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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurement report: Characteristics and sources of non-methane VOCs and their roles in SOA formation during autumn in a central Chinese city" *by* Haixu Zhang et al.

Anonymous Referee #2

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Zhang et al. performed online and offline VOC measurements using a PTR-MS and canister samples, respectively, in a central city in China. They compare their measurements to historical data, perform a factor analysis to identify primary VOC sources, and comment on the potential of the VOCs to form SOA. Based on a preliminary analysis, they find BTEX and C3 carbonyls concentrations to be elevated compared to measurements elsewhere, solvent evaporation to be an important source for the measured VOCs, and benzenoid species to be important for SOA formation in the urban environment.

There is a clear need for detailed online VOC measurements, on which analysis similar

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to those performed in this work would lead to an understanding of the primary sources to air pollution in populated cities. Hence the work is well motivated and the measurements are likely to be valuable to the atmospheric community. However, the analyses performed on the data are insufficiently described in terms of the methods and results. I generally recommend publication of this manuscript after the authors have had a chance to review my broad comments and responded to it.

Overall comments:

1. Measurement report: This article is submitted as a 'measurement report' but it does not seem to follow the data policy outlined on the ACP website: 'The data presented in measurement reports must be openly accessible in accordance with the EGU data policy.'. The data availability section in this paper currently reads 'The observational data in this study are available from the authors upon reasonable request (zhanghaixu@tsinghua.edu.cn).'. Please make sure that the data are available before final submission.

2. Data analysis: My understanding is that the measurement reports 'may include model results and conclusions of more limited scope than in research articles'. The authors, however, have begun to undertake a modest amount of analysis on the VOC measurements to study the source contributions, regional dependence, and implications for SOA. However, I do not think the analysis methods are presented clearly enough and/or the analysis results are described in detail for me to consider them complete; more detailed comments below. My interpretation of 'limited scope' in the context of this paper is to perform a 'complete' analysis for one of the tasks (e.g., source apportionment based on a PMF analysis) rather than a 'light' analysis for many tasks. In case this article is published with its current structure, it should be made clear that the analyses presented in this work are preliminary and will be continued to be worked on before they are published in their final form as a companion research article.

3. PTR-QMS: I know of a few earlier studies that have used a quadrupole PTR-MS

to measure VOCs in urban environments (e.g., Warneke et al. (2012)). As the VOC species are measured at a unit mass resolution in a PTR-QMS, are the species identified and quantified in this work free from interferences? Depending on the answer to the interference question, can the data be used to perform PMF or would the interferences confound the source apportionment? For the reader's sake, can the authors cite earlier work that have used the PTR-MS measurements in the same manner as they are used here and state any limitations in performing such analysis? In addition, I am not sure how secondary production, in general, is resolved in PMF when both primary and secondary species are present in the same factor and when species within a factor have different reactivities.

4. Confusing phrases: Throughout the manuscript, there are lots of instances where it was very hard to understand what was being said. The technical communication needs to be substantially improved in the revision.

Detailed technical comments:

1. Line 23: what does 'obviously' mean?

2. Line 26: what does 'high levels' mean? A recurring comment is that the authors describe trends qualitatively, which doesn't help with the interpretation.

3. Line 28: Are benzenoids only reduced single-ring aromatics in this work? If yes, please specify that or use 'single-ring aromatics'.

4. Line 58: '20-30%' of what? Another recurring comment is the text does not provide context for what is increasing or decreasing and the reader is left guessing (sometimes incorrectly).

5. Lines 68-71: That the 2-product or n-product (i.e., volatility basis set) SOA model has not been used to interpret field data is misleading. There is extensive literature over the past two decades that points to the use of 2/n-product models for field analysis (e.g., Dzepina et al., 2009).

6. Line 81: What are these ‘differences in different regions’? Are there some universal trends in ranking of the sources?

7. Line 93: What is ‘autumn’? Why not just specify the dates?

8. Line 123: ‘as focus or tracers are’? Please correct.

9. Sections 2.2-2.3: My general sense is that the detailed provided in these sections is not sufficient, especially if the reader is not familiar with the techniques.

10. Line 187-188: My sense is that PSCF and CWT need to be redefined in the text, separate from the abstract.

11. Lines 210-220: The other technique I have seen is the toluene/benzene ratio. Was there a reason why isoprene, MACR, and MVK were used to deduce OH exposures? Also, are MACR and MVK produced from photooxidation of other biogenic and alkene VOCs?

12. Section 2.5: This section is very confusing. For instance (not meant to be exhaustive), why is the OH calculated using equation 11 when Section 2.4 did the same thing? Where are the OH production rates calculated in equations 8-9 used? Why are the OH values then ‘set’ on lines 247-248?

13. Line 257: Avoid ‘useless’.

14. Figure 2: Do the earlier measurements use the same/similar instrument as used here?

15. Line 296: ‘slighter’?

16. Line 299-301: What is the point of this statement?

17. Line 314: How is ‘haze’ defined? Is there a textbook definition that is being used?

18. Lines 318-322: Unclear what this sentence means? ‘front zone is in accordance with the increasing phase’? ‘decreasing phase of pollution’? ‘pressure pattern can be

clearly observed and used to explain the accumulation process'? All of these phrases make it extremely challenging to understand what is on the authors minds.

19. Line 334: What is the 'diurnal variation law'?

20. Line 351-353: 'the temperature influence on the evaporation rate can offset the daytime valley'. Not sure what is implied here.

21. Lines 360-362: Why aren't single-ring aromatics linked to a traffic source?

22. Line 368: Why was traffic restricted during the sampling time?

23. Line 404: What is the 'real situation'?

24. Line 407: Is the VOC/NOx ratio limited to the VOCs that were measured by the PTR-MS? If yes, say so.

25. Line 430: How can POA be oxidized in the gas-phase when POA is in the particle phase? Do you mean the semi-volatile vapors in equilibrium with POA?

26. Lines 484: Do you expect secondary production to be high with OH concentrations being so low (<10ˆ6 molecules cmˆ-3)?

27. Lines 500 onwards: Were attempts made to explain the OOA (or SV-OOA) mass concentrations based on the SOA precursors measured by the PTR-MS? Are other SOA precursors (e.g., intermediately volatility organic compounds) relevant here?

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