

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.

Anonymous Referee #1

Received and published: 3 March 2009

This review shares the concerns of referee 2 which should be addressed for publication. Additional comments are as follows:

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

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- Pg. 1352, lines 16-19. The statistical analysis should be used to point to more specific areas of improvement or model limitations.
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.
 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.
 6. Pg. 1339 line 21: There is a UCM option at present in WRF. As it stands, this paragraph is mainly speculation.
 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.
 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.
 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.
 10. Pg. 1343 sec 5.3.1: This section needs more detail as ref 2 points out.
 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 out for several days, or the whole time period, and analyzed in greater detail.

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12. Pg. 1347 line 3-12: The episodes selected do not seem to match with the reference provided.
13. Section 5.4: This section is too vague and does not contribute to understanding model performance.
14. Pg. 1353 line 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.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 1329, 2009.

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