

Interactive
Comment

Interactive comment on “Peroxynitric acid (HO₂NO₂) measurements during the UBWOS 2013 and 2014 studies using iodide ion chemical ionization mass spectrometry” by P. R. Veres et al.

Anonymous Referee #1

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Veres et al. described a new analytical technique to quantify peroxynitric acid (HO₂NO₂) and hydroperoxyl radical (HO₂) using a chemical ionization mass spectrometer with iodine negative ion chemistry. The authors presented laboratory experimental results and field observational datasets from winter field campaigns. This paper has several scientific merits such as 1) Introducing a new analytical technique for the rarely observed reactive species and 2) highlighting potential importance of HO₂NO₂ in boundary layer photochemistry in the mid latitude region during the winter time that has been considered only important in the free troposphere or the polar regions. As a number of researchers is using the iodine negative ion chemistry application, the pub-

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Interactive Discussion

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lication of the presented research outcomes will be highly beneficial to the research community. In this sense, some additional information on the analytical techniques are urged. The specifics are;

1) A diagram of the CIMS system would be helpful for readers to follow. In the text, it is not clear whether the sample was humidified by water vapor addition.

2) The standard generation methods for HO₂NO₂, HO₂, and HONO may be better to be situated in the method section rather than in the results and discussion section. It is understandable that the authors try to balance between laboratory and field studies so that the manuscript is not overwhelmingly extended. However, more specific descriptions on standard generation methods in the manuscript rather than referring previous studies would be better for reader to grasp the described analytical techniques.

3) Is HO₂NO₂ is the only source for nitrite to the snowpack?

4) Line 22. If there is a systematic sensitivity difference between ambient HO₂ and HO₂ from HO₂NO₂. Wouldn't you be able to determine it from laboratory calibration datasets? Further empirical and theoretical explanations are required rather than just a simple speculation.

Specifics

Figure 4b. It seems that the discussion is based on the minuscule concentration differences. Further statistical justification on whether the reported differences are meaningful. There is no figure caption for Figure 4b.

The authors use both “Figure” and “Fig” for the figure references in the manuscript. Please be consistent.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 3629, 2015.

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