Atmos. Chem. Phys. Discuss., 11, C7868–C7873, 2011
www.atmos-chem-phys-discuss.net/11/C7868/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "

Formation of semivolatile inorganic aerosols in the mexico city metropolitan area during the milagro campaign" *by* V. A. Karydis et al.

Anonymous Referee #1

Received and published: 17 August 2011

This paper discusses the performance of a model in simulating inorganic aerosols when compared to observations made during the 2006 MILAGRO campaign. The authors correctly point out that most aerosol modeling studies for that campaign have not focused on inorganic aerosols, but rather organics; therefore, this study provides useful information and makes a contribution to the literature. However, the motivation regarding the remaining challenges in modeling inorganic aerosols needs improvement and the authors do not take advantage of a large fraction of data from the campaign

that could be used to further evaluate the model. There are also numerous specific comments that need to be addressed and clarified.

Major Comments:

Motivation: In the introduction, the authors discuss past research on thermodynamic modules and how they have been used to simulate inorganic aerosols. On the one hand I agree that simulating nitrate can be challenging (sulfate should be more well known), but some of the examples they give suggest that performance may not be that bad. So I found that a clear articulation of the need of further research on modeling inorganic aerosols lacking. A confusing factor is that they list the many thermodynamic modules that are available, and it is not clear from this study how they are fundamentally different, especially when some studies show a general agreement among them (line 20, page 21999). Perhaps going into that detail is appropriate for a review paper and not this study, but setting the proper context of the research is needed. For example, how do the treatments from CAMx differ from the other models?

Evaluation: I did not find the plots that show horizontal variations in average concentrations of various aerosol species very insightful. The authors should elaborate as to what purpose they serve. The time-series comparisons with data were more useful than the averaged results. I am not sure why the authors have limited their analysis to only surface data. Extensive measurements of aerosol composition from several research aircraft are available from MILAGRO and I strongly suggest comparing the simulated results with those measurements as well. This will provide additional information on the processing of aerosols as aerosols and their precursors are transported away from Mexico City. An evaluation of simulated trace gases that affect inorganic aerosols also seems in order to establish whether the inorganic aerosol predictions are reasonable for the right reasons. All that is shown is the end result (inorganic aerosols), not the other components (meteorological, trace gases) that affect their evolution.

Specific Comments:

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Title: “mexico city” should be “Mexico City” and “milagro” should be “MILAGRO”.

Abstract: The performance of the model is defined in terms of average concentrations compared to observations, which is one of many metrics that could have been used. It is certainly important to report this information, but the abstract does not convey the importance of these numbers for regions other than Mexico City to make this paper more relevant. The focus of the paper is on inorganic aerosols, but some mention of organic aerosol needs to be made since it is often the largest component of aerosol mass in Mexico City. So in terms of human health issues and regulations, errors in predicted inorganic material may not be as significant as errors in predicted organic matter (except during dust events). I do not see this sort of context on inorganic aerosols described in the paper.

Page 21997, lines: 6 – 24: The paragraph starts by discussing anthropogenic emissions and then mentions dust, which could be considered either a natural source or man-made one (if human activity alters the landscape). So why is biomass burning not mentioned in this discussion? This seems to imply that biomass burning is not important in terms of aerosol loading. Other works suggest that modern carbon is a large fraction of total aerosol carbon.

Page 21998, lines 7-29: The paragraph is a summary and review of equilibrium thermodynamic models. Did the authors mean to omit dynamic (e.g. MOSAIC, Zaveri et al., 2008) approaches when simulating inorganic aerosols?

Page 2200: Since the authors mention some studies that employ WRF-Chem, there are at least two others that have not been described (Shrivastava et al. 2011; Fast et al., 2009).

Page 22001, line 9: “model domain is expanded . . .”. Expanded compared to what? Compared to a previous application of CAMx I assume, but the authors need to be specific.

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Page 22002, lines 23-25: I presume the boundary conditions are invariant with height? This could be a bad assumption since aerosol concentrations are often higher in the boundary layer than in the free atmosphere. This would mean that the model could be entraining the wrong amount of aerosols from the free atmosphere into the boundary layer every day as the convective boundary layer grows during the day. The constant values along each boundary will introduce discontinuities into the results near the corners. I would also expect that large-scale aerosol concentrations over a month long-period will change significantly. Please discuss the impact of these affects on the present model set up.

Page 22002, line 26: Is the 6 km above ground level or sea level? This makes a big difference for the Mexico City plateau which is $\sim$2 km MSL. Boundary layer heights during MILAGRO were often between 3-4 km AGL which would mean that the boundary layer could grow above the top of the model domain (if the model domain top is at 6 km MSL).

Page 22003, lines 1-2: Much more information on WRF is needed to assess the simulated inorganic aerosols in this study. Was WRF run at the same resolution as CAMx? What frequency was the meteorology used to drive CAMx? How did WRF perform and is this described in a previous paper? As stated in a few instance later, meteorological uncertainties can impact predictions of inorganic aerosols but the reader is given no information as to the performance of WRF in this study.

Page 22003, line 3: It would be useful to include in a table the emission totals used in this study from the inventory. Does the emission inventory extend over the entire modeling domain? The MCMA is a definition that applies only to the immediate vicinity of Mexico City.

Page 22003, line 12: It would be useful to include a plot that shows the distribution of a) anthropogenic sources and b) dust emission sources. This would help the reader understand the spatial distribution plots of aerosols shown later. A discussion of how

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dust emission sources were determined would be useful.

Page 22003, section 3: Are biomass-burning emissions included in the simulation? Presumably they are composed mostly of carbonaceous aerosols anyway, but trace gas emissions could affect inorganic aerosol evolution. What about volcanic emissions of sulfur dioxide that affects sulfate production? Are those included or not?

Page 22004, line 2: Texcoco Lake is not indicated on any figure. How are readers supposed to know where this is located? In many places in the manuscript, the authors seem to expect the reader to follow Mexico City literature.

Page 22005, line 12: Meteorological errors are mentioned. The authors should be more specific on this point given the lack of context of the WRF predictions in this paper. Are the errors reported elsewhere? A few lines later (line 18-19) suggest a transport error, but not evidence is given to support the statement regarding errors in simulated peaks of sulfate.

Section 4: The authors should include somewhere (perhaps a table) a list of instruments in which the measurements were used in this study. Also including the principal investigator of each instrument is needed, along with an estimate of the uncertainties in the measurements and citations to papers that describe the measurements (if possible). It is a bit disappointing to not acknowledge the hard work of many scientists on which this study benefits.

Page 22007, lines 9-10: I cannot verify this statement from the plot. Perhaps that is due to the poor choice of scaling in the plot.

Page 22007, line 14: AMS is not defined.

Page 22007, lines 21-29: In the discussion of the emissions in Section 3, the authors should describe how emissions of various dust species are determined.

Page 22008, section 6: The authors present one figure on the differences in nitrate between simulations that employ either the hybrid or equilibrium approach. They should

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also include the relative effect when compared to observations. Presumably the hybrid (or even a dynamic) approach would be better. Is this true?

Page 22009, section 7: The authors give net changes in PM species based on changes of emissions. It would be useful to include the relative change in total PM, since that is the real metric in terms of human health.

Page 22011: line 5: The "conclusions" read more like a summary to me.

Page 22012: lines 1-2: This statement is an obvious one, but unproven by this study. They authors have not shown this result at all.

Conclusion: This study is evaluating one model for one case, and it is not clear what that means in terms of applicability beyond MILAGRO. It would be useful to include some discussion along these lines in the conclusion.

Figure 2 (and other similar figures): Most readers unfamiliar with Mexico City will not understand the spatial scales in these plots. Please include a scale to indicate distance in kilometers. There are a number of other sites listed but it is not clear what there relevance is. This should be included in the figure caption. Including topography and/or a Mexico City boundary would also be very useful. The black text is often virtually impossible to read on top of the colors (especially dark read and blue).

Figures 4 and 5: There is a misspelling "preridcted". I have not look specifically for errors in spelling, but this suggests the authors should check the manuscript again.

Vertical axes on time series plots: There seems to be poor choice of the scales used on the vertical axes. In many cases most of the model results appear in a narrow range at the bottom of the plot; therefore, a reader cannot tell the differences between the observed and simulated values. The range of values on most of these pl

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 21995, 2011.