

Interactive comment on “Simulation of Mexico City plumes during the MIRAGE-Mex field campaign using the WRF-Chem model” by X. Tie et al.

Anonymous Referee #2

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This paper describes the performance of a chemical transport model in simulating ozone and its precursors over central Mexico during the March 2006. The model is then used to examine ozone production efficiency and the role of different ozone precursors on ozone production. There are a number of issues that need to be addressed before the paper is suitable for publication. While the material in the paper is presented reasonably well, there are numerous awkward phrases and some descriptions are too generic and lack specificity. I have noted some of the awkward phrases and generic descriptions in my comments below, but I may have not found all of them and I encourage the authors to re-check the rest of the manuscript as well.

Major comments:

1) The lack of a description of the emissions, initial conditions, and boundary conditions

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that were used for the model simulation is a major omission in this study. Emissions are an important input, which should not merely be cited in another paper. Also describing how emissions are prescribed outside of Mexico City would be useful to understand how they contribute to background concentrations. The authors should also include a list of meteorological physics options used, particularly the PBL parameterization. There is description regarding the performance of the PBL, but no description on how it is represented in the model. Nor do the authors provide any direct evidence regarding the performance of the PBL.

2) The adjustment factor was not applied to all quantities in Fig. 6. Why not? It would seem that dispersion errors would affect all species. Also, some of the hydrocarbons are already overestimated and applying the adjustment factors would push the model in the wrong direction (assuming a linear relationship, but of course it is not that simple).

3) Section 4 presents an analysis of ozone production efficiency and the role of different ozone precursors in on ozone production. They use March 22 for this analysis. Given that transport to the northeast occurred on other days during March, have the authors analyzed the results to determine whether their conclusions occurred on other days? It would be useful to include some text whether the behavior of the model on March 22 is similar to other transport periods. For example, would varying meteorological conditions either increase or decreased ozone production efficiency? What about cloudiness, its impact on photochemistry in the region, and whether the model adequately simulated cloudiness? I would also encourage some inclusion of aircraft data in this section to show how the model performed both close to Mexico City and further downwind where the secondary ozone maxima was produced in the model. For example, were OH and HO₂ observations made on the C-130 that could be used to verify the behavior seen in Fig. 13?

4) My impression from reading this manuscript is that ozone chemistry is reasonably simulated, since the authors seem to attribute most of the errors to uncertainties in dispersion. If that is the case, does that mean that simple photochemical mechanism

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such as RADM are all that is needed to understand oxidant chemistry in a megacity plume? Are details of hydrocarbon chemistry not included the lumped approaches significant? Some discussion is warranted in the paper.

Minor comments:

Page 9222, line 1: “in the Mexico City outflow” is awkward and should be rephrased.

Page 9222, line 9: Suggest changing “enhancement of” to “increase in”.

Page 9222, line 12: Suggest changing “pollution levels” to “ozone mixing ratios”, unless the 0-25% underestimation refers to all pollutants in addition to ozone.

Page 9223, line 13: I’m not sure the sentence starting “The campaign coordinated . . .” is entirely correct. Were the coordination with satellite measurements part of MIRAGE-Mex, or was that for the INTEX-B campaign?

Page 9223, line 18: Change “The city is at” to “The city is located at”.

Page 9224, line 12: Suggest making “In addition to . . .” a start of a new paragraph, and further down on line 19 make “Lei et al. . . .” a new paragraph. It was difficult to follow the lines of thought in this long paragraph. It would help if it were re-phrased to provide a better motivation for the present work.

Page 9224, line 26: The previous paragraph mentions a few, of the many, photochemistry simulations performed for central Mexico over the years. The authors should include some statements that differentiate the present work from previous studies.

Page 9225, line 26: “cycling pattern” is awkward and should be rephrased.

Page 9226, line 3: What do you mean by “city plume” that was measured by the aircraft. It would be useful to be specific at first, and then use this term later. There are phrases later in the paragraph referring to where the aircraft intersected the “city plume” but provides no rationale for determining that the measured values originated from Mexico City. Although not as likely, the higher concentrations could arise from other large urban

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areas in the region.

Page 9227, line 28: Suggest making “In this study, . . .” a new paragraph.

Page 9227, line 29: Suggest changing “The model ran from ” to “The model simulation period was from”.

Page 9228, line 5: “measurement” should be “measurements”. But perhaps a more specific statement would be that the model was compared to “ground measurements from operational monitors”.

Page 9228, line 11: Change “field campaign” to “field campaign measurements”. Perhaps one could be more specific to mention that in this study the model is compared with aircraft observations collected downwind of the city, which were not done in the previous studies mentioned in this paragraph. This sort of discussion would have been better at the end of the introduction.

Page 9229, line 5: Averaging both the observations and the model results in Fig. 2 (and in Fig. 3) is a useful method of summarizing model performance. But it also hides many errors in the predicted timing of CO plumes and in the spatial distribution of CO plumes. I think some discussion regarding the variability of model performance is warranted.

Page 9230, line 3: The authors state that the calculated BL height during the evening is better on March 18, but provide no evidence that it is. Just because the surface CO is closer to the observations does not imply the PBL is necessarily correct. Have the authors actually evaluated the predicted PBL depths? While I’m not suggesting that an in depth presentation of the PBL predictions be given, at least some comparisons with the available data should be made in light of the discussion for Fig. 3. Similarly, the model discussion needs to include a description of which PBL treatment is used in WRF.

Page 9231, line 1: The authors state tat the March 28 flight for CO is shown to provide

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some insights into the background atmosphere, but they present no such insights. What do they mean by this? Also, the scale of the plot is such that one cannot tell the difference between the observed and predicted. There does seem to be some differences suggesting errors on the predicted background concentrations (which also seem to be evident on other flights). Again, having a description on how boundary conditions were handled would be useful.

Page 9231, line 10: A criterion is described for the “city plume”, but it would seem that an increase in CO could arise from other urban plumes the aircraft encounters – not just the Mexico City plume.

Page 9231, line 22: Change “transport processes” to “dispersion processes” since transport is usually thought of as transport by the mean winds (and included in the parentheses already) and diffusion is a turbulent mixing process. Dispersion represents both mean and turbulent processes. But I’m not sure I agree with the reason for the underestimation stated in this sentence. The errors associated with dispersion are just speculation since an evaluation of transport and mixing has not been presented in this paper. Another plausible explanation is uncertainties in emission rates.

Page 9232, line 2: I understand why one would want to create an adjustment factor based on CO, that could be applied to other species to ‘correct’ for transport errors. However, the differences between observed and simulated CO are not solely due to dispersion processes. As stated earlier, part of the problem could be due to emissions that vary from day to day. And the uncertainties in the emissions of other species are not likely to be the same as CO (which is probably the specie with the best emissions estimate). The authors should include some text to note the assumptions regarding their correction factor in Eq. (1).

Page 9237, line 23: “during outflow” is awkward and should be rephrased.

Page 9283, line 6: Is there a reference(s) that can be provided on the use of Eq. (2) for photochemical age?

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Page 9239, line 6: “lower in nearby city” is awkward and should be rephrased.

Page 9240, Section 4.4: The O₃ to NO_x comparison is similar to the model predictions in Mena et al. (2009) along the C-130 flight tracks. What does this mean in terms of different chemical mechanisms employed by the two models?

Page 9241, line 14: I assume that the secondary ozone maximum downwind of the city is from previous days emissions? Why not use the model results to say something about this history of this air mass?

Figure 4: The blue and green are difficult to distinguish. Suggest changing to other colors. Same comment for Figure 5.

Figure 6: The black and blue are difficult to distinguish. Same comment for Figures 8-10.

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