

***Interactive comment on* “Evaluation of WRF mesoscale simulations and particle trajectory analysis for the MILAGRO field campaign” by B. de Foy et al.**

Anonymous Referee #1

Received and published: 22 February 2009

This paper presents an evaluation of the MM5 and WRF models and a Lagrangian particle dispersion model coupled to these mesoscale models, using data from the MILAGRO field campaign in Mexico City. The paper examines the skill of the models in simulating meteorological parameters as measured at the ground, by radiosondes and wind profilers. It also evaluates how well the dispersion model can simulate concentrations of carbon monoxide (in forward mode) and how well sources of carbon monoxide and sulfur dioxide can be identified using the measurement data and model simulations in backward mode. Clearly, a lot of detailed work went into this paper and it is good to see a detailed evaluation of the models. The work is all solid, as far as I can tell. Nevertheless, I cannot recommend publication of the paper in ACP as it is. I have to

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



admit that I had a hard time to read this paper and I almost did not make it to the end. The reason is simple: The paper is too long, too detailed, and it uses a lot of terminology that the MILAGRO community may be familiar with but that has no broader-scale use, which makes it difficult for anyone outside the MILAGRO community to read the paper. In that respect, the paper is more a report than a scientific paper. Still, I admit that the careful evaluation of the models is of value and the paper could be accepted after major revisions (length cut by AT LEAST 50% by removing unnecessary details, removal of MILAGRO "slang"). I have the following specific comments:

1) While the paper's aim is an evaluation of the models (as already the title suggests), section 7 suddenly discusses features of the transport of the Mexico city plume, like plume residence times, recirculation fractions, etc. This section does not seem to fit with the overall purpose of the paper (model evaluation) and I would suggest removing it entirely. Readers interested in the model evaluation would not expect this section to be part of this paper, and others who might be interested, will not read this paper.

2) Use of MILAGRO "slang": The paper stratifies the data in terms of various circulation patterns. However, the circulation patterns are not consistently used. In every section, different classifications are applied. For instance, in section 6 different clusters were formed for the various data sets (independently). The clusters are given names like NCool, Wcool, etc., for the radiosonde data, Drain1, Drain2, etc., for the surface winds, Northeast, H-Shear, etc., for the wind profiler data, and on top of that names like O3 North, Cold Surge, etc. are used throughout the paper to characterize the data. None of these clusters/groups is ever really explained in the paper. The stratification of data into different groups should normally help to understand the model performance in different meteorological situations. However, the average reader will be confused by all these names, making reading really difficult. At least I got very frustrated. It was simply too much for me! I suggest to use a single consistent classification of all data throughout the paper and explain this classification.

3) The philosophic discussion of model evaluation vs. model validation on page 2117

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

is rather useless. The same applies to the summary section.

4) On page 2117 and 2118, references are being made to various model validation studies. However, the choice of references seems rather arbitrarily and I cannot see a clear system in these various references. What do you want the reader to tell?

5) In section 5, it should be mentioned that forward and backward simulations are basically the same. Therefore, the evaluations based on forward and backward calculations are not truly independent.

6) Page 2129, paragraphs 2 and 3 are a repetition of what has been said in the previous paragraphs.

Minor:

Page 2119, l2: "plume of fuel oil": do you mean a combustion plume?

Figures 4 and 5 seem to be referenced before Figure 3.

Page 2126, l 1: "smaller, coarser simulations": do you mean simulations using a smaller domain and coarser resolution?

Figure 7: Scatter plots would probably be better than time series plot. It is very difficult to see how well the model simulates CO because of the large short-time variability in the time series.

Figure 8: indicate position of Tula power plant in plot.

Page 2132, line 1: "gives confidence in the wind transport in the model" this statement is based on wind directions just discussed. However, previously you have just shown that there are quite some disagreements in wind speeds. Does this statement still hold?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 2113, 2009.

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