

## ***Interactive comment on “Measurement of interferences associated with the detection of the hydroperoxy radical in the atmosphere using laser-induced fluorescence” by Michelle M. Lew et al.***

### **Anonymous Referee #2**

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This manuscript reports laboratory results of interferences from organic peroxy radicals (RO<sub>2</sub>) on HO<sub>2</sub> measurements done by the well accepted FAGE technique. The RO<sub>2</sub> interferences were studied for another two instruments by Fuchs et al. (2011) and Whalley et al. (2013). Still, the characterization of interference is fundamental for each instrument using chemical conversion, because the relative interference from RO<sub>2</sub> towards the HO<sub>2</sub> signal will be quite dependent on the individual set up, with NO concentration, reaction time and efficiency of mixing of NO into the flow. The manuscript is well structured and the points are clear. The results are of interest to the community. Therefore,

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the referee support the publication in AMT.

Minor comments:

1) The discussion of RO<sub>2</sub> interference are mainly associated with the MCMA-2006 campaign. However, the characterization was done with 1 sccm NO addition, which is lower than the flow rates used in the MCMA-2006 campaign. The authors stated that the conversion efficiencies shown in table 2 should be regarded as a lower limit. Could it be possible to quantify how large difference could be made if larger NO flow is used. Using the actual conversion efficiencies will help to discuss the implication of RO<sub>2</sub> interferences for HO<sub>2</sub> measurements during the MCMA-2006 campaign.

2) The subtraction of HO<sub>2</sub> interferences requires the knowledge of speciated RO<sub>2</sub> concentrations. Modelled RO<sub>2</sub> concentrations could be used as in the present paper, but this would be a dangerous exercise given the likely uncertainties in the model. Could the authors provide the error analysis in the modelled RO<sub>2</sub>. In fact, RO<sub>2</sub> measurements was achieved using LIF technique in a recent field campaign in China, which was higher than model predicted for high NO<sub>x</sub> conditions but in good agreement in moderate and low NO<sub>x</sub> regime (Tan et al. 2017 ACP). More discussion should be added if one need to correct the HO<sub>2</sub> interferences.

3) One suggestion for further field application and maybe also helpful to the readers. The authors could add a paragraph to describe how to minimize or quantify interference for further field campaigns.

Technical comments:

Page 9, line 15: ‘Fig. 2’ should be ‘Table 2’

Page 9, line 24: after the lower NO concentration adding ‘(table 1, add the residence time for different cell conditions)’

Page 10, line 15: ‘could contribute to the higher RO<sub>2</sub>-to-HO<sub>2</sub> conversion efficiency reported here for MVK’ is confusing, suggest to quantify such effect with specific num-

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bers.

Page 10, from line 19 to line 21: the sentence is too long and hard to understand, suggest rephrase it.

Page 10, from line 21 to line 23: It states that the alkoxy radicals isomerize and decompose. Could the author provide reference for it?

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