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Geoscientific
Model Development
Discussions

Interactive comment on "Sensitivity of the Community Multiscale Air Quality (CMAQ) Model v4.7 results for the eastern United States to MM5 and WRF meteorological drivers" *by* K. W. Appel et al.

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Comments to reviewer #1:

The authors would like to thank the reviewer for his/her thorough review of the manuscript. The comments and questions posed are insightful, and many of the comments have been or will be incorporated into the manuscript. In addition, the authors appreciate the detailed minor comments, many of which are corrections to editorial errors in the manuscript. We have made every reasonable effort to incorporate the re-

viewer's comments and suggestions into the manuscript. We again thank you for your time and effort reviewing this manuscript.

Major Comments:

Reviewer Comments:

1) Motivation: What is the objective of this study? (a) Is it to inform WRF developers of the performance issues? (b) Is it to tell the AQ community what to expect when WRF replaces MM5? (c) Is it to warn regulators of potential compliance issues when the transition from MM5 to WRF takes place?

Author Response: As the reviewer commented further, the paper comes closest to answering question (b), which is indeed the intent of the paper. This research was not intended to be a rigorous diagnostic evaluation of the differences in the meteorological model that results in significant differences in CMAQ model predictions. Instead, this research was intended to give users of CMAQ a sense of what differences in air quality predictions can be expected when transitioning from MM5 to WRF. The primary objective was to reassure users that the WRF model provides reasonable air quality predictions when coupled with the CMAQ model. The secondary objective was to attempt to explain the possible reasons for large differences between the MM5-CMAQ and WRF-CMAQ results. The analysis does provide some information to WRF developers as suggested in point (a). This analysis is not intended to directly provide information to regulators regarding the CMAQ-WRF performance as the duration of the simulations is too short to apply to regulatory applications. However, we believe it does give members of the regulatory community some sense of the performance differences that might expect when transitioning from MM5 to WRF.

2) Analysis:

Reviewer Comments: What is the underlying assumption of Equations (3) and (4)? That the bias/error has the same distribution as the observations? What if the distributions are skewed differently? In my opinion, the normalization should be carried on each individual observation.

Author Response: The assumption is that the distribution of the observed and modeled concentrations are not normally distributed, but are instead skewed toward lower concentrations. Metrics that use mean assume a normally distribution, which is not the case for species such as sulfate and nitrate. To account for this, the median is used instead of mean, which is not affected by the non-normal distribution of the data. In the case where the distribution is normal, the mean the median are the same. We prefer to normalize by the average concentration and not the individual concentration since the observed concentrations can often be close to zero, which would result in extremely large normalized biases and errors (and in the case of a zero observed concentration, a NAN would result). To avoid this, the normalization is performed using the median of the observed concentrations.

Reviewer Comments: Figures 3 and 5 show the monthly average differences. Give us a sense of the spatial distribution of monthly average concentrations. Where are the differences more significant relative to the concentrations?

Author Response: Figures have been added that show the modeled concentrations from the MM5-CMAQ simulation. This provides a sense of the spatial distribution of concentrations.

Reviewer Comment: From Table 2 and Equation 3, I found that the median hourly O3 is 23 ppb. A difference of 2 ppb in monthly average O3 means that the differences in hourly O3 may be significant. How is the diurnal distribution of the differences?

Author Response: A figure was added that shows the monthly average diurnal concentrations from the observations and each simulation.

Reviewer Comment: Same for PM2.5.

Author Response: Unfortunately, hourly PM2.5 data are not as reliable as hourly O3

data, and therefore are not included in this analysis.

3) Method:

Author Comment: “appear to be” on line 13 page 1095: How do you see? How do you identify the most important factors (line 18 page 1096)? For example, on line 13 page 1097, you mention the use of a tool (the sulfur tracking version of CMAQ). How about elsewhere? Did you confirm that OH concentrations are higher along the Gulf of Mexico (lines 27-28 page 1097)? Recently, I heard that there may be serious issues with CMAQ’s OH concentrations in the upper troposphere. Explain the method you used for identifying the reasons? Did you use process analysis? Did you run sensitivity tests? Do you have quantitative evidence that (what you call) “major factors” are really major factors?

Author Response: The process for investigating the causes of the differences seen between the two simulations was extensive. We began by investigating obvious differences such as precipitation, temperature and wind speed and direction. However, since both simulations utilized an objective analysis scheme, these parameters were relatively similar between the MM5 and WRF simulations. Further investigation pointed to differences in how the clouds were being handled in each simulation, since we saw the largest differences in both ozone and sulfate, which are strongly influenced by clouds (photolysis in the case of ozone and aqueous-phase production in the case of sulfate). The use of the sulfur tracking version of CMAQ confirmed large difference in the amount of aqueous and gas-phase sulfate production in the CMAQ simulations. OH was examined and is indeed higher along the Gulf of Mexico, where you would expect based on the increases in ozone and sulfate production in the WRF-CMAQ simulation. Through our understanding of the CMAQ system, we determined that the likely primary cause of these differences was due to differences in the cloud predicted in each simulation. Since further investigation of these issues is ongoing, future work will involve more quantitative measures such as sensitivity tests and possibly using process-analysis. While our investigation leads use to believe that differences in cloud predictions between MM5 and WRF are a major contributor the differences in CMAQ performance, it is possible that other major factors could be responsible as well.

4) Language:

Given that you probably did not use a method that allows you to quantitatively compare the impact of different factors, firm language such as “were caused by” (line 14 page 1100), “due to less dry deposition of HNO3” (line 21 page 1100), “primarily due to” (line 13 page 1101) is inappropriate. More cautious language such as “likely plays a role” (line 7-8 page 1096), “are likely related” (line 18 page 1100), “appears to be most directly related” (line 1 page 1101) is acceptable.

Author Response: Where applicable, the language in the text has been adjusted to account for the qualitative nature of the analysis, as suggested by the reviewer.

Specific Comments

1) Line 18 page 1083: Need a reference

Author Response: A reference to Gilliam and Pleim paper has been added. These schemes are discussed in that paper.

2) Reviewer Comment: Line 17 page 1085: it is not necessary to state that layer collapsing is not used.

Author Response: The statement was removed from the text.

3) Reviewer Comment: I believe the time-of-the-day reference (i.e. morning, afternoon, night) in meteorological model performance assessment of Section 3 are based on UTC. This is confusing because local time is 5-6 hours behind UTC.

Author Response: Yes, the diurnal metrics are based on UTC. Text was added to the manuscript to define in both EST and UTC what is meant by nighttime and daytime. Thank you for the excellent comment and clarification.

4) Reviewer Comment: Lines 17-18 pate 1090: it is not clear why you want to make WRF parameterization more like MM5's. This statement is probably unnecessary.

Author Response: The MM5 method is preferred over the WRF method for this parameterization. We'll attempt to clarify this point in the text.

5) Reviewer Comment: Line 3 page 1091: Is there any data to compare to predicted cloud fractions? Is it a good thing that WRF has greater cloud fraction?

Author Response: Excellent questions. Unfortunately, there are very limited observed cloud fraction data to use to compare against predicted cloud fractions. However, we are making efforts to obtain such data to compare against the predicted cloud fraction in order to determine which model predicts cloud fraction more accurately. Hopefully, future comparisons between MM5-CMAQ and WRF-CMAQ simulations will include such comparisons.

6) Reviewer Comment: Line 8 page 1092: Similarly, is there any dry deposition data to compare to? Is greater dry deposition a step in the right direction?

Author Response: As with cloud fraction, observed dry deposition data are very limited is temporal and spatial coverage. The CASTNET provides a dry deposition measurement, however it is not a directly measured value, but is instead derived using an equation. We have limited experience using these “measurements” and we would want to first test their accuracy as a measured dry deposition value before including them in a model performance assessment. Efforts are being made to test these data as to their usefulness in CMAQ model performance assessments of dry deposition.

7) Reviewer Comment: Lines 8-26 on page 1093: Is this discussion (how PM2.5 mass is calculated) necessary?

Author Response: We felt the section describing the how PM2.5 mass was calculated from the CMAQ outputs species needed to be included for the following reason. CMAQ does not directly output PM2.5 mass, but instead PM2.5 mass must be “derived” from the CMAQ output species. The method we use for deriving PM2.5 has not been published and therefore it is possible that slightly different methods for calculating PM2.5 could be used by different groups. By including this section, we explain how we calculated PM2.5 so that other groups can reproduce our results and we publish our method for calculating PM2.5 which can be used by other individuals or groups in their assessments of CMAQ PM2.5 performance.

8) Line 16 page 1094: Explain why this was "expected".

Author Response: The "as expected" was misplaced text and has been removed.

9) Line 3 page 1095: The correct reference is to Table 3. Same thing in line 1 and 8 of page 1097.

Author Response: The incorrect reference to table 2 was corrected. Thank you for discovering the errors.

10) Line 5 page 1095: You are mixing 1-hr O3 with 8-hr O3.

Author Response: The text was changed to clarify which values referred to 1-hr O3 and which values referred to 8-hr O3.

11) Line 24 page 1095: I suppose these times are local and not UTC (See minor comment #3 above).

Author Response: The times are indeed in LST and not UTC. This was made clear in the text when referencing time of day.

12) Line 1-4 page 1096: Were all these areas VOC limited?

Author Response: As indicated, concentrations of NOx also increased where large sources of NOx exist, so the increase in O3 is due increases in both VOC and NOx concentrations.

13) Section 4.2.3: Why is TNO3 so much higher than NO3 in WRF-CMAQ along the

Gulf Coast?

Author Response: During the summer, concentrations of HNO3 are much higher than concentration of NO3, hence the concentrations of TNO3 (NO3+HNO3) are much higher than just NO3 along the Gulf Coast. However, this would be true of most regions throughout the domain in August.

14) Lines 20-21 page 1098: By "the same factors that result in the higher O3" do you also mean the gas-phase? Is so, how? There is probably something more basic such as lower mixing height (along the Gulf Coast).

Author Response: The factors include increases in the oxidant reactants (OH, VOC, NOx) by increased photolysis due to less cloud cover in the WRF simulation. We initially examined the mixing heights to determine if there were systematically lower mixing heights in the WRF simulation versus the MM5 simulation. However, we did not find the mixing heights to be systematically lower in the WRF simulation along the Gulf Coast in August. There did appear to be some systematic difference in the development and evolution of the sea-breeze front along the Gulf Coast which may play a smaller role in the difference between the two simulations. The WRF sea-breeze front tended to be slightly weaker and narrower than the MM5 sea-breeze front. This difference may be investigated further when a longer duration comparison is made between MM5-CMAQ and WRF-CMAQ simulations in the future.

15) Line 2 page 1100 "requires further investigation": There are no large differences in precipitation, plus SO4 was much lower, but SO4 wet deposition is higher. How confident are you with this result?

Author Response: The Northeast was not an area where we saw a large decrease in ambient SO4 concentrations. So, it does not appear that the increase in sulfate wet deposition is due to greater washout of ambient SO4, especially since there were not large differences in precipitation in the Northeast. It's clear why sulfate wet deposition increased, but there may be several factors at play that make it difficult to explain exactly what is happening in that region. Hopefully further investigation will be shed some light on the exact cause of the differences.

16) Figure 1: The legends are only visible at 4x magnification.

Author Response: The size of the legends was increased.

17) Figure 3: The scales are hardly visible at 2x magnification. The caption should define the difference as WRF minus MM5.

Author Response: The size of the scales was increased. Text was added to the caption to make in clear that the results show WRF minus MM5 concentrations.

Interactive comment on Geosci. Model Dev. Discuss., 2, 1081, 2009.