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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.

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

Anonymous Referee 1

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General comment

This study focuses on evaluation of regional climate and air quality simulations for summers of 20001-2005. In particular, the evaluation of summer air quality simulations is rigorous, where comprehensive observational data are used as the reference to iden-

tify model biases and possible explanations. These data include measurements from conventional monitoring sites and intensive field campaigns for both surface ozone and vertical profiles. This evaluation is a prerequisite for subsequent applications of the modeling system. As such, the topic of the manuscript is important and suitable for publication in ACP. The following are my suggestions that may help the authors to revise the manuscript for a more concise presentation.

Specific comments

While the manuscript is strong for evaluation of ozone modeling, it is relatively weak on that for climate. As driven by NCEP observational reanalysis, the regional climate model (RCM) is required to well reproduce the large-scale circulation patterns. Hence, the "reasonable agreement" between the modeled and observed circulation patterns is fully expected. Should the authors wish to address the RCM downscaling ability, they could emphasize more on whether the RCM downscaled regional climate quantities like precipitation, surface air temperature and mesoscale meteorology are more realistic than the driving reanalysis. Since this aspect has not been addressed and to my opinion is not critical to the main focus of the manuscript, I suggest that the evaluation on "climate" be de-emphasized. In particular, the title may better be changed to something like "Evaluation of summer ozone simulations for the northeastern US". By the same argument, the abstract and summary shall be revised accordingly.

We appreciate Referee 1's insightful suggestion, and agree wholeheartedly that their suggested title is more pertinent considering the overall content of our work. We will change the title of the manuscript to "A Comprehensive Evaluation of Seasonal Simulations of Ozone in the Northeastern U.S. During Summers of 2001 – 2005". The content of the manuscript has been revised to reflect this change.

Page 17855 line 5 "A common problem in model simulated O3 levels has been underestimation of high O3 values". This statement may not be a general one, as Huang et al. (JAMC, 2007) has shown that the summer ozone peaks (especially for the north-

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eastern U.S.) are realistically simulated. Thus the underestimation is model dependent.

This statement has been revised to reflect exceptions such as the work of Huang et al. (2007). See lines 9-10 on page 4 in the revised manuscript.

Page 17856 line 13 "constrained with" may better be "driven by"

Change has been made as suggested. See line 19 on page 5.

Page 17859 line 6 "1449 sites" - that's a lot. Do you really mean that (different locations)?

It is indeed 1449 sites in total, all different locations.

Page 17859 lines 9-11, regarding CCM2 problem, you may refer to Liang et al. (JCL 2004). They found that the CCM2 radiation package produces a deficit of up to 80 Wm-2 in solar radiation reaching the surface as compared with station measurements in Illinois. They also provided a solution to correct this problem.

This reference is included in the manuscript now.

Page 17860. Since the RCM run is driven by the observational reanalysis, the large-scale weather patterns are required to resemble each other between RCM and OBS. As such not only frequency of the pattern is close, but also temporal correlation or correspondence must be high. The agreement between RCM and OBS only implies that the nudging (via dynamic relaxation) of the lateral boundary conditions from the reanalysis is effectively done. See also comment [1].

The referee's point is well taken. We understand that the large-scale circulation pattern should be preserved in the model results via data assimilation. However, we did not do any nudging in this study. The dynamic relaxation passes large scale forcing into the RCM domain correctly through the boundary conditions. All processes within RCM domain are controlled by its own dynamics and physics and as a result independent of large scale fields outside RCM domain. Hence difference in circulation between

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the model and reanalysis data is inevitable due to the model's inherent physics. This difference can affect the simulation of high ozone episodes, in particular physics in the PBL which cannot be captured by the large-scale reanalysis data.

As much as the map type analysis revealed agreement between the modeled and reanalyzed fields, there is discrepancy in detailed structure of circulation patterns. A case in point is the Duke Forest Campaign period of 12-28 September 2004 where the model overpredicted O3 levels during the first 5 days by up to a factor of 5 (September 14) when the observed daily maxima were <30 nmol/mol. In contrast, it captured magnitude and timing of the daily maxima very nicely with nearly zero model and observation difference on 21-24 September when O3 levels were the highest (Figure 13a). A closer examination showed that the model was far off in representing wind speed over the entire period except the few days, 22-24 September, with relative higher O3 mixing ratios and calm wind speed (See pages 21-22). The primary reason for the overprediction was likely the mismatch of the location of the major low pressure system dominating the area. Therefore, our opinion is that the map typing analysis may err on the side of pointing out the obvious in terms of large-scale circulation patterns, but it is one way to examine whether smaller-scale details driven by model's inherent physics would be important enough to alter the general features of large-scale processes.

Moreover, we would like to point out that the comparison of map types from reanalysis data and model simulation was used further for the comparison of distributions of O3 daily maxima corresponding to the map types, which is one way to support the argument that capturing the transport patterns is critical to reasonable simulations of the O3 distribution. If we did stop at comparing the circulation patterns, this work might risk being perceived as rather superficial.

Page 17861 lines 3-19. The caption of Table 2 indicates that the result shown is for daily 1-hr max ozone, while the text in line 4 is confusing. This confusion continues in lines 11-15. The "mean bias" refers to daily 1-hr max in line 5, but implies daily mean in the subsequent lines. Otherwise the statement ". . .the nighttime overestimated daily

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minima and daytime underestimated peaks" would not be reasonable explanation.

We apologize for the 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.

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