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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling constraints on the emission inventory and on vertical diffusion for CO and SO_2 in the Mexico City Metropolitan Area using Solar FTIR and zenith sky UV spectroscopy" *by* B. de Foy et al.

Anonymous Referee #2

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General Comments:

The paper addresses an important topic: constraints on CO and SO2 emission inventories for the Mexico City Metro area from comparison of models and measurements. The conclusions are generally valid and the paper should be published. However, the paper is presently poorly organized, over long, contains a mixture of convincing and unconvincing analyses, and has too much unfocused, rambling discussion. The paper should be thoroughly rewritten with the unconvincing analyses and unnecessary discussion removed, and the convincing analyses presented much more clearly. In particular more attention should be paid to assigning quantitative confidence limits. Specific suggestions for revisions are specified below.

Specific Comments: 1) The authors should reconsider the conclusions section, with the goal of clearly and concisely stating their conclusions and discussing their implications. Superfluous comments should be removed. Two specifics: a. p. 6152, l. 27 - There is no need to mention satellite remote sensing, (especially since satellite CO measurements are not very sensitive to boundary layer CO, so would be a poor choice for validating vertical dispersion at the lower model levels.) b. p. 6153, l. 3 - It seems to me that the dispersion modeling was, in many cases, in poor agreement with surface measurements.

2) The authors should then consider each preceding section to see that it directly, concisely and accurately leads to the conclusions. Some specific suggestions follow.

3) The introduction is very long. The authors should limit the information to only that needed in the discussion that follows in the paper. Specifically: a) p. 6128, l. 4 - One study concluded that there were "short residence times in the basin and little carry-over from day to day." But another study cited earlier (p. 6127, l. 19) mentions a 2 day lifetime in the basin. Which is correct? The introduction should paint a coherent picture for the reader to understand and upon which the following discussion is to be based. Will the authors assume short residence times or 2 days? Or do they assume any particular lifetime? If not then perhaps these two references and the discussion of residence time is not needed. b) Section 1.2, 2nd paragraph - this section should make clear if the emissions inventories have changed or if the emissions themselves have actually changed. c) p. 6129 - I see no need to mention EDGAR inventory. Make it clear that the MCMA inventory does not include Tula and Popocat'epetl so they have to be taken from the BRAVO inventory. d) The last two paragraphs in section 1.2 seem to contain unneeded detail. e) Section 1.3 seems unnecessary. f) Sections 1.4 and 1.5 should only introduce the methods that will be applied here, not a long review of source identification methods and vertical diffusion.

4) Section 3: a) 1st paragraph, section 3.2 - How can mini-DOAS give wind speed measurements? b) 3rd paragraph, section 3.2 - The emissions of Popocat'epetl measured here should be compared with the results given in the introduction, which are in different units. c) Uncertainty limits should be given for all of the measurements that are quantitatively used in the study.

5) Section 4 a) Pg. 6141, lines 11-12 - Clearer explanation is needed. At least this reader does not know where to look for "the Mexican Plateau, from the pass to Toluca and from the Chalco passage" b) The last 2 paragraphs of section 4.1 and Figure 5 should be eliminated. The whole Concentration field analysis is of very limited interest, and applying it to individual stations gives no results of interest. c) The choice of "All boundary and initial conditions for SO2 were set to 4 ppb" needs more justification; to me this seems like a very high value. d) The statistics diagram of Fig. 7 is a poor choice for presenting quantitative results. What exactly are the data points? Figure 1 suggests that there are 7 measurement sites, each measuring for about 34 days, so I expect very many more data than are shown by data points. It is not clearly explained how the ovals and circles are defined, or even if they have quantitative significance. If they are subjectively drawn to guide the reader's eye, they are misleading. A much clearer explanation is required, or a different, more quantitative comparison approach must be taken. What is index of agreement? e) Pg. 6143, lines 17-20 - The authors discuss several episodes, but these are not introduced. If this discussion is retained, then those episodes must be clearly introduced. f) The authors state, based on Fig. 7b, that "MER and IMP are the most accurately represented by a wide margin." Yet doesn't this plot just deal with correlations and not accuracy? Where are these points in Fig. 7a? Clearer explanation is required. g) At the bottom of page 6143 the authors speculate that "For PED, the poor performance may be due to shifting emission patterns whereas for TAX it may be due to local sources as this station is located in a major bus transport hub. The statistics diagram can also suggest possible problem areas. Both MIN and SAG are near stations that perform well (MER and XAL, respectively) although their statistics are noticeably worse. This may be due to very local effects that

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impact one station but not its neighbour - whether due to emissions near-by or to micro-meteorological impacts." This speculation must be supported by detailed discussion or removed. h) Fig. 8 shows only about 70 SOF column CO measurements, yet section 3.1 reports that over 120 measurements were made. Fig. 8 should be supplemented with a figure that plots modeled versus observed CO column for all SOF column measurements. The slopes, intercepts and correlation coefficients for the 3 model cases should provide the best basis for judging the CO inventory. i) On pg. 6145 the authors state that "Agreement on 21 April is not nearly as good. This is attributable to the fact that this is a Cold Surge day, with heavy clouds and some rainfall. Performance of the meteorological model was noticeably reduced during such events." What leads to this attribution? The only meteorological variable likely to significantly affect column CO is horizontal wind speed. Is the wind speed poorly reproduced by the model? Unless the authors can demonstrate evidence for the attribution, the speculation should be removed. j) In Fig. 8 it is not clear which panel is Santa Ana. The final three paragraphs of Section 4.2.1 are largely unsupported speculation. They should be greatly condensed, or greatly expanded with clear support for the conclusions. I suggest the former. k) Section 4.2.2 needs work. Figure 10 is too confusing; at least the range, and probably the 25 and 75 percentile lines should be removed for clarity. Much of the discussion is simply speculation: it must be removed or supported with clear demonstration through strong data analysis. l) When Sections 4.1 and 4.2 are revised, section 4.3 should be revised and significantly shortened to discuss only the major points that have been clearly established in the preceding sections.

6) Section 5 a) Here the concentration field analysis is illuminating for SO2, and should be retained in contrast to the analysis for CO. b) Section 5.2 - The emissions for Tula and Popocat'epetl should be given in the same units as Fig. 3 for easy comparison. These numbers should be placed in context. If I have done my math correctly, Tula emits about 10 times as much SO2 as the rest of the point and area sources combined. This should be made clear to the reader early in Section 5.2. c) Figure 15 (as Figure 10) is too confusing; at least the range, and probably the 25 and 75 percentile lines should

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be removed for clarity. Further the CENICA monitoring data should not be shown if they are suspect. d) Beginning with the discussion of Figure 15 on pg. 6149 through the end of Section 5.2, the discussion is confusing and highly speculative. It should be greatly shortened, and only concrete statements based on strong analyses included. e) The location of the site TLI should be indicated in Figure 1 f) I find Section 5.3 of most interest. Do I understand correctly that the Tula emissions in the modeling in Figures 12-17 are constatnt? If so, then where do the emissions go during most of the time when there is no SO2 plume event in the city? This should be discussed, along with the rarity of plume events in Figure 13. g) p. 6151, l. 5-6 - The authors state that "Both the measurements and the simulations suggest that the SO2 plume originated to the north of the MCMA, possibly at the Tula industrial complex." Given that the Tula complex is the only possible source for such a plume, perhaps this conclusion should be stated more definitvely. h) p. 6152, l. 8 - How do the emissions for Tula in Table 1 compare to the 5 kg/s assumed in the modeling? i) p. 6152, l. 17-20 - Following up on point f) above, if the Tula source is 10 times greater than the total of all other sources in the MCMA, then why is the Tula contribution only 20%. This should be clearly discussed.

Minor Comments and Technical Corrections: 1) In the title of the paper the term "diffusion" is not appropriate. As the authors seem to realize, the vertical transport in the atmosphere is not physically due to diffusion (even though models parameterize this transport with diffusion treatments), but rather by complex turbulence mechanisms. The term "diffusion" perhaps should be replaced by "dispersion". 2) p. 6127, l. 9 - Here the latitude of MCMA is particularly important; it should be given. 3) p. 6127, l. 19 - The term "lifetime" generally refers to removal from the atmosphere. Here the term "residence time" would be better. 4) 2nd & 3rd paragraphs, Section 3.1. The species measured by long-path FTIR should be mentioned. 5) The word data is plural, this should be corrected throughout the paper.

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