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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.

Anonymous Referee #2

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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

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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.

Specific comments

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

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

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.

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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).

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.

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

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

Page 17857 line 22: Should spell out TDL

Page 17858 line 19: What is “Domain 3”?

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

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.

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.

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

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

Page 17862 line 9: “close” to what?

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

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?

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.

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.

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.

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

Page 17873 lines 16: What does “model time” mean?

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

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

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