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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparisons of WRF/Chem simulations in Mexico City with ground-based RAMA measurements during the MILAGRO-2006 period" *by* Y. Zhang et al.

Y. Zhang et al.

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Response to Reviewer 1:

We are very grateful to the reviewer's thoughtful comments for improving the manuscript. Here are our point-to-point responses to the reviewer's comments.

1. Pg. 1335 line 10: Please state the scaling factors (if any) used for CO and VOC.

Response: Scaling factors used for VOC species are listed in Table 2 in Lei et al. (2007, Atmospheric Chemistry and Physics) and are mentioned in the revised version of this manuscript. There are no scaling factors listed for CO in Lei et al. (2007, Atmospheric Chemistry and Physics).

2. Pg. 1336 line 19: 6 hour initialization seems very short for the meteorological fields, even if the chemical initial conditions have little impact on concentrations.

Response: We have done sensitivity experiments with 6-hour, 12-hour and 24-hour initialization time for meteorology and we have not noticed significant effects of longer spin-up time on the simulated meteorological fields. Therefore, 6-hour spin-up time was used for the entire runs.

3. Pg. 1338 line 27: "deficiencies in the parameterization of mixing processes"- This is so vague that it provides little useful information. The same applies to Pg. 1352, lines 16-19. The statistical analysis could be used to point to more specific areas of improvement or model limitations.

Response: We agree. We meant to say that the model appears to simulate excessive mixing during daytime as reflected by dry and cold biases since excessive mixing would bring down too much dry and cool air aloft. This statement is included in the revised version.

4. Pg. 1339 line 1: RAMA has been found to have weak winds in past studies, a comparison with other data sources would strengthen/qualify this statement.

Response: We obtained observations from some of the Mexican National Weather Service sites and compared the RAMA measurements at stations that are adjacent to the Mexican National Weather Service sites. We noticed that the RAMA measured surface winds (as well as other meteorological variables) are rather consistent with the National Weather Service observations at those stations.

5. Pg. 1339 line 14: This merely reflects the fact that 350 degrees is only 20 degrees off from 10 degrees and therefore no worse than 170 and 190 degrees. I would be more interested to read about the collection of points with 180 modeled and 360 observed winds.

Response: We agree. The statement is removed in the revised version.

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6. Pg. 1339 line 21: There is a UCM option at present in WRF. As it stands, the paragraph is mainly speculation.

Response: We agree. The statement is removed in the revised version.

7. Pg. 1340 line 25: There are estimates of these sources (eg. Grutter et al., in this special issue). These should be included or the discussion about SO2 should be removed.

Response: Discussions about SO2 are removed in the revised version.

8. Pg. 1342 line 1: This discussion would benefit from being more specific, maybe using bias as well as ANB, and looking at individual sites.

Response: More specific discussions are proved in the revised version.

9. Pg. 1342 line 15: There are just a few days in this comparison, the result could be because of the weather conditions on those days. If the model does not simulate SO2 well, then no conclusions can be drawn from this.

Response: We agree. The discussion is removed.

10. Pg. 1343 sec 5.3.1: This section needs more detail as ref 2 points out.

Response: We have contacted Dr. William Shaw and asked for their PBL data based on rawinsonde, lidar and profiler measurements but we have not got the data, yet.

11. Pg. 1344 sec 5.3.2: Because this analysis is based on just one day, selected arbitrarily, it does not contribute to the goals of the paper. The sensitivity tests could have been carried our for several days, or the whole time period, and analyzed in greater detail.

Response: We ran the model for 5 days (March 13-March 17) using the combinations of LSM and PBL and presented the analyses based on 5-day simulations.

12. Pg. 1347 line 3-12: The episodes selected do not seem to match with the reference

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provided.

Response: Yes, there are some differences in the episodes selected since we based our selections on our extensive examinations of daily synoptic conditions at low level (850 mb) and middle level (500 mb) from the NCEP Final Analysis data as well as the daily mesoscale conditions and ozone distributions from our WRF simulations during 3-30 March, 2006. Also, de Foy et al. (2008, Atmospheric Chemistry and Physics) lists more types of weather episodes while we stick to three basic types: O3-South, O3-North and El Norte.

13. Section 5.4: This section is too vague and does not contribute to understanding model performance.

Response: We seem to disagree with the reviewer. In this section, we examined the model performance in resolving the spatial distributions of surface winds and pollutants concentrations associated with specific weather episodes as well as statistical evaluations. We would argue that our analysis is comprehensive and the analysis results contribute to understanding model performance under specific weather conditions.

14. Pg. 1353 lines 10-13: It should be kept in mind that the smaller the diurnal variation, the lower the correlation coefficient will be. A lower CC at night does little more than reflect the fact that during the day there is a strong increase and decrease in concentration, whereas at night concentration fluctuations are much smaller. The same goes for the cleaner days. This issue should be addressed in the discussion about statistical measures in order to draw meaningful conclusions from the analysis.

Response: We agree with the reviewer that a lower CC at night is related to the fact that concentration fluctuations at night are much smaller. We revised the text in the manuscript to reflect this. Between weekdays and weekends, major pollutants (CO, NO, NO2, and NOx) concentrations show slight decreases from weekday to weekend while O3 concentrations change minimally between weekdays and weekends and are occasionally higher on weekends as noted in Stephen et al. (2008, Atmospheric

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Chemistry and Physics) and our simulations. Therefore, it seems justified to suggest that there are large uncertainties in the temporal distributions of the emissions rates for weekends.

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Interactive Comment