

Interactive comment on “Incremental testing of the community multiscale air quality (CMAQ) modeling system version 4.7” by K. M. Foley et al.

Anonymous Referee #1

Received and published: 24 December 2009

TITLE OF PAPER: Incremental testing of the community multiscale air quality (CMAQ) modeling system version 4.7

AUTHOR(S): Foley et al

This paper deals with a comprehensive evaluation of a new CMAQ version, in this case v4.7. It has been the first time at all that such an effort has been done with a new CMAQ version. I am convinced that a lot of CMAQ users appreciate this work. The authors have put tremendous efforts into this paper focusing on five major scientific upgrades implemented in CMAQ v4.7. Overall, this paper is well written. However, some parts might still need some further explanation (for more details see below). One critical issue are two papers (Carlton et al; Kelly et al) which show up in table 1. These papers are still “in preparation”. However these papers are critical, since it seems

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these describe Increments B and C in more details. After addressing the issues lined out below I would be happy to recommend the publication of this manuscript.

Abstract: Page 1246, line 2: “This paper describes the scientific and structural updates to the latest release of the Community Multiscale Air Quality (CMAQ) modeling system version 4.7 (v4.7)”, but it seems that none of the photolysis options reported on pages 1257 and 1258 are available in this release. Not sure if they should be included in this paper.

Review of scientific and structural upgrades in CMAQ 4.7: - Meteorological input model: Page 1249, lines 19-26: I am assuming nudging and ACM2 were not available in WRF versions prior version 3.0. However, it seems they were used in other models. The authors should name them.

Page 1250, lines 18-21: Why the need to use WRF instead of MM5, when both models are comparable?

Page 1250, line 28 – page 1251, line 4: This sentence is long and not easy to read. It should be split into smaller pieces and provide more explanation.

Page 1252, line 26: “ the CMAQ input file OCEAN_1 has been enhanced” – is this file a part of CMAQ 4.7 release? In other words, are these enhancements available to a reader?

Page 1253, line 1.9: “accumulating precipitation” – what is the meaning of that? - Improvements in atmospheric chemistry: Page 1256, lines 4-6: “the relative impacts of this change were small” – impacts on what? Also, Sarwar et al. (2008) showed that a new HONO treatment improved significantly the model predictions of its mixing ratio. The reason for not performing this increment in this paper should rather be that it was already done in above mentioned paper.

Page 1256, lines 10-12: Has a current version of SMOKE capability of deriving Cl emissions?

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Page 1256, lines 13-18: The authors should describe more thoroughly the motivation for Hg modeling and the improvement of Hg modeling in CMAQ 4.7 compared to previous versions.

- Research options: Page 1257, lines 10-12: How are the “beta” versions provided to the community? It seems that they are not available with CMAQ 4.7.

Page 1257, line 15 – page 1258, line 2: It seems that the option that utilizes satellite-derived cloud information to adjust photolysis rates is not available yet (software problems are currently being addressed). If so, what is the point of reporting unfinished work here.

Page 1258, lines 26-29: “Bidirectional surface exchange option . . .” It seems that this option cannot be used, since even authors could not perform tests. If so, no need to report it in this paper.

Modelling approach and observational data sets: Page 1259, lines 17-19: Were initial conditions really derived from 36 km simulations? If so, how? A previous statement (line 14) suggest that “ a 3-day model spin-up was used”.

Page 1260, lines 1-3: It seems that to derive sesquiterpene fluxes the MEGAN model needs to be used. Previous information on page 1252, lines 1-3, suggests that BEIS itself would derive those emissions.

Evaluation of major scientific increments: - New parameterization for heterogeneous reaction probability: Page 1261, lines 12-16: This part should be moved to section 2.2 and repeated information should be deleted. The paper is already long.

Page 1261, line 23 – page 1262, line 6 and corresponding Figure 1: This discussions needs some more explanation. I understand that column 2 in Figure 1 refers to the corrected typographical error in the CMAQ aerosol model. It is not clear to me what the third and fourth column exactly displays, since my understanding is that Increment A also reflects the impact of corrected typographical error in the CMAQ aerosol model.

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Also it would be helpful to spell out the CMAQ versions in the headers of the plots.

Page 1262, line 13: What is the over prediction in the summer time in quantitative terms?

Page 1263, lines 1-4: It seems in the previous version biogenic SOA were also higher in summer than in winter. So in this respect there is no change using the updated version.

- SOA model enhancements: Page 1262, line 21 and corresponding Figures 2 and 3: It would be more helpful to spell what you exactly mean with “previous”. As for Figure 2 it would be helpful to spell out the CMAQ versions in the headers of the plots.

Page 1262, lines 24-25 and corresponding Figure 2: The decrease in anthropogenic SOA in August is not as large as in January. The authors should differentiate their statement.

Page 1263, line 1: What is the difference between the lower-middle and the lower-center plots in Fig 3?

Page 1263, lines 15-16: Where the experimental results in Duke Forest obtained during the entire month of August. Please clarify?

Page 1263, lines 18-19: Not sure, if you can use experimental data obtained on 17 days of August 2003 to validate the modeling results for the entire month of August in 2006.

Page 1263, lines 20-21: It sounds like there was a comprehensive update on the aromatic SOA formation treatment, potentially including aromatics like toluene and xylenes. I am not sure, if I have missed something, but section 2.2 the authors are basically referring to benzene. Please clarify.

Page 1263, lines 20-29: I guess the most important new anthropogenic SOA precursor which was implemented was benzene. The differences in anthropogenic SOA in

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summer in the new version is really tremendous. Is there a good explanation why anthropogenic SOA is also strongly enhanced over rural areas. Also, it looks like urban areas like Houston do not show though I would expect some benzene related SOA signal. Also the reference of Kleindienst et al is not really helpful, since it refers to “some polluted days” in 2003. So I would expect some bias to higher values. Anyway the Kleindienst et al values of $0.8 \text{ mikrogramC/m}^3$ are actually closer to the previous values, whereas in the new version values up to more than $20 \text{ mikrogramC/m}^3$ are found.

- Coarse-particle chemistry: Page 1264, line 22: I guess it should be “bottom row of Fig. 6”. Page 1264, lines 26-27: I guess it should be “top row of Fig. 6”.

- In-line photolysis research option: Page 1266, lines 2-12: This part should be moved to section 2.6 and repeated information should be deleted.

Page 1266, line 19: Higher values of what?

Page 1266, lines 24 – 25: The authors want to describe what the different O₃ column values in these models were. Also, the authors want to explain why O₃>O₁(D) photolysis rates at the surface should decrease due to different (supposedly lower) O₃ column values. Assuming lower stratospheric O₃ values (which make up most part of the O₃ column) would lead to more UV radiation at the surface. Wouldn't this lead to higher O₃>O₁(D) photolysis rates at the surface?

Page 1266, line 26 – page 1267, line 1: I agree that there was a stratospheric ozone depletion over the last 30 years, but I am not sure, if this may explain the fact that changes of O₃ photolysis depend on the elevation.

- Ozone: Page 1269, lines 14-16: According to Appel et al. (2009) cited here the change from MM5 to WRF has a significant impact on ozone. In contrast, on page 1250, lines 16-23, the authors say that WRF and MM5 meteorological fields are comparable and there is a comparable CMAQ performance using either MM5 or WRF me-

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teorology. Is it a different study than Appel et al. (2009)?

Page 1269, lines 25-26: Cannot understand the following sentence: "For higher-level observed O3 mixing ratios, O3 predictions tended to decrease."

Evaluation of CMAQ 4.7: - Wet deposition: Page 1270, lines 21- page 1271, line 3 and associated figure 12: It is difficult to decipher any consistent geographic distribution of the parameters. So I am not sure about the value of this figure.

Discussion: Page 1272, line 8: please clarify: is it Mathur et al., 2008a or 2008b?

References I was hoping that the status of these two papers could updated upon a potential acceptance of the manuscript Carlton et al, submitted 2009 Otte and Pleim, submitted 2009

Tables Table 1: It is a critical issue that these two papers are in preparation. Carlton et al, in preparation Kelly et al, in preparation It seems these are key papers. I want to leave it up to the editor how to handle this.

Figures

Some of the figures and the legends of figures are not easily readable because they are too small, i.e. figure 1,5,6,7,8,11, and 12.

Figure 3, figure caption: Where only sesquiterpene emission fluxes added to BEIS 3.14? In chapter 2.2 the authors state that SOA formation from other biogenic VOCs were included as well. Please clarify, if these were also included in the plots of the third column.

Figure 11: It should be noted somewhere (either in the plots or in the figure caption) what compound is shown.

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