjing or not due to differences in daily operations due to the Covid pandemic.
- The conclusion section should be re-written for clarity. The authors have numerous statements that are either redundant or grammatically incorrect.

Technical Corrections

Line 115: The authors should cite the supplemental from Mozaffar et al., 2020 here to give the reader a resource the GC/FID/MS technique, since the validity of the data included in the report relies on the quality of the analytical measurement

Line 124: There are two Mozaffar et al., 2020 references in the list. They should be clarified as "a" and "b"

Line 149: I believe the notation for reaction rate constant with OH should be "$k_{OH,i}$" generally using a capital "K" is for equilibrium constants.

Line 217-218: Which citation are you referring to? There are two Nanjing studies included in Table S2.

Figure 2: It would be helpful if the figure spacing was revised so that it is clearer that the TVOC and alkane data are on their own axis.

Figure 3, Figure 4: It could help the comparison if all left and right axes were kept to the same range.

Revised Figure 6: Keep color scheme consistent between pie graphs

Table S2: Missing concentration units (ppb)

Throughout: The manuscript should be edited for grammar, spelling errors, and redundant sentences.