Atmos. Chem. Phys. Discuss., 9, C7785–C7793, 2009
www.atmos-chem-phys-discuss.net/9/C7785/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Seasonal climate and air quality simulations for the northeastern US – Part 1: Model evaluation" *by* H. Mao et al.

H. Mao et al.

hmao@gust.sr.unh.edu

Received and published: 2 December 2009

Interactive comment on "Seasonal climate and air quality simulations for the northeastern US – Part 1: Model evaluation" by H. Mao et al.

Anonymous Referee 2

Received and published: 29 October 2009

General comments: This paper attempts to establish the credibility of the RCMS-CMAQ model system for assessment of the effects of future climate change on regional air quality, which is planned for a future Part 2 paper. While the air quality (AQ) evaluation is quite extensive the evaluation of the regional climate simulation is rather brief. The weather pattern typing analysis demonstrates the ability of the RCMS to reproduce

the synoptic scale flow patterns and the importance of this capability in producing reasonably accurate ozone simulation. The similarity between the RCMS and FNL is to be expected and is an important prerequisite for use of the RCMS for future climate studies. Such analyses, however, are not sufficient for evaluation of the RCMS for AQ modeling. In addition to the overall statistics on temperature, humidity, and winds, an evaluation of precipitation distribution, amounts, and frequency would be helpful. Average diurnal statistics of temperature, humidity, and winds would also help, particularly since the average temperature and wind biases are rather large. The mean absolute error or RMSE should also be reported. Evaluation of PBL heights would be very relevant, particularly since it is noted that the MRF scheme tends to predict a PBL that is too dry. It would be good to see how these results compare to previous modeling studies where the meteorology was run with frequent re-initialization and/or data assimilation. This would provide some context for the AQ evaluation in comparison to previous studies. A key question is: How much does the meteorology simulation degrade running as an RCM compared to meteorology models and what impact does this have on the AQ simulations?

The ozone modeling evaluation is quite comprehensive with measurements from surface sites, ships, ozonesondes, and aircraft. The results seem to be quite consistent with other modeling studies which support the assertion that the system may be useful for future regional climate and air quality studies. However, I recommend that this case be made with more direct comparison to previous studies. The summary does a good job reviewing the shortcomings of the AQ model that are deduced from the evaluation. However, there are no concluding remarks as to the model system's suitability for use in future climate change studies.

We agree with the referee's comment that this study is not as strong in evaluation of RCMS as that of air quality modeling. Provided the already extensive work on air quality model evaluation included in this manuscript, it is apparent that we had too ambitious a goal with this manuscript. A complete model evaluation for an air quality modeling system should encompass evaluation of the climate, emission and air quality models. We should have set out incremental goals to accomplish the task, and admittedly this manuscript constitutes one of the three components of a complete evaluation, the air quality modeling aspect. What Referee 2 suggested is exactly how one should approach in their evaluation of the regional climate modeling aspect, and we will keep these comments in mind for future work. Therefore, we will change the title of this work to "A Comprehensive Evaluation of Seasonal Simulations of Ozone in the Northeastern U.S. During Summers of 2001 – 2005", which was also Referee 1's suggestion. The content of the manuscript has been revised to reflect this change.

In the meantime, we would like to point out that detailed information on RCMS evaluation can be found in our published work (Chen et al., 2003, JGR; 2004, JClim; 2005, GRL). Also, here in the manuscript we did provide bias and rmse values in the paper (see page 6327). We agree that PBL height could be an important variable relevant to our current study; however, there are no direct measurements of PBL height from the campaigns we looked into. As to comparing our RCMS results to previous studies with frequent reinitialization and data assimilation applied, we think that, with frequent reinitialization, the modeled meteorology would be no longer a continuous process; instead, it would be subject to frequent re-correction through initialization. As to data assimilation, it could introduce artificial forcing to model results and force the simulation to be close to large scale forcing. Different researchers have different opinions about the approaches to use in regional climate modeling. The reviewer is absolutely right about the need of intercomparison of these different approaches. However, this needs concerted effort from the modeling community; it is beyond the scope of this particular study.

Specific comments Abstract line 10: "daily" is used twice in this sentence.

The first "daily" has been removed (it's Line 8 in the revised version).

Page 17854 line 1: This sentence should be re-worded.

It is not clear to us why this sentence needs to be reworded. All parameters mentioned in this statement will vary with climate changes.

Section 2.2: The 1999 NEI was used, but the 2002 and 2005 NEI would be more appropriate for this modeling period with projections in between. Also, were continuous emission monitor (CEM) data from EGUs used in this study? These kind of emission updates might produce better results.

The referee is absolutely right that use of the updated emission inventories might produce better results, and this sort of issues should be one of the aspects of a complete evaluation of an air quality modeling system. Also, no CEM data from EUGs were used in this study. We are fully aware of the update of the emission inventory. It is commonly known that multiple seasonal simulations of regional climate and air quality are very demanding in resources, and our situation here is such that the work had to be done over an extended time period. One of the consequences of this is that upgrades might have been compromised. Further, the effect of different versions of emission inventories and models on model output is unknown. This topic itself warrants independent systematic investigations.

Section 2.3: A more recent reference for CMAQ is: Byun, D. W. and K. L. Schere. Review of the Governing Equations, Computational Algorithms, and Other Components of the Models-3 Community Multiscale Air Quality (CMAQ) Modeling System. Applied Mechanics Reviews. American Society of Mechanical Engineers, Fairfield, NJ, 59(2):51-77, (2006).

This reference replaced the old one.

What version of CMAQ was used? Much of the modeling seems a bit dated. The RCMS is based on MM5 rather than WRF, the NEI is 1999, and the CMAQ is probably at least a few versions behind.

CMAQ 4.5 was used. During the previous years we were running 5 summer seasonal simulations each for 6 case scenarios of the present and future. To ascertain the consistency of the different scenarios for fair comparisons, we had to stick to the same version of model in spite of the release of new versions. In future independent projects, we will most definitely use upgraded models.

The referee must be well aware that it is a major task to develop a regional climate model from the standard version of MM5 with our own land surface model and idiosyncratic features in various aspects. There are not many groups in the community that have their own regional climate models well documented in literature. One of the co-authors Dr. Ming Chen developed this RCMS when she was working in Penn State. She has published a number of publications on our regional climate modeling system (Chen et al., 2003, JGR; 2004, JClim; 2005, GRL, etc.). She and her colleagues at the time chose MM5 instead of WRF mainly because, when they started their work, WRF was still undergoing its early stage of development. Later on, they compared WRF and MM5 simulations over a short test period (1-week), and did find that MM5 actually performed better than WRF. For this reason, they decided to keep using RCMS with MM5 as its atmospheric component. But it is a good suggestion to move to WRF now that WRF is maturing.

We readily acknowledged in our response to the referee's previous comment that we certainly would prefer to use updated emission inventories. As expressed early on, the effect of different versions of emission inventories and models on model output is unknown. This topic itself warrants independent systematic investigations.

The reference for CB-IV is Gery et al 1989 not Grey.

We apologize for the typo in the text. Correction was made.

Page 17857 line 7: This sentence does not make sense. FNL data is extracted from the RCMS?

The Line 7 the referee referred to must be the one on Page 17858. We did not mean

to say that FNL data is extracted from the RCMS. The statement is reworded for clarification: "For the evaluation of the circulation patterns we extracted the reanalyzed and modeled sea level pressure fields at 12 UTC from FNL and RCMS respectively". See Lines 12-13 on page 7.

Page 17857 line 22: Should spell out TDL

"TDL" is in the NCAR dataset title, which we directly used here: http://dss.ucar.edu/datasets/ds472.0/

TDL is the acronym of Task Description Language, which refers to how the dataset was formatted.

Page 17858 line 19: What is "Domain 3"?

It must be page 17859 the referee was referring to. "Domain 3" has been changed "the RCMS domain as illustrated in Figure 1". See Lines 5-6 on page 9 in the manuscript.

Page 17860 line 4: Statistics are given for "hourly O3 mixing ratios". Aren't these actually max daily 1-h O3 ?

The referee is correct that it is 1-hr O3 daily maximum mixing ratios. Correction was made. See Line 16 on page 10 in the manuscript.

Page 17860 line 10-11: The low slope of the correlation does not mean underprediction but rather overprediction of low values and underprediction of high values.

We agree with the referee. Changes were made and the sentence reads as "The slope value of this correlation was 0.37, resulting from overprediction of lowers values and underprediction of higher levels". See Lines 21-22 on page 10.

Page 17860 line 15: The statement about nighttime overestimated daily minimum is not relevant to this discussion since this is about max daily 1-h values only.

We apologize for our oversight here.

Line 4: "Hourly O3 mixing ratios" has been changed to "1-h O3 daily maxima".

Lines 15 – 17: The part starting with "particularly the nighttime" and ending with "Sec. 5.1" is removed.

Page 17861 line 4: Overpredictions are noted for AL but even greater overpredictions seem to be in GA.

Indeed one grid cell in GA showed overprediction, although we did not see in Figure 5 that it is "even greater" overpredictions in GA than AL. This point was added in the text now:

"Our model simulations captured the pattern and magnitude of the observed salient features in the spatial distribution of 1-hour O3 daily maxima with primary exceptions in Alabama (AL) and Georgia (GA) as well as along the coast of the Mid-Atlantic States extending to southern New England, where modeled O3 levels were higher than observations by 5 – 10 nmol/mol (Figure 5b)."

See Lines 14-18 on page 11.

Page 17861 line 8: Much reduced PBL heights over water is also a likely reason for higher ozone over water.

This point was added into the text. See line 20 on page 11.

Page 17862 line 9: "close" to what?

The 8-hour O3 daily maximum mixing ratios from model simulations showed the magnitude of under- and over-predictions "close" to the 1-hour O3 daily maximum data.

Page 17863 line 8: Is the 90th percentile value calculated for the observations only?

The 90th percentile value was calculated for both observations and model results for a fair comparison.

Page 17868 line 27: This discussion of the effects of the coarse grid resolution raises

the question: were the model results spatially interpolated to the observation site locations?

Yes.

Page 17869 line 19-21: Clearly, comparing a 36 km grid value to a point on top of a mountain is a problem. What are the elevations of the observation site and the grid cell? It may be a better to compare to a model value above ground that is equal to the difference between the obs elevation and the grid cell elevation.

The elevation of the observation site at the Mount Washington Observatory is 2 km, and that in the grid cell is 486 m. This is the reason why we stated one of the three possible causes for the observation-model discrepancy to be that "it may be the result of a mismatch in the model grid-averaged value and the observed value from a single point in the grid".

It is not clear to us what the referee was suggesting us to do in their last statement.

Page 17870 line 16: The elevated wind speed seems to be only for the model since the observations at Duke Forest never exceed 4 m/s.

Based on our multiple year surface wind speed record at a coastal site, Thompson Farm in Durham, NH, 70

Page 17872 lines 21-29: The contradiction between Yu et al (2007) and Tarasick et al (2007) is simply because of the very different magnitudes of the upper LBCs. Whether the LBCs cause over or underestimation of ozone in the upper layers depends largely on the magnitude of the LBCs.

We agree with the referee on this. We would be happy to incorporate this argument into our paper should the referee provide a citation.

Page 17873 lines 11-15: All Fig 19 references should be Fig 18.

Corrections were made.

Page 17873 lines 16: What does "model time" mean?

It should be just "model".

Page 17873 lines 17: "prescribed top boundary condition" should probably be "top lateral boundary conditions".

Change was made as suggested. See line 21 on page 25.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 17851, 2009.