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Geoscientific Model Development Discussions

Interactive comment on "Evaluation of dust and trace metal estimates from the Community Multiscale Air Quality (CMAQ) model version 5.0" *by* K. W. Appel et al.

Anonymous Referee #2

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GENERAL COMMENTS

The Community Multiscale Air Quality (CMAQ) model has been recently updated to version 5.0, including several enhancements. Specifically, some improvements are devoted to dust cycle, focused on emission, transport and chemistry of dust. In the present paper authors show an evaluation of CMAQv5.0 which includes anthropogenic fugitive dust emissions (agricultural farming and construction) and the effects of a new parameterization to estimate wind-blown dust based on the full year 2006 over the continental US. Furthermore, CMAQv5.0 shows an adapted mechanism to considered chemical speciation of trace metal in the fine fraction of particulate matter. The evalua-

tion of dust is done in terms of soil and their individual trace metals over different environments: urban areas (mainly from the CNS network) and rural areas (mainly stations from the IMPROVE network). The results show that overall CMAQv5.0 overestimates observed soil, where bias is bigger in CSN stations than in IMPROVE stations. Authors attribute part of such overestimation to underestimation of night-time mixing in the urban area and uncertainties in spatial and temporal distribution of dust related emission sources in the emission inventory.

The paper is very well written, structured and supported with appropriate references, figures and tables. The present work could be of interest to the readers of GMD and good reference to the large CMAQ Community. I think it should be suitable to be published on GMD. However, I think the paper would be improved if some parts would be discussed and presented more in detail.

SPECIFIC COMMENTS

The points to be considered by the authors:

1. Abstract. Overall the abstract summaries the main ideas of the paper, but in some part is a little bit subjective at qualifying the model behaviour using words like "did well" or "good". I think it could be more forceful if authors quantify model performance using statistics. Here some examples:

P1860, line 12-14: could you replace the sentence "The CMAQ modeling system generally did well [. . .] the western United States;" by something more quantitative, e.g. indicating the annual mean bias or correlation coefficient in order to compare with eastern coast. The same thing should be considered in the summary section, e.g. P1881, lines 11-13.

2. Chemical boundary conditions. P1867, line 10-19: the way as the boundary condition is treat is not very clear explained in the manuscript. On the one hand, GEOS-Chem dust concentrations are speciated into CMAQ trace metals and lumped species

using a composite of four desert soil profiles from SPECIATE. My questions at this issue are: did you create a mean profile from the four ones? Which profiles are they? Which is the percentage of composition of each species?

On the other hand, it is not clear the chronological order that the authors applied to treat GEOS-Chem to feed CMAQ for PM boundary conditions. I guess that first, they grouped aerosols by size matching bins and corresponding CMAQ modes; and second, they speciated aerosols from the PM2.5 fraction (which came from mainly from the first GEOS-Chem bin) as AFEJ, AALJ, ASIJ, ATIJ, ACAJ, AMGJ, AKJ, and AMNJ, using the composite of four desert soil profiles.

From the text is not clear, please clarified how you performed that.

3. Soil variable definition and comparison with observations. Soil from CMAQ is post-processed using the Equation 1. However, this equation is not very well supported in the manuscript. Furthermore, the link the authors use as reference does not work. From Equation 1, one can guess that your only evaluate soil in fraction PM2.5. In this sense, which is the size of PM in the measurements? Do the authors think that the size between model and measurement is a source of uncertainty in the present evaluation?

4. Reasons for CMAQv5.0 soil overestimation. The results show that overall CMAQv5.0 overestimates observed soil, where bias is bigger in CSN stations than in IMPROVE stations. Authors attribute part of such overestimation to underestimation of nighttime mixing in the urban area and uncertainties in spatial and temporal distribution of dust related emission sources in the emission inventory. This conclusion is present in the abstract and in the conclusion. However, along the paper there is not enough evidences about that.

In this sense, have the authors quantified the model day-time and night-time cycles of PBL, wind direction and velocity? This evaluation could give an objective idea in what extend meteorology is affecting to under-/overestimation of concentration.

5. Effect on sulfate chemistry. P1875, line 23: author suggest that one reason that SO42- decrease between both CMAQ version is related to the fact that Fe(III) and Mn(II) concentrations in the CMAQv5.0 are often lower that the prescribed Fe and Mn used in previous version. In this sense, my question is: how lower are these Fe(III) and Mn(II)?

On the other hand, at P1875, line 25-28 the paragraph appears confusing. From this sentence is not clear if the new rate constant is pH dependent or not. Please clarify this issue.

6. Impact on soil concentrations from African dust event. CMAQv5.0 underestimates soil concentration in summer under some specific episodes. The authors suggest that these underestimations (the days 13 July, 28 July and 3 August) are related with mineral dust long-range transported from the Sahara desert. However, the performed analysis does not justify the present of this event.

Could the author confirm that the soil in these events come from Sahara desert or other part of Africa? At least, could the author confirm that air masses come from this continent? Maybe you can use trajectory analysis, global model or satellite data to support the evidence of a long-range transport of dust from African desert.

7. The number of available stations form IMPROVE network is 161 (P1870, line 24). However, the number of stations which appear in Figure 1 is 160. Are there any special reasons for that?

8. Figure 5 b shows the change in bias between the two CMAQ simulations at IMPROVE, CSN and CASTNET. Here, CASTNET appear for the first time. You should introduce the network properly in section 3 as you did for the other networks.

9. Figure 4. I suggest including the number of observed data available to have an idea of the temporal coverage of the measurements.

TECHNICAL CORRECTIONS

- P1863, line8: the references “Xiu and Pleim, 2007” and “Pleim and Xiu, 2003” are not in the References section.

- P1863, line15: the reference “Otte and Pleim, 2009” is not in the References section.

- P1863, line25: the link does not work.

- P1864, line17: replace “single emissions category” by “single emission category”

- P1864, line25: replace “PM2.4” by “PM2.5”

- P1865, line27: BEIS acronym is previously defined in Pg 1864, line2.

- P1867, line4: remove capital letter in “Chemical”.

- P1870, line20: the link does not work.

- P1874, line14: define MI as Michigan, I guess.

- P1877, line24: the reference “Fairlie et al. (2006)” is not in the References section.

- P1883, line 28: Davis and Swall (2006) is not referenced along the manuscript.

- P1885, line 14: Otte and Pleim (2010) is not referenced along the manuscript.

- P1885, line 32: Pleim and Xiu (1995) is not referenced along the manuscript.

- P1890, caption Fig. 1: the sentence “the IMPROVE network sites (blue) and CNS (red)” should be replace by “the IMPROVE network sites (red) and CNS (blue)”.

- P1892, caption Fig. 3: replace “RMSE” by “root mean squared error (RMSE)” as for the other statistics IA and r.

- P1892, caption Fig. 3: replace “correlation” by “correlation coefficient”.

- P1893, Fig. 4: Figure 4 left panel makes reference to Mn but in the caption you indicate it as K. I guess you should replace “K” by “Mn” in the caption since the plot and the discussion is based on Mn.

- P1893, Fig. 4: Could you indicate in both plots the number of observation for Mn and Ca?.

- P1893, Fig. 4: in the caption replace “MI” by “Michigan”.

- P1894, Fig. 5a: the colour chart indicating low positive bias is in green range and low negative bias is also in green range. Could you make this colour chart consistent with the one in Fig. 5b?

Interactive comment on Geosci. Model Dev. Discuss., 6, 1859, 2013.