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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Emission and chemistry of organic carbon in the gas and aerosol phase at a sub-urban site near Mexico City in March 2006 during the MILAGRO study" *by* J. A. de Gouw et al.

J. A. de Gouw et al.

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We thank the reviewer for careful reading of the manuscript and constructive comments. Below we repeat the reviewer's comments followed by our responses.

1. Page 21269, line 9: The sentence about SOA formation in cloud is a little ambiguous - if the authors are suggesting this might be an important source of SOA, perhaps they could clarify?

Several papers have focused on SOA formation in the aqueous phase. We do not think that mechanism explains the discrepancy between measured and calculated SOA observed by several authors (de Gouw 2005, Volkamer 2006, de Gouw 2008, Kleinman 2008), because these studies took place in mostly clear air conditions. We modified

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the sentence to make this clearer.

2. Page 21269, line 15: It would be nice to have a few sentences here describing the T1 site and typical met conditions. Particularly if there might be changing wind patterns that might affect the observed diurnal variations in VOCs and aerosols (or to have this ruled out).

We added a few sentences to the Introduction describing the T1 site and references to Fast (2007) and Doran (2007), which give more details on the surface sites during MILAGRO. We also added a description of local winds to section 3.1 including a new Figure 5 that better explains where the sampled pollutants originated. Two co-authors were added to the paper, A. Salcido and A.T. Celado, who supplied us with the meteorological data from T1.

3. Page 21273: Could the collection efficiency estimated for the AMS also play a role in the relative positive bias of OC?

No. If the collection efficiency were in error, it would result in an overall multiplication of the AMS data, not in an offset in the data. No change was made to the manuscript in response to this comment.

4. Page 21274, line 7: Did the authors examine the data for any markedly different behavior in particular weeks, perhaps using a tracer such as WSOC, which is available throughout the campaign?

A thorough discussion of the observations in terms of the different meteorological conditions would be a very significant addition to our manuscript, which is already long, and we decided against this suggestion. The new paragraph in section 3.1 including the addition of Fig. 5 gives a better description of the source areas sampled at T1. Also, in response to a later comment by this reviewer, we redid part of the analysis for just the period after March 23. See our response to comment #7.

5. Page 21274, line 10: Is the morning peak solely attributable to accumulation in the

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boundary layer, or is there also a contribution from morning rush hour traffic in Mexico City?

The diurnal variation in traffic emissions has most likely a much smaller effect on the early-morning peak in primary species, i.e. traffic does not peak at 6 am. Nevertheless, the reviewer is correct to point out that emissions are not necessarily constant throughout the day and a sentence to that effect was added.

6. Figure 5: Although one expects anthropogenic VOCs and CO to be very correlated in an environment such as Mexico City, I was curious what isoprene vs. CO would look like. Perhaps this could be added to the figure? Was isoprene included in Figure 10? On a related note, given the interest in segregating anthropogenic and biogenic sources, it would be nice to see what fraction of alkenes are made up of isoprene in Figure 13.

We did not quantify isoprene from the NOAA GC-FID measurements. The peaks were too small to be reliably separated from the baseline in our chromatograms. There is a limited amount of isoprene data from T1 from the UC Irvine canisters, but it has not been included in this analysis. The number of samples per day is, for example, not sufficient to determine a meaningful diurnal variation. A sentence to point this out better has been added to section 3.6. For the reviewer's interest: the correlation coefficient (r2) between isoprene and CO was 0.501.

7. Section 3.3: The discussion here is to highlight that biomass burning is present but not an overly important source of organic carbon at T1. It would be reassuring if the authors repeated their analysis of Section 3.2 and 3.4, 3.5 having filtered out the biomass burning influence (perhaps when $>$ 20% in Figure 8b) to confirm that their results are not influenced by fires. If this has been done, perhaps the authors could add a sentence at the end of Section 3.3 indicating that this has been tested.

As it is difficult to accurately estimate the biomass burning contribution on a point-by-point basis (Fig. 8B contains highly averaged data only) we have addressed this

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comment in a different way. Several authors including Fast (2007), Stone (2008) and Aiken (2009) have pointed out that the period after March 23, 2006, had significantly less biomass burning than the period before. We repeated the analyses in section 3.1, 3.2 and 3.5 for just that period, with a special focus on the organic aerosol data. Within the uncertainties, the analysis of the period after March 23 led to the same results as for the entire study, and we conclude that our conclusions on organic carbon emissions and chemistry are not strongly influenced by biomass burning emissions. We considered adding this added analysis to the manuscript, but decided against it: other authors (Aiken, Zaveri) are presently studying the influence of biomass burning on the measurements at the surface sites in more detail and using more data than we are. We did add some sentences at the end of section 3.3 and to section 3.5 to point out that we did these additional tests.

8. Page 21279, line 28: semantics: CO is not "an inert tracer"

Sentence has been changed.

9. Page 21284, lines 6-8: how does the enhancement in OM/OC/WSOC/OOA compare with the study by Volkamer et al, 2006 in Mexico?

Unfortunately, that study did not report any CO data, so the direct quantitative comparison that the reviewer asks for is not possible.

10. Page 21286, lines 23-24: Aromatic VOCs do not have the highest VOC emissions (eg. isoprene) or the highest smog chamber yields (eg. sesquiterpenes) and would thus not be characterized as the "most important precursor of SOA". Perhaps the authors meant among the precursors measured in this environment? This sentence should be clarified.

We added the word "anthropogenic" in front of "precursor".

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 21265, 2008.