

Interactive  
Comment

***Interactive comment on “Modelling constraints on the emission inventory and on vertical diffusion for CO and SO<sub>2</sub> in the Mexico City Metropolitan Area using Solar FTIR and zenith sky UV spectroscopy” by B. de Foy et al.***

**B. de Foy et al.**

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The authors would like to thank the referees for their careful reviews which have been used to improve the quality of the paper.

Referee #2:

**General Comments** The wording was tightened. Uncertainty limits have been added for all measurements and uncertainties of the model results discussed in greater detail.

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**Specific Comments** 1. The conclusion has been rewritten to match clearly the discussion in the paper.

(a) Reference removed.

(b) Wording corrected.

2. Sequence of description/discussion/conclusion has been clarified.

3. The introduction was shortened

(a) Discussion was clarified. Because this work uses the same meteorological simulations as the paper showing the short residence times, these are implicitly used.

(b) Both the emissions and the inventories are in a state of flux. Wording modified.

(c) The EDGAR inventory was used to support my claim that the boundary conditions sufficiently accounted (within the accuracy of the present simulations) for neighbouring emissions. I would rather keep supporting evidence where possible.

(d) OK, I've cut them out.

(e) Removed with pieces moved to appropriate sections.

(f) Done for the trajectories. For the vertical dispersion schemes there is not very much literature on the subject and I think it is worthwhile to review what there is.

4. (a) By using 2 axes, the plume can be seen along 2 different lines and the time taken to go from one to the other can be measured giving an actual speed measurement. Explanation + reference added.

(b) All emissions now changed to tonne/year. The current Popo emissions are on the low side compared to previous, more active, periods.

(c) Based on the standard deviation of the measurements, it is estimated that the uncertainty is 35%.

5. (a) Yes, added to the map.
- (b) This was summarised and one of the figures removed. I should stress however that the point of doing the Concentration Field Analysis for CO is to validate the method and to get a handle on its limitations. Only then can it be used to explore unknown sources of SO<sub>2</sub>.
- (c) Indeed, boundary condition changed to 1ppb based on GOME satellite data from IASB-BIRA. This does not affect the discussion however.
- (d) OK, I added a table with the data, and Index of Agreement was removed (it is explained in CJ Willmott, 1982). I however retained one of the diagrams with a clearer explanation. Whereas some readers will be more comfortable looking at a list of 275 numbers, others may find a graphical representation useful. In particular, I want to show how too much averaging removes useful information. The scatter in the metrics is itself informative. The metrics are done for the entire episode. Doing them by day can also be informative, but provides too many points (23 stations \* 34 days).
- (e) These were added to the introduction and references to previous papers given.
- (f) The table now provides all the detail.
- (g) A paragraph with references to detailed site information was added in the measurement section. This section was summarised with reference to places with supporting evidence.
- (h) Figures added with all columns. Scatter plot added for all cases. Table added with coefficients of line of best fit.
- (i) Detailed evidence was provided in de Foy et al., 2006, and is now referenced.
- (j) Clarified and condensed.
- (k) Clarified and condensed.

- (l) Done.
6. (a) It is the authors' hope that having evaluated the method with CO makes the results for SO<sub>2</sub> all the more interesting.
- (b) Yes, Tula and Popo are big sources.
- (c) Clarified. The offset was applied to CENICA data as part of continuing QAQC. Fig 16 and the associated discussion was removed entirely.
- (d) Clarified and condensed.
- (e) Yes, TLI added to map.
- (f) Yes, emissions were held totally constant for simulations of Tula and Popo. The Tula plume usually goes west and/or is diluted by vertical mixing. Only stable northerly flow brings it to the city.
- (g) Having laid out what evidence there is, it is up to each reader to draw his/her own conclusions on this question.
- (h) This is clarified. There is some round off error - with uncertainties at 35%, it seemed to make sense to round off the emissions to 5 kg/s.
- (i) As discussed above, the Tula plume only impacts the MCMA under certain meteorological conditions.
- Minor Comments (a) Yes, diffusion changed to dispersion where appropriate.
- (b) Latitude and altitude added.
- (c) Changed to residence time.
- (d) Data are available for the following species at MER: H<sub>2</sub>O, CO<sub>2</sub>, CO, CH<sub>4</sub>, C<sub>2</sub>H<sub>2</sub>, C<sub>2</sub>H<sub>4</sub>, O<sub>3</sub>, NO, N<sub>2</sub>O, NH<sub>3</sub>, HNO<sub>3</sub> and HCHO. At CENICA, the following are available: CO, CO<sub>2</sub>, HCHO, CH<sub>4</sub>, N<sub>2</sub>O, alkanes.
- (e) The orthographical data points in question were adjusted.

Referee #1:

**General Comments** Wording was clarified.

- Specific Comments**
1. There is indeed a lot of uncertainty in the results due to the weak, variable and complex winds in the basin. I have tried to clarify this in the text but cannot do so for every case without speculating.
  2. The model underestimates CO concentrations at CENICA as explained in the text. This is attributed to the current spatial distribution of the emissions in the model.
  3. This was added with reference to Chow et al., 2002 who have a descriptive table, and to the RAMA website that have maps, descriptions and photographs. Emissions profiles are shown in Figures 2 and 3. For more information the reader is referred to the bibliography.
  4. The model works best for O3-South episodes, followed by O3-North episodes and worse for Cold Surges, as discussed in de Foy et al., 2006. The strong variability among the stations is shown. It is the belief of this author that detailed analysis of measurements and models can provide valuable feedback for the development of inventories.
  5. Graph changed. A second graph was not added as this is intended to show peak rather than baseline levels, as the referee mentions. Fig. 14 was removed as the purpose of the paper is not to analyse the baseline levels, but rather to focus on the peak episodes. The comparison can be seen in the revised Fig. 13.
  6. Because of the fixed site nature of the instruments, an intercomparison of these 2 specific instruments was not carried out. They have individually been validated elsewhere. Note that at MER the model overestimates the DOAS measurements. Problems were identified with some of the point measurements and so these have been removed from the discussion.

**Discussion** This is the evidence that there is, let each reader decide for themselves.

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Interactive comment on Atmos. Chem. Phys. Discuss., 6, 6125, 2006.

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