

Interactive comment on “In situ measurements and modeling of reactive trace gases in a small biomass burning plume” by M. Müller et al.

Anonymous Referee #1

Received and published: 4 December 2015

This paper presents measurements of CO₂, CO, NO, NO₂, HONO, NH₃, and NHO_gs in a biomass burning plume observed from aircraft. The observations are used to calculate emission ratios and emission factors in the fresh fire smoke and observations after aging to test a 0-D model of plume evolution. The results are extremely well presented in a very concise manner. This topic is currently of significant interest in the field of atmospheric monitoring and this paper represents a great addition to the literature available on understanding the evolution of fire smoke in the atmosphere. I highly recommend this manuscript for publication in ACP after consideration of the limited comments presented below.

Detailed Comments:

On the detection of HONO as NO₊, it seems to be there would be some issues relat-
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ing to the reaction of NO_+ , native in the ion source as background, reacting with the high concentrations of VOC in the fire plume that would alter the background of NO_+ in a non-quantifiable manner. To rephrase, NO_+ will react reducing the background on that mass while in the plume, and performing a standard instrument zero cannot reproduce that reduction therefore increasing the error in your background subtraction. This would in effect, if my reasoning were correct, make the measurement of HONO a lower limit. Is there any way to potential approximate this effect, can the authors comment on the relative amount of NO_+ in the ion source which is unavoidable? Or are the proportions such that one would not expect and NO_+ + VOC reactions to occur. Also, the authors correct for HONO production on instrument surfaces, is this done using a laboratory measured conversion efficiency of 1%, or is this figure an approximation. This is important to state in the text when discussing the correction.

The authors discuss the effect of large amounts of NO_2 titrating O_3 in the initial stages of the fire, but it seems the model does not pick this effect up (see figure 6). I assume this is why there is a steep drop in NO_2 in the measurements prior to 600s. Why then is there not a corresponding increase in NO at this point, rather a drop in the measurements? Then the overall trend in the measurements for both NO_2 and NO are increasing from 600s onward while the model shows a significant decrease in both mixing ratios. As the authors state in the text the model does a good job of capturing NO and NO_2 , this would seem to be a significant discrepancy, especially considering the log nature of the scaling.

Could the decrease in the methanol mixing ratio in the early stages of aging be a repartitioning of methanol to the aqueous phase of particles? There should be a fair amount of water vapor produced in the hot fire that would rapidly condense on existing particle phase.

The comparison of the HONO emission ratio to previously available data is a very nice addition to the discussion of these results. I would urge the authors to consider explicitly adding additional comparisons to previously published emission ratios or fac-

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tors to this manuscript. Another column on tables 2, 3 or S2 citing previous literature would be a very nice addition. Measurements of fuels from this region of Georgia have been performed before and would help to aid the connection of laboratory studies on biomass burning emissions with field observations such as these. I do not believe this is a necessary addition, but would be a welcomed addition to the work.

Specific comments:

Page 31508, line 3: The use of the word tentative HONO here give the impression that the concentration used in this publication could change, or are preliminary. I suggest a different word choice. The Authors give good grounds for why the concentrations are reasonable, especially considering the agreement with previously published values.

Page 31510, line 17: In reference to “compounds identified in previous studies” is a citation needed here?

Page 31513, line 14: A comma is needed in 10,472 ppbC

Page 31514, line 15: There is a figure order issue here as figure 9a is mentioned prior to figure 6 it seems.

Page 31514, line 25: I believe you need to delete the word “respectively”

Figure 1: This is visually nice, but I am not entirely sure it is necessary in the manuscript. While seeing a fire plume is neat, it scientifically does not add to the discussion or conclusions. However, I am quite content leaving the decision up to the authors and the editor.

Figure 2: Consider adding that the black arrows indicate the direction of flight to the caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 31501, 2015.

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