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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Eddy covariance flux measurements of pollutant gases in urban Mexico City" *by* E. Velasco et al.

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Review by S. Madronich, guest ed., in lieu of referee #2

The manuscript describes the use of fast organic sensors and eddy covariance methods from a tower in central Mexico City, to measure concentrations and fluxes of several hydrocarbons and CO2. These measurements are used to evaluate the most recent (2006) bottom-up inventory for Mexico City. Such evaluations are critical to understand the uncertainties in the emissions and therefore in the 3d modeling of the urban environment. The manuscript is appropriate for publication in the MILAGRO special issue, after consideration of minor suggestions listed below.

1. The methanol story is not presented clearly. Apparently, the 2006 inventory has

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some methanol point sources but no mobile sources. What is the nature of these point sources? (p. 8014/lines 22-24): Why are methanol emissions underestimated by the inventory over the local footprint, but seem to be much higher when considering all 63 cells? Shouldn't the measured fluxes be compared to the footprint-weighted inventory? Why is the 63-inventory more relevant than the footprint? (8016/3): Please clarify the logic/evidence for mobile sources of methanol. In general, I urge the authors to expand their discussion of methanol sources.

2. At various points throughout the text, there is an implication that concentrations and fluxes contain fundamentally different information. While this is generically true, the specific implications are not clearly stated. For example (8005/15-17) as written, you are implying that concentrations are not due to anthropogenic activities (which is false, of course). Perhaps you mean that concentrations depend on emissions as well as removal (chemical or ventilation), while fluxes depend strictly on emissions. Please rephrase to clarify. Similarly, on (8007/23,24): Both long term trends and the differences between sites could lead to differences in concentrations, so I don't understand the logic leading to the conclusion that emissions have not changed over the 3 years.

Minor comments:

7993/16-19: It isn't the propagation of the errors, it is the errors themselves that are the problem. A more direct phrasing could be: The large uncertainties associates with this bottom-up process reduce the utility of ....

Throughout text: time notation should be consistent, e.g. 15:00 not 3:00pm.

7997/7: Delete 'To instrument the flux tower,'

8004/13-15: (for methanol) why would a weaker diurnal variation with peaks suggest that photochemical processes are important?

8004/27: The enhanced NOx and CO during the evening hours of Friday and Saturday and the early hours of Saturday and Sunday was also noted by Stephens et al. (2008,

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9, C3938–C3940, 2009

this special issue) and attributed to 'increased weekend evening activities.' The enhanced values are clearly visible in Figure 1 of Stephens et al. This is a better citation than the Sanchez et al. report which is gray literature and not generally available to the scientific community.

8005/1: Friday and Saturday should be capitalized.

8005/19: yet methanol fluxes show a diurnal cycle, so there should be at least some accidental (not necessarily causative) correlation with T and radiation.

8006/3: Is this factor of 4.5 relative to the 6am value, or to the nighttime values? Is it useful to give this enhancement factor relative to very small nightime values?

8006/19-20: Again, not clear to what the increments (factors of 1.7 and 2.2) refer. Is this relative to the night-time average?

8006/23: How robust is this second peak? Figure 3c shows five distinct peaks for methanol during the daytime hours, all of comparable size. How is one to know which of these peaks are statistically significant?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 7991, 2009.