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Interactive comment on "Twelve-month, 12 km resolution North American WRF-Chem v3.4 air quality simulation: performance evaluation" *by* C. W. Tessum et al.

Anonymous Referee #1

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Manuscript Summary: This manuscript describes model performance results for a 12 month WRF-Chem air quality simulation for North American (continental United States primarily) for the year 2005. The authors compare ozone, PM2.5 and speciated PM2.5 from the WRF-Chem model to routine ground observations from U.S. air quality networks, as well as comparing meteorological variables. The authors also attempt to compare the performance results from the current WRF-Chem simulation to other published model evaluation results with similar scope (both temporally and spatially).

General Comments: Overall I found the manuscript to be generally well written and organized. As is typically the case when attempting to evaluate model simulations that span large spatial domains and time periods, the difficulty becomes in summarizing

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the results in a meaningful way that does not overwhelm the reader with statistics and numbers. Here, the authors present annual mean performance metrics for the entire domain, along with regional/seasonal statistics. I generally don't find the annual statistics to be helpful in any way, other than perhaps initially to make sure there isn't some huge gross error in the model results. Otherwise, bulk annual/domain-wide statistics are typically difficult to interpret due to often compensating seasonal biases (e.g. particulate nitrate is often underestimated in the summer and overestimated in the winter). To their credit, the authors do acknowledge this issue with the bulk statistics. I'm wondering if the manuscript would benefit from dropping the annual domain-wide statistics and just focus on presenting the seasonal and regional statistics. I will leave this decision to the authors, but just note that I think most readers would find a much value in the annual/domain-wide stats and would immediately focus on the seasonal/regional stats. It might be nice to move some of the seasonal/regional plots for the speciated PM2.5 components from the supplemental material to the main text. Finally, the authors need to support some of their statements with references, specifically regarding difference in sampling protocols and/or analysis techniques between the different networks.

Specific Comments:

Abstract: Perhaps mention the modeling year earlier in the abstract.

Provide some examples of "contemporary models".

Again, bulk annual average statistics are not all the useful. Maybe replace these with more meaningful seasonal/regional metrics.

It's a little strange to look at 24h average ozone, given the large biases that typically can occur with ozone overnight. It might be better to present a different, more meaningful metric for ozone here (e.g. daily 8hr average maximum).

Page 8435, lines 13-15: It might be a little disingenuous to refer to 12-km as "fine-scale". Understanding that scale is relative thing (15 years ago, 12-km was "fine-

scale"), 12-km is probably better referred to as regional-scale at this point in time, considering that more and more modeling is taking place at 4-km and below.

Page 8436, line 21: 28 layers seems like it's on the low-end of layer structures these days. Were the computer limitations the deciding factor in going with 28 layers instead of something closer to say 40 or even 50? Do the authors feel that increasing the number of vertical layers (and in particular using the smaller first layer) would significantly impact the results?

Page 8437, line 13: What exactly constitutes "miscellaneous PM2.5"?

Page 8438, lines 7-9: The 2008 NEI has been available for quite some time now (and 2011 NEI is now available too). It seems like 2005 is a fairly old year to simulate at this point. When the authors say that the 2005 NEI was most recent available it makes it seem like this work started a long time ago. Has it just taken that long from start to finish for this modeling exercise?

Page 8439, Line 23: A 50-60 meter first layer height seems quite large, especially since nighttime boundary layers can often reach 50m or below. What impact do the authors feel there is from having such a deep first layer?

Page 8445, Lines 9-10: Exactly what differences are there between the network measurement techniques and why would they result in such larger differences? IMPROVE sites are rural, so perhaps background SO2/SO4 is greatly overestimated.

Page 8445: First, the authors state a MFB = -110%. What is does that statistic represent, since later in the paragraph the authors state a contiguous US MFB = -120%? The nitrate biases reported are really large. Do the authors have any explanation as to why nitrate is underpredicted by so much (especially in the west where nitrate makes up a greater percentage of the total PM2.5 than in the east)?

Page 8446: The OC underestimation at CSN sites is really large too. How is it that the differences don't appear to be rural vs. urban, since the urban CSN sites have an OC

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MFB = -113%, but the IMPROVE sites have an MFB = 15%)? That seems indicative of an urban emissions problem (or possibly meteorological, or both). I'd really like to know how those large differences are the result of simply sampling or analysis. References are needed if the authors are going to make statements like that.

Page 8446, Line 26: Change "lower" to "worse".

Table A2: Are these annual values being reported?

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