Response to Anonymous Referee #2

We would like to thank the reviewers for their efforts in reviewing this manuscript, and we feel that the manuscript is much stronger with the suggested changes. Below are detailed responses to their comments, which are highlighted in italics.

This manuscript reports laboratory results of interferences from organic peroxy radicals (RO2) on HO2 measurements done by the well accepted FAGE technique. The RO2 interference 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 RO2 towards the HO2 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, the referee support the publication in AMT.

Minor comments:

1) The discussion of RO2 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 RO2 interferences for HO2 measurements during the MCMA-2006 campaign.

We have clarified that the NO flow of 1 sccm used in these experiments is the same NO flow reportedly used during the MCMA 2006 campaign. However, the measured HO_2-to-OH conversion efficiency at this NO flow in these experiments was found to be approximately 20% lower than the HO_2-to-OH conversion efficiency measured during the MCMA 2006 campaign, and the reason for this discrepancy is unclear. As discussed in the manuscript, potential explanations include the possibility that the NO flow during MCMA-2006 was actually greater than the 1 sccm that was measured, or it may indicate problems in accurately recreating the flow conditions during this campaign in these laboratory experiments. Since it is not known whether the flow was greater than the 1 sccm that was measured, or whether the flow conditions during the campaign led to more efficient mixing, we chose to conduct the experiments using the measured 1 sccm flow rate, with the caveat that the conversion efficiencies may represent a lower limit to the actual conversion efficiencies during MCMA-2006. Given that the conversion efficiencies for the other instrumental configurations do not appear to directly correlate with the measured HO_2-to-OH conversion efficiency, it is difficult to quantify how the higher HO_2-to-OH conversion efficiency measured during MCMA-2006 would translate into the various RO_2-to-HO_2 conversion efficiencies, although it is likely that many of them would be larger. We have attempted to clarify this in section 3 of the revised manuscript.

2) The subtraction of HO2 interferences requires the knowledge of speciated RO2 concentrations. Modelled RO2 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 RO2. In fact, RO2 measurements was achieved using LIF technique in a recent field campaign in China, which was higher than model predicted for high NOx conditions but in god agreement in

moderate and low NOx regime (Tan et al. 2017 ACP). More discussion should be added if one need to correct the HO2 interferences.

As pointed out by the reviewer, it is possible to correct the measured HO_2* through subtraction of the modeled speciated RO_2 interferences, and compare these results to the modeled HO_2 concentrations. However, as noted by the reviewer, this method has a much greater uncertainty as a result of the uncertainty associated with the modeled RO_2 measurements. We estimate that the uncertainty associated with the modeled RO_2 to be approximately ±70% (2σ), similar to that for the modeled HO_2 (Dusanter et al., 2009b). As a result, we prefer to compare the modeled HO_2* to the measured HO_2*. This has been clarified in the revised manuscript.

As suggested, we have also included a discussion of the results from Tan et al. (2017) regarding the model underestimation of their morning RO_2 measurements, which appear to be consistent with the morning observations during MCMA-2006.

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.

We have expanded the discussion of minimizing the RO_2 interference in section 5 as suggested, including more quantitative information on the concentration of NO that we have shown to minimize the interference from isoprene-based peroxy radicals.

Technical comments:

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

This typo has been corrected, as the text is actually referring to the experiments shown in Fig. 3.

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

We have added the reference to Table 1 as suggested. The reaction time for the different configurations is approximately 1-2 ms based on simulations of the kinetics of the system. Unfortunately, the precise residence time for the different flow conditions is difficult to simulate given the different OH radical wall losses that may be occurring.

Page 10, line 15: 'could contribute to the higher RO2-to-HO2 conversion efficiency reported here for MVK' is confusing, suggest to quantify such effect with specific numbers.

We have expanded and clarified the discussion of this potential interference as suggested. However, the actual interference is difficult to quantify as addition of water vapor may reduce the HO_x radical production from photolysis of these VOCs through quenching of the excited VOC.

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

We have shortened and rephrased this sentence as suggested.

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

We have provided references as suggested (Atkinson, R., Int. J. Chem. Kinet., 29, 99-111, 1997; Finlayson-Pitts, B. J. and Pitts Jr., J. N.: Chemistry of the Upper and Lower Atmosphere, Academic Press, San Diego, 2000).