Atmos. Chem. Phys. Discuss., 7, S2681–S2684, 2007
www.atmos-chem-phys-discuss.net/7/S2681/2007/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

Received and published: 26 June 2007

This manuscript describes the results of airborne measurements of forest fire emissions near Mexico City. The most significant finding, to me at least, is that NOx and HCN emissions were relatively high in the forest fire plumes, possibly as a result of nitrate deposition to the vegetation surrounding the city. The authors use the emission factors for HCN and PM1 to estimate the contribution of biomass burning to the outflow of aerosol from the Mexico City Metropolitan Area (MCMA) and this is where the manuscript runs into some very serious problems as outlined below. I suggest a very major revision before the manuscript is acceptable for publication in ACP.

A. The authors derive the biomass burning contribution to the aerosol in the MCMA by attributing the difference in measured PM1/CO ratios downwind from MCMA and the PM10/CO ratio in the emission inventory entirely to regional biomass burning. This assumes that (1) the PM10 emission inventory is accurate, and (2) that there are no other sources of aerosol apart from direct emissions. The first assumption is not tested

in the current manuscript, and the second assumption is clearly not true. Secondary formation of nitrate and sulfate are significant in the MCMA plume and also, several authors have shown that secondary organic aerosol in urban plumes can exceed primary sources by an order of magnitude (summarized in Volkamer, GRL 2006). In addition, mineral dust was shown to be an important aerosol component during Milagro (for example from the airborne Lidars onboard the NASA King Air and the DC-8. Mineral dust contributes mostly in the super-micron size range of course, but also below 1 micron (Moffet, ACPD 2007).

B. A wealth of data was presented at the Milagro workshop in Mexico City from May 16-19, which indicated that the BB contribution to the MCMA plume estimated in this manuscript is at the high end:

i. Measurements of levoglucosan (T0 and T1), acetonitrile (T0, T1, the Aerodyne mobile lab and the C-130), and a BB component derived from the AMS spectra (T0, Aerodyne mobile lab and C-130) all indicated lower BB contributions in the MCMA outflow. In addition, the ground-based measurements of these BB tracers were the highest in the early morning when local emissions built up in a shallow mixing layer under low wind conditions, indicating a significant local contribution to the BB source, i.e. no the forest fires discussed in this manuscript.

ii. Measurements of potassium in single particle mass spectra at T0 indicated a high percentage of particles attributed to biomass burning (Moffet, ACPD 2007). However, the diurnal variation of the BB particles was quite different from the other BB indicators (acetonitrile, levoglucosan, BB factor from the AMS).

iii. Carbon 14 data indicated a relatively high fraction of modern carbon, which can of course also be attributed to secondary formation from biogenic VOCs (although this is a less likely explanation in the MCMA).

In summary, different measurement results disagreed on the BB contribution to the MCMA outflow. Few of these results have been published, thus far, so in principle the

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authors of this manuscript are not strictly required to take them into account. Nevertheless, it is important for them to understand that there are conflicting pieces of evidence, which they may want to consider in publishing these results.

It seems to me that the authors could choose between 2 possibilities: they could significantly shorten the manuscript by focusing on the determination of emission factors (and give more details on those). Or they could strengthen their arguments about the BB contribution to the outflow from the MCMA by taking into account additional evidence that should be readily available to them: the AMS data for BB organic aerosol (Jimenez) and the PTR-MS and TOGA data for acetonitrile (Karl, Apel) from the C-130, as well as the findings from the other platforms and ground sites.

Further comments:

1. The authors use MODIS hot spots to estimate BB emissions in the vicinity of the MCMA. I wonder if this can lead to overestimates of BB emissions. Grass fires, for example, can be observed by MODIS, but the burnt biomass is very different from forest fires. This may be a problem in the MCMA where open fires to burn agricultural waste and weeds along roads are quite common. The authors could strengthen their case considerably by overlaying the MODIS hot spots with land use and vegetation type around the MCMA to estimate how many of the hot spots were actually forest fires. Wiedinmyer at NCAR has been working on this problem for North America and may have some relevant data for Milagro.

2. Another problem arises upon examination of Figure 1B: one of the hot spots seems to be on the top of Popocatepetl. The summit of this volcano is at 5400 meters and is well above tree line. I would guess that the hot spot detected by MODIS is the crater of Popocatepetl in this case. I am not an expert in this area, but a quick literature search shows that thermal volcanic features are also detected using MODIS.

3. Many of the other hot spots to the north of Popocatepetl also seem to be high up on the volcanoes. Are these hot spots above or below tree line? The higher up the

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volcanoes the forest fires are, the less likely it is that the emissions are mixed down to the surface. Also, are there any volcanic/geothermal features that could be observed by MODIS?

4. The first author of this paper has a long and outstanding track record in the determination of BB emission factors in the laboratory. It seems obvious to add a section to this manuscript to determine the HCN and NOx emission factors for vegetation in the MCMA, which could make the conclusions about the influence of pollution on HCN and NOx emission factors a lot stronger.

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