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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessing the regional impacts of Mexico City emissions on air quality and chemistry" *by* M. Mena-Carrasco et al.

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General comments.

This paper presents an analysis of regional air quality impacts of Mexico City, estimated by combining aircraft measurements taken at the MILAGRO experiment and dispersion modeling.

The authors have collected a rich database and the results presented in this manuscript are new and relevant for understanding a megacity influence on air quality at different spatial scales. A full month (March 2006) has been analyzed on a daily basis and more detailed analyses are presented for a few days.

Nonetheless, there are several issues that deserve further clarification before the paper

is suitable for publication.

Specific Comments.

Section 2.1.2: emission inventories and boundary conditions

a) There are several sources of emission inventories used to construct the baseline emission inventory for simulating the MILAGRO conditions. i) Were those emissions adjusted to a common year or not? ii) Why was BEIS2 used for biogenic emissions? BEIS 2 seems outdated and has been superseded for models such as MEGAN. Please justify.

b) There is no Table with the aggregated emissions for the whole MILAGRO domain, this ought to be included for comparison with other studies.

c) I would encourage the authors to make their emission inventory available, so that the scientific community may benefit from such info.

Section 3. Results and discussion

The first paragraph needs editing. It does not convince me that the base model run has been obtained from a systematic model improvement. In fact the model overestimates ozone and CO, and NOx (NO and NO2) are underestimated. Correlation coefficients in Table 1 are OK but they do not tell the whole picture; also NOy is overestimated by the model, which is certainly consistent with the ozone overestimation.

Thus there are biases in the emissions of different species, in the boundary conditions of some of them (figure 5) and on photolysis rates (figure 8); these pitfalls have to be taken into account when photochemistry processing is analyzed. As discussed further below, the manuscript needs editing to achieve clarity.

Section 3.2.2. Time series of modeled and observed values along flight track

Figure 5 indicates that CO and ozone boundary conditions may be overestimated, at least for that episode of March 19. Was this behavior persistent for the whole March

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simulation? This BC issue needs to be addressed.

Section 3.2.3 Impact of MCMA emissions on ozone formation and regional photochemistry

In the first paragraph figure 6 is introduced and discussed. Both panels depict the same data with different labels and show two clusters of data points: a large one that extends to high NOz values, and a small one that extends up to 7 ppb of NOz, within 100 km of MCMA and that has a distinctive slope higher that the one defined by the large cluster; both clusters have similar high values of ozone. Thus I wonder what is the slope value reported in the figure. Is that slope computed for all data points or not?

The second paragraph states that aerosols reduce NO2 photolysis (modeling results) and so they reduce ozone production and concentrations (figure 7). Given that the model overestimates ozone (see Table 1) this also shows that there is a problem with the boundary conditions and emission inventories of ozone precursors.

This is an appropriate place to critically examine model runs. One way to sort things out is to acknowledge that the baseline scenario needs further improvements, which will be addressed in another publication. And so the claims on first paragraph in Section 3 would be toned down.

Third paragraph. The authors recognize that photolysis rates are over predicted 10-50 per cent near MCMA. Thus we find another source of model error that needs to be addressed in this section.

That third paragraph then goes back to discussing aerosol effects upon photochemistry, and this discussion would better be in another paragraph.

4. Conclusions

I agree with most of the conclusions presented here, for they are supported by the analyses carried out. I particularly agree with the need of combining model simulations and observations to estimate a megacity air pollution footprint, as reported extensively in

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this manuscript for MCMA and its regional scale surroundings. However, some caveats about model uncertainties ought to be summarized here.

Detailed comments

Abstract. a)Include the period analyzed. b)Please clarify sentence running from lines 17 to 18, write explicitly whether you are referring to observed or modeled quantities. Current syntax is ambiguous to me.

Introduction. Last paragraph. Has the term air quality footprint been used before? If so please cite accordingly.

3 Results and discussion.

First paragraph. Since the paper cited therein is not available to me, I would appreciate a better explanation of the process of adjusting emission inventories, a non-trivial task in modeling studies.

Page 20294. The sentence running from lines 6 to 7 needs some editing.

Page 20296. The sentence running from lines 18 to 20 needs editing: the NOx levels are high because of the MCMA emissions, not because of aerosol-reduced photolysis rates (this can only be a second order effect).

Figure 1. First two columns have the labels swapped.

Figure 4. Check caption, a number (5) is mistyped.

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Interactive Comment