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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Hit from both sides: tracking industrial and volcanic plumes in Mexico City with surface measurements and OMI SO_2 retrievals during the MILAGRO field campaign" *by* B. de Foy et al.

Anonymous Referee #1

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General remarks

The paper investigates the contribution and transport of SO2 emitted from two point sources (Popocatepetl volcano and the Tula industrial complex) in the Mexico basin for some episodes during the MILAGRO field campaign. Model simulations are compared with SO2 surface measurements, while SO2 column measurements from satellite based remote sensing and vertical wind profiles are used for aiding the interpretation of the results. The work takes advantage that both point sources have been well characterized during the same campaign by other authors and that the urban sources

from the official inventory are well known.

The presented surface measurements, vertical wind profiles and OMI satellite data offer a unique possibility to check the model performance and identify possible weaknesses in the simulated transport processes and possible errors/uncertainties in the source estimation. The use of SO2 as tracer to investigate the flow patterns in the Mexico megacity and to check the coincidence of model results with satellite data is original, the topic is relevant to the reader of this special issue and therefore within the scope of ACP.

However, the paper shows model results, measurements and satellite products in a descriptive way rather than use them together and compare them, so that the conclusion about the model and the sources is vague and not much can be learned apart the rough estimation that during MILAGRO, more SO2 reaches the surface of the city from the Tula region than from the volcano. Publication is recommended only after addressing the comments below:

Mayor comments:

1 - As mentioned in the results, the correlation between the measurements and the model simulation (maxima) is very low, R=0.15. The authors state: "These low performance indices are due to offsets in the maximum levels of the concentration spikes and their timing, suggesting that case-by-case analysis is required to determine the basin-scale transport and to evaluate the numerical simulations." Maybe this analysis can help to identify the problems of the model in the individual cases, but the result of the study should be the knowledge gained as to what is generally going wrong and the authors should show a way and at least the effort of improving the correlation. In the current stage, the model results and the measurements are shown separately (time series, maps), and no conclusion is taken from the comparison with the measurements (correlation plots and tables). The results stay qualitative and the conclusions driven from the case-to-case study remain therefore vague and somewhat speculative. Some

suggestions on how to improve the statistical evaluation could be the following: If timing is the problem, compare an average of maybe 3 hours or more. If errors propagate along the simulated trajectories, compare the averages of larger regions. If different biases are expected in the transportation for different meteorological patterns, analyze the correlations independently for the chosen episodes. If the PBL heights are not well reproduced in the model, compare them with the available measurements and filter the data using only the ones with good agreements. It would be beneficial if the authors could, based on these "natural experiments in basin dispersion", identify in general terms which the main weaknesses of the model are and how its performance could be improved to reproduce the observations.

2 - The authors compare maxima of the surface measurements with maxima of the simulation (Fig. 2), without explaining their motivation for this choice and defining exactly what the maxima are. In p. 16571 line 1, however, is stated that averages were used. Especially extreme values should be taken with caution since they could be caused or influenced by local events, which cannot be reproduced by the model. If the authors find the maxima are the quantity which gives the best correlation, then the model performance is really poor. Furthermore, neither the baseline values reported between 5 and 20 ppb nor its diurnal cycle would be well represented if only the maxima are considered.

3 – The evidence of the impact from the industrial emissions in Tula to the surface concentrations within the city is straightforward and well justified from the repeated cases analyzed. The dynamical processes which would explain the impact of the volcano, however, are not so clearly exposed and justified. Although it is possible to have such impacts, the evidence presented in the few cases is vague considering the following points: 1) it is shown that the confidence in the model reproducing the single-case episodes is poor; 2) the meteorological patterns within the basin can be very complex, as stated by the authors (sec. 1.2), and leave the possibility of contaminated air masses being recirculated within the basin and 3) other sources, such as biomass

burning, are not considered in the analysis (just mentioned in the conclusions). Moreover, the simulated impacts from the volcano seem to be influenced by the release height used. In p. 16576, line 9 is stated that the simulated volcanic plume changes when a higher release height is used. It is known that the effective emission can be considerable higher by hundreds of meters if the velocity and temperature of the gases are considered.

4 - The satellite measurements are more showed additively than integrated in the investigation and the correlation between the SO2 column of OMI and the simulation isn't reported at all. Only in p. 16577 line 1 is stated that the model sees lower total columns than the OMI product. It would be nice to see a qualitative comparison but taking into account the altitude-dependent sensitivity of the satellite by means of the averaging kernels or the air mass factor. It is not specified if the PBL column product used has the largest sensitivity near the surface (Tula source) or at the top of the PBL (volcanic source).

Specific comments

1. In the abstract should be reflected that from the results of this analysis, the volcano could impact the MCMA only on very limited occasions (10% during MILAGRO, as suggested by this study). In this direction, the title "Hit from both sides" does not seem representative from what was learned since only in one occasion during the campaign a simultaneous impact could be taking place, whereas the impact from the industrial source takes place repeatedly. I would therefore recommend omitting this part. The model results about the contribution of the three sources should be summarized in a table. The statistical distribution should be compared when the released emissions from the volcano are changed to meaningful values.

2. Assuming that SO2 is chemically inert is fine for the objectives sought in this study, but would lead to a systematic underestimation of the sources. It would be nice if this assumption could be tested by estimating by how much are the simulated concentra-

tions overestimated at the receptors at different distances from the source and as a function of elapsed time along the trajectory. If the chemical dilution is found to be relevant, the authors could include it in the model and maybe increase the correlation between model and measurements.

3. In p. 16575, line 10 is stated that the simulated concentrations are lower than the measurements by a factor of 2 or 3. This doesn't seem to be the case for all days and the reasons given are not convincing, thus the statement could be changed or removed.

4. The scales in the maps shown in Figs. 6, 9 and 13 are different between the measurements and simulations making it difficult to compare the distributions. The scales could be either matched or the domain of the measurements could be marked with a box within the simulation maps.

5. Section 4.3 on double impacts is too long and difficult for the reader to follow. Since the first 3 days described (14-16.3) correspond rather to industrial impacts, this part (and Figs 10-14) could be shortened and placed in section 4.1.

6. In the conclusions is stated that the relative impacts observed during MILAGRO could change significantly during the wet season. Is there any indication or evidence from the model in which direction this change could go with respect to the Tula and volcano?

Minor comments

Check spelling, for example:

p 16565, l 5 / analyzed

p 16565, l 25 / characterize

p 16574, l 13 / indices

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