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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurement Report: important contributions of oxygenated compounds to emissions and chemistry of VOCs in urban air" *by* Caihong Wu et al.

Anonymous Referee #1

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PTR-ToF-MS provides a powerful tool to measure VOCs in the atmosphere, and analysis of the full mass spectrum dataset is rare, particularly for cities in China. This paper presents a detailed analysis of VOC observations made by PTR-ToF-MS and GC-MS/FID. It confirms the importance of oxygenated VOCs in the urban atmosphere: nearly 80% of PTR ions, or 60% of total VOCs, 20% of the total OH reactivity. It also suggested that about only 12% of the OH reactivity and below the instrument uncertainty, is not accounted for in the deployed instrument payload ('Missing' reactivity). The paper is in general carefully written and is of interest to the ACP community. Thus I'd recommend a publication. A few specific comments are:

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- Page 4, line 95: It is an overstatement and vague. At least one study has reported the importance of OVOCs in the urban air, and another in review (see below). Nevertheless, it is still quite new work.

- The mass resolving power of the PTR-ToF-MS: what is the resolving power to separate isoprene and furan for this instrument?

- Section 3.1. The overview of selected VOCs is a weak section of this paper. The interpretation of VOC sources here is oversimplified. For example, OVOCs only came from photochemical/secondary sources, while the biogenic source can actually play a significant role (See its own Section 3.3). Secondary formation of furan to explain the nighttime decrease. It could be due to the nighttime transport pattern or nighttime source variation. I would suggest a revision of this section and focusing on the large pattern rather than going into too specific details and simplifying the explanation, which in many cases could be wrong. Just leave the source attribution to Section 3.3, or discuss the consistency of results between the two sections/analyses.

- Section 3.2. Can the authors comment on why Sekimoto et al (2017) method didn't work here for the instrument here? Is there anything about the affected ion chemistry in the PTR-QiToF-MS. This could be important for the PTR-MS community for quantifying many VOCs without calibrations.

- Can propagated errors of uncalibrated species be estimated here? How would those errors affect the conclusions?

- Page 15, lines 401-402: For OH reactivity, LOH may be a better symbol as KOH would suggest it is a rate constant, but it is actually a loss rate.

Reference: Karl, T., M. Striednig, M. Graus, A. Hammerle, and G. Wohlfahrt (2018), Urban flux measurements reveal a large pool of oxygenated volatile organic compound emissions, Proceedings of the National Academy of Sciences, doi:10.1073/pnas.1714715115.

Sanchez, D., Seco, R., Gu, D., Guenther, A., Mak, J., Lee, Y., Kim, D., Ahn, J., Blake, D., Herndon, S., Jeong, D., Sullivan, J. T., Mcgee, T., and Kim, S.: Contributions to OH reactivity from unexplored volatile organic compounds measured by PTR-ToF-MS – A case study in a suburban forest of the Seoul Metropolitan Area during KORUS-AQ 2016, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-174, in review, 2020.

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