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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of Mexico City emissions on regional air quality from MOZART-4 simulations" *by* L. K. Emmons et al.

Anonymous Referee #1

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This paper describes an evaluation of a global chemical transport model using the extensive trace gas and particulate measurements collected during the MILAGRO field campaign. The model is then used to determine the sources of pollution, the age of pollution, and ozone production over Mexico. The paper is well organized, details of how the analyses are performed are described adequately, and the findings are articulated clearly; however, my primary concern is that the motivation for the particular model analyses needs to be described better. The introductory material should be revised to describe why the topics investigated in this paper are important. The conclusions should also reflect on whether the present results regarding this megacity applicable to other megacities around the world.

The following is a list of other specific comments I have:

1) Page 3461, line 21: Suggest changing “Z” to “UTC” for time.

2) Page 3462, first paragraph: Please include whether estimates of volcanic emissions of sulfur dioxide are included or not in the model run. Are dust emissions included in the model run? Although not central to the paper, the effect of NOy uptake on dust is mentioned on page 3471.

3) Page 3463, line 16: Include the number of flights used in this analysis for both the C-130 and D-8.

4) Page 3463, first paragraph in Section 3: It would be useful to state why measurements from the G-1 aircraft are not included in the analysis. Presumably, the flight paths occur largely in one or a few MOZART grid boxes so the model would show as much variability as observed.

5) Page 3464, line 14: The authors refer to uncertainties in volcanic emissions, but do not state earlier whether they are included or not in the simulation. Previous papers suggest that sulfur dioxide emissions from Popocatepetl could be quite large compared to other sources in the region.

6) Page 3464, line 16: It is true that the simulated sulfate could be improved by adding emissions of sulfate, but it would be better to either add more realistic volcanic emissions or evaluate why the particulate mechanism in the model is producing too little sulfate.

7) Page 3464, line 20: Suggest changing the text to indicate the model over-predicts the average OA concentration somewhat, but there are very large over-predictions at times.

8) Page 3465, line 17: Different regimes are mentioned. Perhaps it would be useful to change Figs. 1 and 2 to use flight paths within the same latitude/longitude box so that the results are more comparable. Or perhaps breaking the analysis in Figs. 1 and 2 into flight paths over central Mexico and those transects further downwind. This

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would provide a means of evaluating model performance as a function of distance from Mexico City. Presumably, MOZART may not be able to represent the large variability of trace gases very close to Mexico City, but the performance would improve farther away from local sources.

9) Page 3465, line 24: As far as the underestimation in photolysis rates, could this be due to uncertainties in simulated clouds? How are cloud fields included in MOZART? This is probably described in another paper, but seems relevant here.

10) Page 3465, line 28: OVOCs has not been defined yet. Here and elsewhere, the authors should check that acronyms are defined before being used.

11) Page 3467, lines 14-16: This sentence does not represent what is in the figure, and needs clarification. I agree that other large cities in central Mexico contribute to regional pollutants, but the figure that is being referred to does not really distinguish those cities from Mexico City. All that is seen is a single source in central Mexico.

12) Page 3468, lines8-11: Why are the biomass-burning emissions seemingly centered over Mexico City? Were the largest fires located in that region during the month? I would have expected that fires would be distributed throughout the region. Perhaps adding a panel on the locations of the biomass-burning source would be helpful to the reader.

13) Page 3468, line 21: The winds did not shift to northerly until March 23 (the third Norte described in Fast et al.) and Fig. 6 shows the increase of US+Canada sources on this day and not March 20.

14) Page 3468, line 22: Change “increases” to “minor increases”?

15) Page 3470: line 5: I am not sure this is a “large “region. Suggest changing “A large region over the Gulf of Mexico, north of the Yucatan peninsula, contains“ to “Just north of the Yucatan peninsula is a region containing”.

16) Page 3470, line 9: “Long ages” seems awkward.

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17) Page 3470, line 16: The second part of the sentence starting at “the majority of the CO” is redundant.

18) Page 3471, line 14: Is there a reference for the box model that can be included? Does the box model contain the same mechanism as MOZART? If not, would this not affect the comparisons in Fig. 9 where the unconstrained MOZART model results are compared to the constrained box model? Please explain.

19) Page 3471, line 21: I do not understand the sentence starting “However, the MOZART-4 . . .”

20) Page 3472, line 1: Can the authors speculate why MOZART had greater difficulty in simulating the high concentrations in this region. Is it primarily the spatial resolution?

21) Page 3472, line 26: At the end of this paragraph, I got the impression that it may be important to compute OPE from MOZART if it is expected to be lower than reality (as a result of resolution). Perhaps, the discussion needs some refinement.

22) Page 3474, line 23: Here an elsewhere, the authors suggest that coarse resolution is a reason for some of the uncertainties. I agree that it is certainly a likely factor. But given other higher resolution trace gas simulations that have been performed for MILAGRO, can the authors provide some evidence that the time series (or stats) from their study differ from other higher resolution model studies?

23) Page 3475, line 2: What is the implication of this statement and why is this relevant.

24) Page 3475, line 11: Similarly to my comment on line 2, what is the implication of OVOCs?

25) Page 3475: Somewhere in the conclusions (and perhaps earlier in the text too) there should be some discussion on how the relative contribution of anthropogenic and biomass burning sources from this study should be compared to other studies of fossil vs modern carbon (via C14 measurements). The C14 studies suggest a much higher fraction of modern carbon. The biomass-burning inventory likely misses many small

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fires and other sources of burning may be neglected by current emission inventories.

26) Figs. 1 and 2: Include how many flights were included in the analysis in the caption.

27) Fig. 3: Add “(MZ4)” after “MOZART”. Change “C130” to “C-130” in the figure labels.

28) Fig. 8: Change “Dashed line” to “Black dashed line”. Include what the white dashed line means.

29) Fig 9: Definition of the red lines and dashed blue lines is not given.

30) Fig. 11: Add “(MZ4)” after “MOZART”. Would it be useful to have a similar plot for those days with strong transport only (e.g. March 19)?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 3457, 2010.