



## Comment on acp-2021-702

Anonymous Referee #1

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Referee comment on "Ground-based Investigation of HO<sub>x</sub> and Ozone Chemistry in Biomass Burning Plumes in Rural Idaho" by Andrew J. Lindsay et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-702-RC1>, 2021

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This manuscript presents measurements of XO<sub>2</sub> (=RO<sub>2</sub> + HO<sub>2</sub>) at a ground site influenced by biomass burning plumes. The measured XO<sub>2</sub> is used to evaluate box modeling with different mechanisms. It is shown that model results are consistently greater than measured [XO<sub>2</sub>] by ~30%. The influence of biomass burning on ozone formation is also studied. The ozone formation is only slightly impacted by smoke because of low NO<sub>x</sub>, indicating the O<sub>3</sub> chemistry is NO<sub>x</sub>-limited. Overall, the measurement is valuable and I recommend publication with major revisions noted.

### Major Comments

- In the manuscript, the discrepancies between measured and modeled [XO<sub>2</sub>] are noted, but not fully explained. Namely, what causes the over-estimate of XO<sub>2</sub> in models? Any recommendations to the chemical mechanisms? Or the difference is within the measurement uncertainty, which undermines the valuableness of the XO<sub>2</sub> measurement? Looking at Figure 6, model sometimes is 50-60% higher than measurement. Why? The difference shown in Figure 6 seems larger than the 31% noted in the abstract. In fact, how is the "31%" calculated?
- The effect of HO<sub>2</sub> heterogeneous uptake on the radical budget is likely over-stated. As the authors noted, the HO<sub>2</sub> uptake coefficient is highly uncertain. A relatively large value (i.e., 0.2) is chosen. In dense BB plumes, organic aerosol is the dominant composition with mass fraction up to 80%. As discussed in Abbatt et al. 2012 and George et al., the HO<sub>2</sub> uptake coefficient to solid organic particles is < 0.001 and to liquid organic particles is < 0.01. If applying the small gamma values, the HO<sub>2</sub> heterogeneous is negligible even in dense BB plumes.
- Missing HONO as model input is invoked in several places as a possible reason for the model vs measurement difference, but this reasoning is questionable. First of all, the  $\Delta_{\text{HONO}}/\Delta_{\text{CO}}$  after 3hr aging of BB plume, as shown in Peng et al., is 0.1 ppt/ppb, rather than 1 ppt/ppb as quoted in the manuscript (Line 377). In addition, Peng et al. clearly stated that after 3hr the [HONO] is near or below the instrument detection limit and they refrain from interpreting what it implies in terms of potential HONO steady state in aged plumes. Lastly, HONO can be added to the box model (using  $\Delta_{\text{HONO}}/\Delta_{\text{CO}} = 0.1$  ppt/ppb and measured CO), to directly test the effect of missing HONO on the modeling results.
- Figure 5 is interesting and related discussions should be expanded. For example, what

does the lack of correlation between P(O<sub>3</sub>) and NO when P(RO<sub>x</sub>) is small suggest. Does it suggest O<sub>3</sub> formation is VOC-limited? More information can be distilled from the figure, or from the four variables in the figure (PO<sub>3</sub>, PRO<sub>x</sub>, NO, and HCN).

- In the manuscript, some metrics are from direct measurement (e.g., [XO<sub>2</sub>]), some metrics are calculated from measured species (e.g., P(O<sub>x</sub>) from Eqn. (3)), and some metrics are purely based on box model. These need to be carefully worded in the discussion to avoid confusion. For example, Line 460 and others use the term "measured OH reactivity", which the reader believes is the calculated OH reactivity based on measured VOCs. Then, Line 477 refers to Eqn. (3) as measured P(RO<sub>x</sub>), which makes the reader confused for a bit, as it is calculated, not measured. Such subtleties hinder the readability of the manuscript.
- Line 482. Are there any constraints or independent verification on the contribution of photolysis of methyl glyoxal and glycoaldehyde to the P(RO<sub>x</sub>)? Their contributions seem much larger than expected, based on measurements of these two species in previous wildfire studies.

#### Minor Comments

- Line 26-28. The reader suggests to rephrase the sentence to "the model over-estimates the XO<sub>2</sub> by 30%".
- Line 29-30. Suggest rephrasing to "likely due to the presence of an unmeasured HO<sub>x</sub> source that is not included in models". The whole sentence may need to be rewritten after investigating the role of HONO as mentioned above.
- Figure 5b. P(O<sub>3</sub>) is used in the y-axis, but P(O<sub>x</sub>) is used in the figure caption. Be consistent.
- Line 387-397. The negative  $\Delta_{O_3}/\Delta_{CO}$  on 8/16 is likely a result of inaccurate O<sub>3</sub> background and mixing, rather than depletion. It is because as the authors noted, the [NO<sub>2</sub>] is much smaller than [O<sub>3</sub>], suggesting that [O<sub>x</sub>]  $\approx$  [O<sub>3</sub>], which rules out the possibility of depletion.
- Be consistent with which model results are presented. For example, why is MCM-BBVOC-het presented in figure 8, but MCM-BBVOC in figure 6?