

Interactive comment on “An overview of the MILAGRO 2006 campaign: Mexico City emissions and their transport and transformation” by L. T. Molina et al.

Anonymous Referee #1

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General comments: This paper provides an overview of the spring 2006 MILAGRO field campaign. A well-crafted overview paper is important for such a field study, as it serves not only as a guide to the goals, operation and results of the program, but also ties the results together in a coherent manner. This paper largely achieves these objectives. The paper is generally very well written (with a few exceptions noted below), and follows a clear and logical thread, introducing the entire MILAGRO program including its historical context, describing the measurement platforms and instruments, and then focusing on an extensive summary and synthesis of the results.

I have one primary reservation about this paper: it is extremely long (165 pages). The

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authors would be well advised to eliminate simple recitation of quantitative results and focus on a) the synthesis of results where possible and b) qualitative description of the work done with reference to the published papers. As much as possible recitation of quantitative results from published work should be eliminated. In these regard, I think that Section 8 on PM can be taken as a guide. I recommend that the paper be published once the following specific points have been addressed.

Specific comments:

- 1) The caption for Fig. 2 needs improvement with regard to ozone. It is clearly not simple annual averages that are plotted.
- 2) In Fig. 10, the geographic location is not clear. Some specific features on the ground (e.g. the outline of MCMA from Fig. 1) should be clearly shown to orient the reader. Or is that the Gulf Coast of the U.S. shown? If it is a much clearer presentation is required.
- 3) In the discussion of Fig. 10 the OH reactivity of background CO should be indicated. Only a fraction of the CO reactivity at the furthest extent is due to MCMA outflow. It should be contrasted with the reactivity of background CO, which is something like 130-140 ppbv in March at northern mid-latitudes.
- 4) Pg. 7866 – I do believe that the conclusion of the following statement is necessarily accurate: “However, afternoon ozone concentrations showed minimal changes over the weekend with occasional increases, providing direct empirical evidence that ozone production is VOC-limited.” It may be that simple titration of ozone by the higher NO_x emissions on weekdays accounts for the weekend-weekday differences. This requires a more complete discussion.
- 5) Pg. 7871 – The following statement is not clear: “An important scientific and air quality management question in many of these megacities is quantifying the relative contributions of different sources such as mobile, industrial, biogenic, biomass burning, etc., as well as separating the contributions from primary emissions vs. secondary

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processes, both of which were an important objective of the MILAGRO Campaign.” Contributions to what is not specified. Total PM, organic component of PM_{2.5}?

6) Pg. 7872 - The following statement is not clear: “Scanning Transmission X-Ray Microscopy (STXM) spectra of collected particles (Moffet et al., 2010), and ¹⁴C content of organic carbon, elemental carbon, Water-Soluble OC (WSOC), and Water-Insoluble OC (WIOC) (Aiken et al., 2009b) were measured in the MCMA.” Was ¹⁴C content of all four (OC, EC, WSOC and WIOC) really measured separately?)

7) Pg. 7886 - The following statement is not clear: “This study found that organics were causing a surface depression of 10–15%.” What was depressed?

Technical Corrections:

1) Pg. 7840, line 19 – Replace “data is” with “data are”.

2) Pg. 7844 – There is no need to reiterate percentages in the text that are already clearly displayed in Fig. 6.

3) Pg. 7846, line 8 – Eliminate the term “research grade, real-time”, as it is ill defined. Perhaps “sensitive, fast response” would be more informative. The same comment applies to pg. 7909, line 17.

4) In contrast to most of the paper, Section 5.3 has several typos and grammatical errors, and the writing is not clear in places. It should be carefully edited.

5) In Fig. 10, the geographic location is not clear. Some specific features on the ground (e.g. the outline of MCMA from Fig. 1) should be clearly shown to orient the reader. Or is that the Gulf Coast of the U.S. shown? If it is the latter, a much clearer presentation is required.

6) Pg. 7865, line 28 – The phrase “. . .that emission controls would depend on location and meteorology” is incorrect. What would be correct is “. . .that the effectiveness of particular emission control strategies would depend on location and meteorology”

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7) Pg. 7905, line 5 – The conclusion “– Many hydrocarbon emissions show greater enhancement ratios in the MCMA than the US.” is not a clear statement in isolation. It should be clarified.

8) Pg. 7905, line 7 – The conclusion – Total OH reactivity due to VOCs in the MCMA remains largely unchanged from the 2003 study; however the speciated attribution is quite different; the present study found that formaldehyde and acetaldehyde were the two most important measured VOC species for OH reactivity - is not clear. Has the VOC speciation in the ambient atmosphere changed, or is the MILAGRO result an improvement over an earlier, erroneous result? This is important to clarify.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 7819, 2010.

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