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Atmospheric Chemistry and Physics Discussions

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

R. Yokelson et al.

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Response to comments by Reviewer #1 on "Emissions from forest fires near Mexico City" On behalf of all authors by R. Yokelson.

Comments by Anonymous Referee #1 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 NOx 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 use to perform the measurements and data analyses used

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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 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: R1.1. 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. Au1.1. We added this description (and a reference to the Fast et al. overview paper) to the revised paper.

R1.2. 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. Au1.2. We tried that, but then the description of the sampling makes less sense when the nature of the instruments is not understood so we moved this section back to the original location.

R1.3. 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., EFCO). Au1.3. We use the “EFCO” format throughout the revised paper, as we have in past papers. This is consistent with the most common literature convention.

R1.4. 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? Au1.4. The empirical relationship might change a little with aging - we have not tested that. In the revised paper we clarify that we are taking a relationship measured for fresh smoke in non-humid conditions and applying it to fresh smoke in non-humid conditions.

R1.5. Section 2.1.7: the authors describe one prescribed fire for which they estimate

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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? Au1.5. There was one major planned fire event. The other fires sampled by the aircraft were “fires of opportunity.” Some of these fires of opportunity were later located by the ground-based crew and they measured the fuel consumption. The ground-based crew also measured fuel consumption on a few area fires that we did not sample. This is clarified in the revised section on the ground-based fire measurements. (See response to reviewer #2 for text of new section.)

R1.6. And were the conditions for which these measurements made similar to the conditions during the typical burning time period? Au1.6. Yes. The airborne sampling time for each fire is shown in Table 1 and it was always within, or just a few minutes after the 12-5 “burning period.” We note in the revised paper that the planned fire was ignited at about noon and lasted for ˜1 hour.

R1.7. Section 3.1: Why didn’t the authors also use GOES fire detections or other satellite products for this analysis? Au1.7. We did examine GOES fire detection data. The GOES fire detection data (from the geospatial archive at the National Geophysical Data Center, NESDIS (National Environmental Satellite, Data, and Information Service) contained 110 fire detects in the study region during March 2006. The general spatial distribution of the GOES fire detects are qualitatively similar to the spatial distribution of the MODIS hotspots. The lower number of GOES fire detects compared to MODIS is not surprising given the much lower resolution of the GOES imager (4 km2 versus 1 km2 for MODIS) and the low intensity of the fires in the study region. We only use MODIS in this paper, because it has the best resolution.

R.1.8. Page 6697, line 5: I am surprised that the particles in biomass burning plumes are only 8% black carbon. Is this realistic? Au1.8. Yes, this value is unusually consistent between studies of biomass burning. One more reference to a review article on this topic (Reid et al., 2005) was added to the existing reference.

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R1.9. 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. Au1.9. This is done in the revised paper.

R1.10. 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? Au1.10. Biomass burning is considered by most to be only significant source of HCN. The reference indicating this was moved up to this point in the revised paper.

R1.11. 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? Au1.11. We do not use the single-particle chemistry results in the estimates of the PM contribution, but this is a multi-topic paper and the single particle chemistry is consistent with the high N emissions. This paper seems like the best place to put this interesting, relevant info that may be of use to other investigators for a variety of purposes.

R1.12. 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? Au1.12. We don't know of other estimates for a study area with the same dimensions, but we note that our two independent estimates made in this paper turned out to agree with each other quite well.

R1.13. Page 6703, line 19: the authors report a measured NOx/VOC ratio from the fires. However, since they are not measuring many of the oxygenated and larger compounds, are they overestimating this ratio? Au1.13. We are. The revised paper states

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that this ratio is an upper limit throughout the text.

R1.14. 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. Au1.14. We did not want to substantially broaden the scope of the paper at this point, but it does seem important to remind the reader that dust is also an important issue when thinking about the outflow. We added a reference (Moffet et al, 2007) to the revised paper that gives more details on dust impacts during MILAGRO.

R1.15. 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? Au1.15. The revised paper contains a much-improved Figure 1 which includes contours, vegetation, hotspots, fires sampled, urban areas, political boundaries, and a legend.

R1.16. 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. Au1.17. The revised figure 4 includes error bars and the appropriate number of decimal places.

Technical Comments: R1.18. 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? Au1.18. Done in revised text. Re page 6689, the separate papers are only

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planned at this point so we changed “separate” to “future” in the revised text.

R1.19. Pg. 6690, Line 18: Change to “sampled ram air into two-liter stainless steel canisters on board the Twin Otter. These canisters were shippedĚ” Au1.19. We left this sentence unchanged since two different canister sizes were filled with the same inlet as is already clarified subsequently.

R1.20. Page 6691, line 11: Define WAS Au1.20. WAS was already defined in the section header on page 6690.

R1.21. Pg. 6692, line 1: Is there a reference for the inlet? Au1.21. (Jonsson et al., 1995) was added to the revised paper.

R1.22. Pg. 6692, line 15: Is the UHSAS a commercial instrument? If so, please include the manufacturer. If not, is there a reference? Au1.22. We added the manufacturer (Droplet Measurement Technologies)

R1.23. Pg. 6692, line 21: Define CIMS Au 1.23. Chemical Ionization Mass Spectrometer was added to revised text.

R1.24. Page 6692, line 24: change to “dependent” Au1.24. Done in revision.

R1.25. Page 6693, line 4: Start a new paragraph with the description of the CO measurement, and reword. Au1.25. OK.

R1.26. 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). Au1.26. NMOC and NMHC are different in important ways since NMOC includes oxygenated and nitrogen containing organics, but NMHC strictly includes only hydrocarbons. We define NMHC in the revised paper and the need for two terms should now be clear.

R1.27. 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

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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. Au1.27. Its important not to equate the terms “ER” and “EF.” ER refers to a molar ratio unless a mass ratio is specified. EF always refers to a mass per unit fuel consumption. We clarify this in the revised paper.

R1.28. Page 6698, line 21: change “are” to “were” Au1.28. OK.

R1.29. Page 6703, line 13: This sentence is a bit confusing, and I am not exactly clear on how these numbers are determined. Au1.29. The EF gives the mass of emissions per unit amount of fuel burned. Multiplying the EF by the total amount of fuel burned in the region then gives the total emissions. We describe this more explicitly in the revised paper.

R1.30. Page 6704, line 12: The sentence starting with “the unusually high HCN emissions” can be removed. Au1.30. The point is that using lower initial HCN/CO emissions from the literature would inflate the estimate of fire PM. We therefore retain this in the revised paper.

R1.31. Page 6704, line 25: start a new paragraph here. Au1.31. OK

R1.32. Page 6704, line 28: The authors state that fires are the main source of PM. Should this be PM2.5? If measuring coarse PM, would this still be the case? Au1.32. The revised paper states that fires are the main source of “primary fine particles” here and elsewhere as needed. This is partly also in response to the other comments.

R1.33. Page 6705, line 1: Change to “(e.g., cooking, garbage burning)” Au1.33. OK.

R1.34. Page 6705: include the proper reference for HYSPLIT Au1.34. We are using the reference requested on the HYSPILT website.

R1.35. Page 6706, line 3: change to “impacts may be determined with other MILAGRO airborne data.” Au1.35. OK

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