

## ***Interactive comment on “The Tropical Forest and fire emissions experiment: overview and airborne fire emission factor measurements” by R. J. Yokelson et al.***

### **Anonymous Referee #3**

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#### General Comments:

This paper describes an aircraft campaign that took place in Brazil in 2004. The purpose of the field observations was to characterize emissions of biomass burning from tropical forests in Brazil. The authors provide a detailed overview of the field campaign and measurements, quantify emission factors from many important trace gas species, including specific oxygenated volatile organic compounds (OVOCs), estimate total emissions from tropical forest deforestation based on the results from the study, and present unique observations of a large smoke plume encountered at the end of the study. The authors offer interesting details of the biomass burning that occurs in

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Brazil and provide a substantial introduction to the paper. A detailed comparison of the measurements from this study is made with other reported observations from the region.

The experimental methods applied during the field campaign and the analytical techniques applied to the data are well done and have been used successfully in past studies. The results presented provide new emission factors for many compounds of importance to the regional and global modeling communities, and are a great advancement to the existing data. This manuscript is appropriate for Atmospheric Chemistry & Physics, and I recommend that the paper be published after some changes have been made.

In general, I feel that this paper is much too long. Although extremely interesting, much of the information presented in the manuscript is not entirely necessary. For example, I believe that the Introduction could be condensed. Also, Section 3.3 that describes explicit details about Brazilian biomass burning practices could be summarized or left out entirely.

#### Specific Comments:

In the Introduction, the authors should point to Figure 1 when mentioning areas of interest (e.g., Page 6906, line 6 and page 6907, line 11). All of the important places mentioned throughout the paper should be included in this figure, for those unfamiliar with Brazil.

Page 6910: Why was air pumped into the canisters, and why weren't they initially at vacuum? Is anything lost when the air is sent through a pump?

Page 6911, lines 20-22: I may be missing something here, but, if the detection limits for the AFTIR were 15-20 ppbv for NO<sub>x</sub>, O<sub>3</sub>, and a few other compounds, how is it possible to estimate the enhancements of these compounds from these measurements? Aren't the background concentrations of these compounds lower than the detection limits?

Pages 6910-6911: How did the O<sub>3</sub> from the AFTIR compare to the measurements made from the UV absorbance instrument?

Page 6913, line 16: Why were only grab samples from the AFTIR made in the background air?

Page 6917, lines 6-7: How do the authors know that the mixed layer air was calculated to come from the northeast? Did they perform trajectory model analysis of this (or did someone else)? If so, please explain or provide the proper reference.

Page 6919, lines 2-10: The authors mention their PM<sub>10</sub> measurements, but do not include any comparison with other PM observations here (this is done later), as mentioned in lines 9-10.

Page 6919, lines 23-27: The inclusion of CO from MOPITT here is interesting. However, I am not clear what the authors mean to say here. Is it that the observations do not match the MOPITT retrievals because the vertical profile used in the MOPITT inversion is not correct? I suggest rewriting these sentences to be a bit more clear.

Section 3.3: Again, I think that this section is too long and contains too much detail that is not directly relevant to the rest of the paper. Also, I think it is worth mentioning here (when it is first brought up) that the surface measurements made at the ground level could have observed such high CO concentrations since it was measuring the emissions and plumes from RSC. (I don't think this is mentioned until later?)

Page 6922: the authors explain a lot of historical changes in biomass burning in the Amazon. Could this be another reason for the differences between the observations from the late 80's and those given here?

Page 6924-6925: What is the mean of the annual average of acreage burned? (They only present the two highest year numbers).

Page 6926: this is just a note: AM includes the Ward et al. and Ferrek et al. papers in its compilation of emission factors.

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Page 6930-6931: Although interesting here, there is a lot of text about cooking and other fires that may not be necessary for this paper.

Section 3.5.3: This is a very important section and I think could be made a bit more clear. First, the fact that much of the emissions comes from RSC, but do NOT get lofted is very important, especially to modelers. It is not clear where the authors got ~ 240 Tg of biomass burned. Is this from the Brazilian database? Is this a 10-year average? Is all of this burning assumed to be from deforestation? It is also not straightforward to arrive at the numbers 2148 Tg CO and 24 Tg PM10 from the text. This needs to be made much clear. (I see an average emission factor of 101.4 g/kg CO and a total tropical forest biomass burned 1330 Tg yr<sup>-1</sup>. Did I miss something?)

Section 3.6: This section is very interesting, especially since the measurements made during this plume intercept were so much higher than the other observations. I think that this entire section could be condensed some. Also, it is a little unclear in general. The authors talk about a plume that covers a huge expanse. It appears that the last flight of the campaign caught the beginning of this event. However, despite its size and duration, the authors talk about identifying the source of the “white ocean” to a few particular fire points detected by satellite? It sounds like the observations included a combination of aged smoke and relatively new plumes. Therefore, I am not entirely convinced that the smoke that was measured was relatively young (0-4 hours as stated on page 6935, line 6).

Page 6935, line 27: The authors state that the plume went a “Little further north than usual.” What’s usual?

Page 6936, line 15: I can’t remember where in the paper the speculation that large fires may be becoming more common in the Amazon. A reminder would be helpful.

Page 6937: The conclusion includes a lot of really interesting and worthwhile topics for further pursuit. Although I think that this is a valuable list, I feel as though a few of these topics weren’t addressed in the paper (e.g., 6).

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## Technical Suggestions:

Page 6909, lines 23-25 could be omitted.

Page 6914, line 5: another “)” is needed after deltaCO)

Page 6914, line 15: The parenthesis around the sentence starting with “EFs are combined” can be removed. The sentence can also be moved to line 11.

Page 6918, line 8: Could be changed to “Most of the lower CO values were observed above the mixed layer”

Page 6918, line 19: Remove “above”

Page 6920, lines 11-14: This sentence should be reworded. A suggestion is “A large uncertainty in the estimated area burned is due to uncertainties in remote sensing applications. For example, it is unclear if small fires or understory fires can be quantified from space, and many fires can be missed from space due to cloud cover, which is common over tropical forested regions.”

Page 6922, line 6: What is Caatinga? This could be put on the map in Figure 1.

Page 6922, line 23: Where is Mato Grosso? This could also be put on the map in Figure 1.

Page 6929, line 12: This is not a complete sentence.

Page 6930, line 14: The text in the parentheses could be removed.

Page 6930, line 17: Add a “:” after questions

Page 6933, line 4-5: This sentence doesn’t make much sense.

Page 6933, line 16: separate the sentence: “ that it was sampled. The EEF are ”

Page 6933, lines 27-29: This sentence doesn’t make much sense, and could be reworded.

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