

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

Anonymous Referee #2

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This paper describes the development of a chemical ionization mass spectrometer for the measurement of OH. The authors describe tests using ^{210}Po , and a corona discharge as ionization sources. They also describe tests using propane, C_3F_6 , and NO_2 as different OH scavengers. The authors conclude that using the radioactive ^{210}Po and propane are the best ionization source and OH scavenger respectively. The authors also present a calibration system based upon water photolysis and N_2O actinometry.

Overall, I find that there is little novel information presented and the work is not suitable for publication here. The CIMS technique described has been in use since the

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early 1990's and the description is a rehash of the works published by Eisele, Tanner, Mauldin, Berresheim, Sjostedt, and others. The calibration is merely an adaptation of that used by Kürtén et al. (2012). The stated limit of detection and uncertainty is approximately the same as reported in previous works.

I find that the present work also lacks detail when compared to the previous works. For example, there are no concentrations of reagent gases given, only flows. Many of the tests performed (varying lens voltages, SO₂ and scavenger flows etc.) are specific to the geometry of a particular CIMS system and are typical tests to characterize the instrument and establish operational parameters. The tests of the different ionization sources and OH scavengers are interesting but have little significance when compared to the rest of this work.

On the other hand, a manuscript with some brief descriptions of the measurement techniques and in-depth data analysis of the field measurements would be much stronger and would be of great interest, but will be more suitable for a more general journal (not an instrumentation journal).

Specific questions/comments:

There are many grammatical errors in the manuscript that make it not suitable for publication.

The words titrate and titration are used throughout the manuscript. Titrate and titration are chemistry terms that indicate the smallest amount of reagent necessary to reach an endpoint. The term convert and conversion are more accurate.

No mention is made of the distribution of nitrate ions and clusters. Is this distribution measured? It is shown in Figure 1 that HNO₃ is added to the rear injector flow (as in Sjostedt et al. and references therein) in addition to that added to the sheath gas to maintain the HNO₃ cluster distribution in the instrument. There is no mention of this aspect in the text. Does the cluster distribution change over time or even between

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operating modes (OH signal and background)? Does that affect the sensitivity?

It is stated that the OH background measurement is made to account for pre-existing ambient H₂SO₄. Is this the only source of the background signal? A publication in Nature by Mauldin et al. states that a significant fraction of this OH background signal caused by a non-OH oxidant of SO₂ in environments with large biogenic emissions. Could such emissions affect the sensitivity?

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