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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Size-resolved aerosol emission factors and new particle formation/growth activity occurring in Mexico City during the MILAGRO 2006 Campaign" *by* A. J. Kalafut-Pettibone et al.

Anonymous Referee #1

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This paper describes concentration measurements of particle number and volume and CO2 at a site in Mexico City during MILAGRO 2006. The measurements are used to estimate primary emissions factors of particle number and volume concentrations as a function of carbon mass concentration. There are many uncertainties associated with this approach, but I think that the authors have been quite thorough and discussed most of them. The extension of their approach to the measurements from the NASA DC-8 sampling the Mexico City urban plume is interesting. I have a concern about the treatment of the APS measurements and a few minor comments.

1) Page 6662, lines 1-20 – It has been observed before that the number distribution obtained from the APS drops at smaller sizes in a manner inconsistent with other measurements. In particular, this reviewer has found that the number concentrations observed with the APS in the lowest three channels are lower than when compared with numbers from a SMPS and optical methods; the discrepancy associated with the first channel being the greatest and that associated with the third channel being the least. Unfortunately, I am unaware of a paper that deals specifically with this issue, but I believe there are others who have observed a similar result. An example of the ability of the APS to compare with a SMPS and a FSSP300 (an optical probe) when the first three channels of the APS are removed can be found in Figure 2a of Leaitch et al. (ACP, 2010). The point of this comment is that by applying a factor of 2.86 to all sizes of the APS, which is what I understand you to have done, your volume estimates may be biased significantly high for particles >500 nm.

2) Page 6671 and Figure 8 – Your technique has validity when the correlation of CO2 and particle number is very high. But when the correlation is lower, other factors besides emissions are strongly influencing your results. Since that correlation is near zero for your daytime points (Fig. 4), is it valid to include the daytime points at all; you note that the most of the points (86%) are in the hours from 00:00–12:00 and 21:000-24:00. Indeed is it valid to include any points with a correlation of say <0.9 in your overall average?

3) Table 2 – mention in the header that the conversion of CO2 mixing ratio to a mass concentration is done at the lower elevation (pressure) and higher temperatures associated with your measurement site.

4) Abstract - define T0 and MCMA

5) Page 6654 – lines 22-24 – "which can also nucleate. . ." – is this different from the homogeneous nucleation mentioned on line 15?

6) Page 6655, line 12 – the largest "megacity" is also the largest city in NA?

7) Page 6656, line 3 – define MCMA

8) Page 6656, line 22 – particleS

9) Page 6657, lines 9-10 – you mean volume-controlled growth?

10) Page 6659, lines 13-15 – Potentially it could if there were significantly different slopes within your # to CO2 correlation plot, but only one slope is evident in your figure 4, excluding of course the new particle events.

11) Page 6662, line 1 – To enable rather than To improve. They are not comparable otherwise.

12) Page 6671, lines 12-24 – You discuss here that including the apparent new particle formation events (Table 3) does not impact your emissions factors. Then you point out that afternoons have lower emissions factors as well as fewer identified points. Do you believe the sources were the same from day to night?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 6651, 2011.