Atmospheric Measurement Techniques Discussions

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Comment on amt-2021-432

Anonymous Referee #3

Referee comment on "Comparison of airborne measurements of NO, NO2, HONO, NOy and CO during FIREX-AQ" by Ilann bourgeois et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-432-RC3, 2022

Review report.

Bourgeois et al. presented comprehensive intercomparisons of airborne NO, NO2, HONO, NOy and CO in biomass burning plumes, each measured with differing techniques during FIREX-AQ in the summer of 2019. This study provides valuable dataset and the evaluation of accuracies of major techniques deployed in the challenging biomass burning plume conditions. Additional literature review on these species from major airborne field campaigns are helpful for understanding the accuracy of these measurements under different environmental conditions. The manuscript was written thoroughly, and the figures are made clear. Thus I recommend acceptance after revision. Below are my comments:

- Line 204, hourly calibration of NO LIF was performed with [NO] 4-20 ppbv, did this concentration range apply for all the smoke conditions? How do you ensure the linear response beyond this range?
- Lines 508-510, “Trajectories and ages that were grossly inconsistent with smoke transport patterns seen in geostationary satellite images were excluded from further analysis”. Whiich group should these data categorized into
- Lines 648-649, what is the p-value of Figure S4 b and d, any explanation for the seemingly dependence of the difference on NO2 concentration?
- Figure 3 and Figure 4, no letter label (e.g., a to e) was assigned to any of the panel.
- In section 3.3.1, intercomparison between CES and CIMS measured HONO were presented. I have the following questions: 1) The slopes shown in Figure 8 suggests CES HONO was higher than CIMS HONO. However, it seems neither the flight averages of the absolute difference shown in Figure S9, nor the histograms of the absolute

difference between the two methods suggest the CES-HONO > CIMS-HONO. Any explanation? 2) In Figure S9, why are there many missing points for intercepts (middle panel) and slopes (bottom panel), while the top panel (mean absolute difference) shows all the data on each sampling day? 3) it is interesting to see the measurement of HONO with CIMS are significantly affected by temperature, especially above 30°C, as is shown in Figure S10. Would the slope of CES-HONO vs CIMS-HONO be closer to 1 since it's not shown in this figure? 4) Could the inlets for the two methods be an issue that cause the discrepancy during FIREX-AQ?

- Do the measurements shown in Figure 10 (a) include both fresh smoke and aged smoke? If so, what if the fresh smoke and aged smoke were separately considered? Will the relative contribution of each NOy be significantly different? Are the large uncertainties associated with NO2, APNs and pNO3- driven by flight-to-flight difference, secondary processing, or environmental conditions (humidity and temperature)? What could be possible causes for the different contributions of major species (e.g. NO2, APNs and pNO3-) between western wildfires and eastern agriculture fires?
- In Figure S11(a), from the slopes determined for fresh versus aged smoke, can we say the sum of NOy outweigh CL-NOy for fresh smoke and the CL-NOy outweigh the sum of NOy, although the difference is within the combined instrumental uncertainties? If so what would the explanation be?
- Lines 732-734 described what different NOy measurements were used to calculate total NOy. While I understand the choices are based on precision, I wonder why CIMS HONO instead of CES HONO was chosen, as CIMS HONO underestimated CES HONO and its accuracy seems to be significantly affected by temperature variation as is discussed in 3.3.1?
- Lines 747-779 are difficult to follow. Figure 12(a) should be well explained first followed by Figure 12 (b). The current order is reversed, and I don't quite get the idea of Figure 12 (a). For Figure 12 (b), it is unclear how the missing NOy fractions (bottom panel) were calculated. My understanding is that fraction of each individual NOy to total NOy was calculated from the individual measurements and sum of NOy, then particle sampling fraction was calculated from the model. Combining the two pieces will enable the quantification of missing NOy (0-24%) resulting from the CL-technique, but how? Thus, further clarification will be needed. Also, in section 3.4.1, it is interesting to see the possible reasons that cause the negative and positive mode of the discrepancy between CL-NOy and sum of NOy. The authors separated the two modes and interpreted them separately. However, if one reason is important (e.g. pNO3- loss through the CL inlet), it should be important throughout the entire campaign, instead of certain period. I might miss something, but a clarification would be helpful.
- In section 3.5.1, it was noted the cause of the discrepancy between ICOS and TDLAS measured CO was unclear. I am curious whether temperature plays a role? Additionally, Figure 14(a) shows when CO goes above 10 ppmv, ICOS seems to outweigh TDLAS; as CO is higher the deviation from 1:1 line is larger. What are the possible explanations?