**Geoscientific
Model Development
Discussions**

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Comment

# *Interactive comment on* "A multi-resolution assessment of the Community Multiscale Air Quality (CMAQ) Model v4.7 wet deposition estimates for 2002–2006" *by* K. W. Appel et al.

**Anonymous Referee #1**

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General comments :

The manuscript evaluates extensively how well the CMAQ model simulates wet deposition of SO4–, NH4+ and NO3-. It also describes some model improvements that lead to better simulations. The manuscript is about model evaluation and model development and the paper is therefore suitable for GMD. The methods are valid and lead to clear conclusions.

My main remarks concern the readability of the manuscript and the number of figures. The manuscript, especially chapter 3, contains a lot of data and results, both in the text and in the figures. I think that in some cases too much (detailed) information is given,

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which hinders the readability of the manuscript. This could be improved by removing some of the more detailed results (e.g., see specific comment #8) and by mentioning fewer actual values of NMB and RMSE in the text (these values are already given in the tables). Readability will also be improved by improving the interpunction, e.g. by splitting up the longest sentences and/or by adding comma's. Furthermore, in my opinion the number of figures could be reduced. For example, the text on p. 2329 could do without figs. 7 and 8. Also, could the authors comment on the necessity of all supplementary figures displaying results for all years? There are many similarities between the years and most of the supplementary figures are discussed only briefly. For most supplementary figures, average plots for the entire period 2002-2006 would suffice, because the separate spatial situations in each year (if any) are often not discussed in the text.

Specific comments :

1. The abstract could be improved by including more of the conclusions about the general performance of the model from the summary (like the last sentence of the abstract). Some of the more detailed results could be deleted from the abstract.

2. p. 2319, l. 13-16: Why are for the period 2002-2005 other meteorological boundary conditions used than for the year 2006?

3. p. 2319, l. 16/17: The 32-km North American Regional Reanalysis data; by what system or model are these computed?

4. p. 2320, l. 24/25: Please rephrase the words " created by taking . . .GEOS-Chem simulation ". It is not clear to me what median value is taken and what is meant with a "24-vertical layer".

5. p. 2321, l. 20: isn't 100% the maximum percentage of $SO_2$ that can be oxidized into $SO_4-$?

6. p. 2321, l. 23: similar as the previous point, but now for $NH_3$ and $NH_4+$.

7. p. 2324, l. 14-16: Can you give a brief description of the important spatial differences between observations and model results?

8. p. 2325, l. 8-10: " with the 12-km . . .of the 60 months." => this information is superfluous and already captured by line 7/8.

9. p. 2328, figure s5 (also applies to figs. s1, s2 and s7): it would be more informative to display the difference between modelled and observed values, instead of the modelled values.

10. p. 2331, l. 1-4: is the reduction of NOx emissions also present in the emission inventory that CMAQ uses?

11. p. 2331, l. 6-10: refer to figure s7 here? (s7 is not yet mentioned in the manuscript).

12. p. 2334, l. 1-7: will the lightning generating algorithm be included in the next release of CMAQ (like the NH3 flux mechanism)?

13. p. 2332-2334, summary: Could you also discuss what these results mean for the purpose of this work, mentioned in the introduction (to test whether CMAQ can provide a spatially complete estimate of deposition)? Could you also indicate, based on your results, to what extent the model can be used for this purpose? In relation this: does the model indeed mix, transport, transform and scavenge regional emissions well (see the introduction, p. 2318, lines 8/9)? Also: even when the model estimates wet deposition well, it is difficult to evaluate dry deposition. In view of this difficulty, how certain are the computations of total (wet+dry) deposition? Is it possible to determine whether the uncertainty in the modelled total deposition is smaller in regions with a large relative contribution of wet deposition to total deposition?

Technical corrections :

1. P. 2316, line 5/6: " Performance of the wet deposition species is determined ...". Please rephrase these words.

2. P. 2316, lines 10/11 and 11/12: the same information is given twice ("... slightly higher ...")

3. P. 2316, lines 15-21: "The CMAQ model ..... for the eastern US". This part can be shortened.

4. p. 2319, l. 10: CONU => CONUS

5. p. 2320: Is it necessary to mention the abbreviations (EGUs), (CEMS) and (CMU)? These are not further used in the manuscript.

6. p. 2321, l. 16: The abbreviation NADP was already explained on p.2318, l. 2.

7. p. 2324, l. 17-22: Please split this one long sentence.

8. p. 2328, l. 26: "... in the those regions ..."

9. p. 2329, l. 25: proven => provided?

10. p. 2330, l. 1-2: please rephrase.

11. p. 2330, l. 4: "from a monthly profile to hourly," => please rephrase.

12. p. 2331, l. 14/15: "One large source ... generated NO." => please rephrase.

13. p. 2331, l. 17: "...is high and ..." => "...is high, and ..."

14. p. 2333, l. 22: improve => improves

15. p. 2334, l. 2: is the high => is high

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Interactive comment on Geosci. Model Dev. Discuss., 3, 2315, 2010.