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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Peroxynitric acid (HO_2NO_2) measurements during the UBWOS 2013 and 2014 studies using iodide ion chemical ionization mass spectrometry" *by* P. R. Veres et al.

Anonymous Referee #2

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Veres et al present details of the use of I(H2O)- for chemical ionization mass spectrometry measurements of HO2NO2 in the ambient atmosphere. Two inlet set-ups, "cold" and "hot", are discussed, as well as the detection of several product ions, including IHO2-, NO3-, and IHO2NO2-. Results of ambient measurements and modeling for HO2NO2 during the 2013 and 2014 Uintah Basin Wintertime Ozone Study is presented. Most of the manuscript is dedicated to the description and calibration of the technique (∼12 pages) with only ∼5 pages discussing the science of the ambient measurements. Since the discussion of the ambient results is minimal in comparison, it might be more appropriate for this manuscript to be published in Atmos. Meas. Tech-

nol. Regardless, the manuscript is well-written and is an important contribution. I agree with the first reviewer that section 3.1 should be part of the methods, rather than results and discussion, section. In addition, section 3.2 is labeled "UBWOS observations", but the first three paragraphs discuss the method rather than the science. Perhaps this should be labeled differently from the discussion of the ambient data in the subsequent paragraphs. Characterization of the method is stated as "laboratory results", which can be confusing. In terms of the science, the authors should address the near-surface production of HO2NO2 from snowpack photochemistry (e.g. NOx emission – a well-known phenomenon). Additional suggestions are noted below.

Abstract: The first half of the abstract is very technical. I suggest removing sensitivities and added an explanation of the importance of HO2NO2 at the beginning of the abstract.

Section 2.2: Were snow samples collected during the UBWOS 2014 study?

Section 3.2, 4th paragraph: Discuss the concentrations of HO2NO2 so that the reader isn't required to look at the figure.

Figure 5 is a great contribution to the paper. It would be good to expand the discussion and implications of this figure, including where HO2NO2 chemistry will matter and what the impact of oil activities in the region have on the chemistry in terms of this figure.

Page 3647, lines 11-13: Move to previous paragraph or integrate paragraphs.

Page 3647, lines 16-21: These sentences describe the figure but not the observed result, as would be helpful.

Page 3647, lines 21-23: Why is the emission of precursors from snow photochemistry not discussed?

Section 3.2: It would be useful to add discussion of the differences between the magnitude of the 2013 and 2014 results in Figure 4. Alternatively, just 2014 could be shown to illustrate the vertical profile conclusions. In general, more discussion of the results would be useful.

Page 3648, lines 1-9: This paragraph would be more well-suited in the methods section.

Page 3650, line 1: Zhang et al refers to an experiment with sulfuric acid solution, which is quite different from the snow surface.

Page 3650 discussion: Couldn't increasing nitrite levels increase NOx production from snow, potentially resulting in more HO2NO2 production?

Page 3651: The snow as a sink of HO2NO2 is discussed; however, the surface snowpack as a source should also be discussed.

Figure 4: Is the shaded area campaign variability or uncertainty? This impacts interpretation of the results.

Technical Corrections: Page 3638, line 10: Fix typo

Page 3642, line 9-13: Remove repeated sentence.

Figures 1 & 7: Increase font size in figure.

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