

Interactive comment on “Emissions from forest fires near Mexico City” by R. Yokelson et al.

Anonymous Referee #1

Received and published: 21 June 2007

General Comments: This paper describes gaseous and particle measurements taken from aircraft that flew around the Mexico City region in March 2006. The observations focused on forest fire plumes, and the results were used to estimate emission factors of many compounds from fires in the pine forests of the region. The authors estimate that NO_x and HCN emission factors from fires in the region are much higher than those predicted by other studies. Additionally, the particulate matter (PM) emitted from fires in the MC area is substantial, and fires are estimated to be the major source of PM in the region for March 2006. The results presented in this manuscript are particularly useful for the prediction of fire emissions and their inclusion in models used to interpret the atmospheric chemistry of the region. The methods used to perform the measurements and data analyses used are referenced and have been applied successfully in past efforts. Many assumptions have been made to predict the potential impacts of the emissions on the region; however, the assumptions made by the investigators are

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well stated and discussed in the manuscript. This paper is relevant for Atmospheric Chemistry Physics, and I recommend that this paper be published after some changes to the manuscript have been made. My comments and suggestions are written here.

Specific Comments:

For the readers who are not familiar with the MILAGRO campaign, a brief description of the study (dates, locations, sponsors) should be given in the introduction. The web site or a reference should also be provided when the campaign is first mentioned. Additionally, section 2.1.6 should be moved to the beginning of section 2.1 (instead of last), since a description of the overall campaign, aircraft, dates, and locations should be given before the specifics of the measurements.

The authors at times use EFCO and EF HCN for the emission factor discussion. One convention should be used throughout. I suggest using a subscript (e.g., EFCO₁).

Section 2.1.3: Does the algorithm determined from the experiments (eqn. 1) change as the PM becomes aged? Would this make a difference in any of the results presented in this paper?

Section 2.1.7: the authors describe one prescribed fire for which they estimate the amount of fuel consumed. They mention that this was also done for other fires. I am assuming that these were also planned burns. Is this correct? And were the conditions for which these measurements made similar to the conditions during the typical burning time period? Section 3.1: Why didn't the authors also use GOES fire detections or other satellite products for this analysis?

Page 6697, line 5: I am surprised that the particles in biomass burning plumes are only 8% black carbon. Is this realistic?

Page 6699-6700: The authors compare the results to Andreae and Merlet (2001). It may be worthwhile to note that many modelers use the emission factors in this paper to calculate emission estimates.

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Page 6700: The investigators discuss HCN. Is there any atmospheric relevance of HCN, other than a potential marker for biomass burning? What about potential mobile sources of HCN?

Section 3.4: The discussion and inclusion of the particle chemistry is interesting, but is it used in the analysis? Can any quantitative information be used to determine the fraction of particular compounds or particle components be used to speciate the PM emission factors?

Section 3.5: This analysis is a stretch, but I think a useful exercise. And the authors do state their assumptions made. It may be interesting to compare the estimates from this method to other estimates (say, the GFEDv2) for a similar time period. The comparison to the anthropogenic emissions is very interesting, and shows the potential importance of fire emissions in the region. How does this simple analysis compare to other emission estimates?

Page 6703, line 19: the authors report a measured NO_x/VOC ratio from the fires. However, since they are not measuring many of the oxygenated and larger compounds, are they overestimating this ratio?

Page 6705, line 23: if the authors include a comment about O₃ production in the downwind plume, and how this may be impacted by dust, the authors need to include more information about this. Dust has not been mentioned yet in this manuscript. I suggest adding more detail about this process, or removing this comment altogether, since it's not relevant to the discussion about fire emissions.

Figure 1: This figure is difficult to read. An overlay of the political boundaries and the location of the vegetated regions would be useful. In the lower panel, the text and the scale bar can not be read. There should also be a legend (I am assuming that the purple denotes urban areas? And there is some information in that figure about the topography?

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Figure 4: Units need to be included on the Y axis, and 3 decimal places is not necessary. Also, it would be really useful to include an error bar on the emission factors.

Technical Comments:

I suggest that the authors go through the paper again for editorial corrections. Specifically, there are a lot of long sentences that could be broken up, or to which commas could be added. This would make the manuscript much easier to read.

The authors need to define all of the acronyms that are used when they are first used.

Pg. 6689, last sentence: The authors refer to separate papers. Could these be cited?

Pg. 6690, Line 18: Change to “Sampled ram air into two-liter stainless steel canisters on board the Twin Otter. These canisters were shipped E”

Page 6691, line 11: Define WAS

Pg. 6692, line 1: Is there a reference for the inlet?

Pg. 6692, line 15: Is the UHSAS a commercial instrument? If so, please include the manufacturer. If not, is there a reference?

Pg. 6692, line 21: Define CIMS

Page 6692, line 24: change to “dependent”

Page 6693, line 4: Start a new paragraph with the description of the CO measurement, and reword.

Section 2.2: the authors used both NMOC and NMHC. I think they should just use one of the other (and NMHC should be defined).

Page 6695, line 13. This first sentence is a bit confusing. Might it be better to use: “The ratio of ER to CO₂ for the NMHCs observed in the U-Miami cans is derived by multiplying the ration of can EF to CO with the AFTIR EFCO to CO₂.” (I am not sure if this is correct, but this is the way I currently read the sentence). Writing out the general

equations for this method would be most useful.

Page 6698, line 21: change “are” to “were”

Page 6703, line 13: This sentence is a bit confusing, and I am not exactly clear on how these numbers are determined.

Page 6704, line 12: The sentence starting with “the unusually high HCN emissions \dot{E} ” can be removed.

Page 6704, line 25: start a new paragraph here.

Page 6704, line 28: The authors state that fires are the main source of PM. Should this be PM_{2.5}? If measuring coarse PM, would this still be the case?

Page 6705, line 1: Change to “(e.g., cooking, garbage burning)”

Page 6705: include the proper reference for HYSPLIT

Page 6706, line 3: change to “ \dot{E} impacts may be determined with other MILAGRO airborne data.”

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 6687, 2007.

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