

***Interactive comment on* “Characterizing ozone production and response under different meteorological conditions in Mexico City” by W. Lei et al.**

Anonymous Referee #2

Received and published: 14 August 2008

Review of Manuscript ACPD-8-12053

Characterizing ozone production and response under different meteorological conditions in Mexico City

By Lei et al.

Lei et al. present a modeling study of the air pollution dynamics in the Mexico City airshed under distinct meteorological conditions. The main objective of the paper is to analyze the response of ozone under these meteorological conditions, and establish implications of the findings on emissions control strategies. The paper is concise and,

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



in general well written, though additional editing should be conducted. In their attempt to be brief, the authors left out valuable information on the methods and data used that should be included (as mentioned below). This merits extensive review of the paper before considering it for publication.

General (major) comments:

1. The literature review of the authors is limited to two recent papers, which might give the wrong impression on the amount of literature available on the subject. There is a vast amount of literature that has been published on the influence of meteorological conditions on photochemical air pollution. Authors should acknowledge this literature, and try to make reference to classic papers in this area.

2. P. 12056, line 14. Briefly describe the model configuration and modeling domain. Even though this information has been published elsewhere, it will help to get an overall idea on how the modeling was conducted. For example, in Table 1 several emission categories are indicated (e.g., ALKx, OLEx, etc.) What do they stand for? What species were considered for each category? Is this a direct function of the chemical mechanism used by the model? If such, what chemical mechanism was used?

3. P. 12056, line 22. Why were two distinct emissions inventories used?

4. P. 12056, line 26. Authors indicate that "...an extensive array of ground measurements for VOCs were made..." and used in their study. However, then they indicate that VOC measurements were only conducted in three sites. Finally, in the conclusions (and before in the text) they indicate that "...VOC comparisons were still made over limited locations...". There seems to be lack of consistency in these comments.

5. P. 12057, lines 7-8. It is indicated that some emissions estimates were compared against values obtained by Zavala et al. (2006), though I did not find any further comments or evidence on this in the following sections of the paper.

6. One key issue of the paper is the emissions manipulation. Authors indicate that they

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



used the procedure devised by Lei et al. (2007) to evaluate the emissions for these new model applications. As the model is driven heavily by emissions, more needs to be said about these and the procedure devised by Lei et al. (2007). As implied by the paper, emissions were adjusted until they obtained a "satisfactory agreement". How and on what basis were the emissions adjusted? Was this based on a stochastic approach? Was this a one-at-a-time change for each species, or was it a multivariate approach? What do they mean by "satisfactory agreement"? What was the metric used to assess this? How sure are you that you got the "right" combination of adjustments given the non-linear response that one could get from these changes? These questions might have an answer in the Lei et al. (2007) paper, though I believe that the issued is so relevant, that further comments are needed in this paper.

Following the above discussion, authors indicate that from Figure 2 it can be implied that they got "good agreement" between VOCs observations and simulated values. Be more quantitative (how "good" is "good"?), and compare with what others have obtained elsewhere. Clarify in Figure 2 what the lines represent (1-sigma values? 2-sigma? max and min?) What episode is being presented in Figure 2?

Throughout the paper, the term "uncertainty" is used to express the plausible error in the emissions. I have trouble with how the authors are using the concept of uncertainty. Given that they refer to the uncertainty of an empirical quantity, it is expected that a corresponding probabilistic analysis accompanies the description of the variable. For example, in P. 12058, line 5, authors state that "...are accurate (within 10%)". How should we interpret this? As a 1-sigma std dev?

P. 12058, line 12. It is not clear what is meant by "variations in different EI base years and locations". The modeling is conducted for the year 2003, so why are there different base years? Was a location-by-location analysis conducted? This is not presented.

A statistical model performance evaluation is conducted for O₃, NO_y, and CO. Similar values should be presented for VOCs given the availability of the data.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

7. P. 12059, line 1. How does Figure 4 shows that "O₃ was too high because the simulated component (gap flow) was too weak...". Could there be another reason besides the weak gap flow to this observation in the model performance? Any reason why the met model did not capture accurately the prevailing wind conditions on April 10 and 26?

8. P. 12059, line 16. Model performance was "reasonable". What is the benchmark used to make this statement?

9. Fig. 5. The way the figure was constructed does not allow getting a clear interpretation of the results. There is high degree of scatter and overlapping of the data clouds. Averaging or other techniques of data reduction might help.

10. Fig. 6. Same comment with respect to Figure 5; too much overlapping limits the interpretation of the plot. Try to use a data reduction technique.

Minor comments:

P. 12054, line 6. I recommend not including the terms in parenthesis (Cold Surge, O₃-North, and O₃-South). There is no need to be so specific in the abstract.

P. 12054, line 8. Instead of using the term "weakly", be more quantitative.

First paragraph of the introduction: Review the wording. It seems that the authors are referring to photochemistry of polluted air; though, as written, one might indicate that when referring to the photochemistry of the natural troposphere one does not talk about "pollutant precursors".

P. 12056, line 10. "...improve out understanding of air pollution in megacities." This gives expectations that cannot be satisfied. The comment should be limited to understanding air pollution in Mexico City, which is the objective of the paper.

P. 12055, lines 28-29. "evolution" instead of "evolutions". "...response of the urban plume.": response to what? Please clarity.

P. 12057, line 20. Show on a map the location of the sites where the VOC samples were obtained.

P. 12060, lines 7 and 10. Change "...episode is..." for "...episode are..."

P. 12060. Figure 7 is mentioned before Figure 6. Line 14: Figure 7 does not reflect what it is being discussed in the text.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 12053, 2008.

ACPD

8, S6000–S6004, 2008

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

S6004

