

Interactive comment on “Development of a chemical ionization mass spectrometry system for measurement of atmospheric OH radical” by Wei Pu et al.

Anonymous Referee #1

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Overall evaluation

In this paper, a new Chemical Ionization Mass Spectrometer (CIMS) is described for the measurement of the important atmospheric radical, OH. The results described here are both novel and relevant to the atmospheric chemistry community. The authors describe how difficult this radical is to measure given its short atmospheric lifetime and thus, low concentration. However, due to its critical role in controlling the oxidizing capacity of the atmosphere, accurate quantification of this radical is needed in order to successfully evaluate our understanding of atmospheric chemistry. In situ field measurements of OH is relevant in its own right, but accurate assessments of OH these data are also

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very valuable in testing atmospheric trace gas models. In this work, the authors present a new CIMS system that is a welcome addition to the relatively few mass spectroscopy-based instrumentation that has been used in the past to measure OH. The present the instrument as an alternative to the more common LIF systems that have been shown to have some drawbacks in accuracy and interference effects in certain environments that the authors describe.

The paper is broken into two sections. The first is a rigorous evaluation of the performance, calibration, sensitivity and optimization of the new instrument in the lab and then second, the work describes a brief field deployment as a “proof of the instrumental concept” that has shown to provide data on the urban atmosphere in a Asian city. The authors report concentrations of OH typically found by other authors in similar environments and describe accuracy and uncertainties in these measurements that are again, typical of those found in the literature. Overall, I found the paper to had excellent technical description of the new CIMS instrument for the measurement of atmospheric OH concentrations and well worth of inclusion in AMT, pending a few small changes and or corrections/clarifications.

Specific Comments or suggested corrections are listed as follows.

Page 3, Line 11. The statement “third most important greenhouse gas” should include a reference.

Page 3, Line 18. The word “enormous” seems unnecessary hyperbole, as does the word “huge” on Page 3, line 21.

Page 8, Line 21. The authors describe sample air entering the system by way of a “blower”. I was confused as to the design of this blower. To me, a blower is a plumbing system that makes use of the fan which transfers the air in the emitted environment at high flow or low pressure. I assume the authors are referring to such a system and that is flow rate is regulated by the MFC they mention. I think a clarification of this inlet system would be warranted to describe the initial portion of the inlet more clearly.

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Page 9, Line 5. The authors describe the distance of the injectors from “the tube”. Is this the front of the inlet scoop with the blower or the front of the ionization section of the instrument?

Page 18, Line 7. The sentence that “Coronal discharges are known to produce OH ions should include references.

Page 19, Line 15. The authors describe the suppression of the instrument sensitivity by the addition of C₃F₆. Do the authors have any ideas as to why this was so? For the clarity of the paper and for attempts to replicate the results it would be helpful to expand this section with some suggested explanations of the effect of C₃F₆. Running a simple model of the ion chemistry could shed light on why there is such a suppression of the ion detection system.

Page 19, Section beginning at line 17. Although the addition of NO₂ to the system that the authors describe did effect both OH and HO₂ the absence of a higher concentration standard that those available to the authors make this section somewhat redundant. I feel removal of this discussion would not detract from the paper at all.

Page 25, line 12. The authors describe the high background signal due to large concentrations of ambient H₂SO₄. Do they have any feel for what these ambient concentrations were (even ballpark)? Several studies suggest concentrations at sites typical to those the authors measured (e.g. Guo et al., 2012, Zheng et al. 2018) that could be described or at least referenced.

Page 26. The authors describe their future plans to reduce the interference of ambient H₂SO₄ by using isotopically labeled SO₂. This technique was first used by Eisele and Tanner (1991) and this line should thus reference this important earlier paper.

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