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Geoscientific Model Development Discussions
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# *Interactive comment on* "Application of WRF/Chem version 3.4.1 over North America under the AQMEII Phase 2: evaluation of 2010 application and responses of air quality and meteorology–chemistry interactions to changes in emissions and meteorology from 2006 to 2010" *by* K. Yahya et al.

**K. Yahya et al.**

yzhang9@ncsu.edu

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Reply to Comments from Reviewer 1

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Interactive comment on “Application of WRF/Chem version 3.4.1 over North America under the AQMEII Phase 2: evaluation of 2010 application and responses of air quality and meteorology–chemistry interactions to changes in emissions and meteorology from 2006 to 2010” by K. Yahya et al.

Anonymous Referee #1 Received and published: 12 March 2015

A comprehensive model evaluation study on the WRF/Chem performance for simulating meteorology and air quality over two years with two different configurations (offline and online), respectively, is presented. Effort has been made in putting all the analysis together and trying to make meaningful presentations of the data. It is very challenging to perform mechanistic evaluation of air quality models over different years with so many uncertainties in meteorology, emissions, and ICONs/BCONS. It often entails more advanced skills and techniques to draw credible conclusions about a model's responses to some specific changes over the years by eliminating or reducing interference from other uncertain factors.

Reply:

We thank the reviewer for careful review of this manuscript and valuable comments to improve the quality of manuscript. In particular, the reviewer recognized the challenge in performing the mechanistic evaluation for long period simulations and for years having different meteorology, emissions, and ICONs/BCONS, especially for the online-coupled model used in this study.

We have carefully addressed all the comments raised by the reviewer to improve the technical and presentation quality of our paper. Please see below our point-by-point replies.

However, the authors are trying to achieve the goal by simply comparing the model



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results with observations using the simple statistics (Corr, NMB and NME) and some plots. As the authors pointed out that the main objectives of the Part II paper are to examine whether the model has the ability to consistently reproduce observations for two separate years, as well as to examine whether the trends in air quality and meteorology-chemistry interactions are consistent for both years. But after reading the manuscript from the beginning to the end, the answers to the above questions are not there.

In order to achieve our goals, we first compared the model results with observations in 2010 (see Table 1) (similar evaluations for 2006 were performed by Yahya et al. (2014), see Table 1 in Yahya et al. (2014)). The evaluation we performed is very comprehensive and includes all major meteorological, chemical, radiation, and cloud related variables using various available surface network and satellite datasets. We have calculated full sets of statistics (> 16 statistical measures), although for the sake of brevity, our discussions on the statistics only focused on a few of them in this paper. We also evaluate agreement of predictions with observations on various temporal resolutions (i.e., diurnal, seasonal, and annual) and spatial correlations. Such a comprehensive evaluation can assess the model's ability to consistently reproduce observations for two separate years. The examination of model ability to consistently reproduce observations for two separate years has been discussed in Section 3.5.

Following a comprehensive evaluation, we then calculated the percentage changes in observed and simulated meteorological and chemical variables between 2010 and 2006 (see Table 2) to assess whether the trends in air quality and meteorology-chemistry interactions are consistent for both years. The trends in air quality and meteorology-chemistry interactions for both 2006 and 2010 are further discussed in Sections 4.1-4.3. Based on collective analyses of all those evaluations and trend analyses (instead of just the simple performance statistics), we found that the model is able to reproduce the observations to a large extent for most meteorological surface variables except for precipitation. The model has significant biases in a few aerosol



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and cloud variables well, such as for AOD, COT and CCN, however, it is able to reproduce the trends in the aerosol-cloud-radiation variables for 2006 and 2010. The model performs better for O3 mixing ratios and PM2.5 concentrations for 2006 compared to 2010 due to more realistic chemical initial and boundary conditions ICONs/BCONs and emissions. For 2010, Im et al. (2014a) found that the MACC model underpredicts surface ozone levels over North America by 22%. Im et al. (2014b) also showed that most models that used the MACC boundary conditions underpredicted PM2.5 concentrations for 2010.

In addition, we conducted several sets of sensitivity simulations as described in Section 4.4 (also see a new table (Table 3), for the simulation setup) to examine the model's responses to specific changes such as meteorology or emissions or chemical ICONs/BCONs only and to estimate the relative impacts of changes in meteorology, emissions, and chemical ICONs/BCONs.

With a comprehensive model evaluation, trend analyses, and additional sensitivity simulations, we believe that we have achieved our objectives. To address the reviewer's comments, we have revised the manuscript thoroughly to include more in-depth analyses and better relate the findings of this work to the main objectives of the paper. In addition, we added a new table (Table 4) to evaluate if the sensitivity simulations with different meteorology, emissions, and chemical ICONs/BCONs for Jan. and July 2010 can improve the model's capability in reproducing the trends in both meteorological and chemical variables, as comparing to baseline results in 2006 and 2010.

The sections that have been revised include: (i) Section 3.1 to explicitly state the similar trends in terms of meteorological performances from 2006 and 2010 as well as additional explanations for several biases in meteorological performance; (ii) Section 3.2 stating that the chemical performance between 2006 and 2010 is more variable compared to the meteorological performance of surface variables; (iii) Section 3.4 stating that the model is able to reproduce generally similar performances against observations for most of the aerosol-cloud variables for both 2006 and 2010; (iv) Section

3.5 stating that overall, the model is able to predict the trends in all the listed meteorological, chemical and aerosol-cloud-radiation variables between 2006 and 2010 with the exception of WS10 against CASTNET, Precip, CF, maximum 8-hr O3 against CASTNET and 24-hr EC against IMPROVE as well as additional analysis to explain the reasons (v) Section 4.4 in which we added discussions on the model's capability in reproducing trends between Jan./Jul. 2010 and 2006 with incremental changes in meteorology, emissions, and chemical ICs/BCs.

Throughout the manuscript, the authors were talking about statistics superficially without in-depth analysis about what caused the agreement/disagreement.

Reply:

The manuscript has been thoroughly revised to include more detailed analyses on model evaluation and likely causes for discrepancies. For example, we added that the cold bias in T2 is attributed to the lack of soil data assimilation in this study in Section 3.1.

When pairing cell-averaged model predictions with point measurement data in space and time (incommensurability), how much confidence do you have in terms of the good/bad performance of a model for different years with a few percentage differences in NME? I don't oppose using the statistics to perform model evaluations, but it seems too much for me if the analysis is heavily dependent on these numbers and the conclusions were drawn based mainly on these numbers.

Reply:

The U.S. EPA has provided benchmarks (US EPA, 2007) in model evaluation in terms of statistics such as the mean normalized gross error (MNGE) and mean normalized bias (MNB). The performance criteria used in this study follow Zhang et al. (2006), which include model bias (e.g., NMB) and error (e.g., NME) for good or poor model performance. For example, an NMB of $\leq$ 15% and an NME of $\leq$ 30% indicate satisfactory performances for O3 and PM2.5. We fully agree with the reviewer that the assessment of the model performance should not be simply based on performance statistics. As mentioned before, our conclusions for model performance are based on not only statistical evaluation but also other evaluations temporal (e.g., annual, seasonal, diurnal average) and spatial analysis, as well as several sensitivity studies during Jan and Jul.

Comparing the diurnal variations (Figure 4) using the whole year and all site data doesn't make sense to me. Considering all the averaging effect through space and time, to relate temperature with O3 concentrations in this context is very weak.

Reply:

Figure 4 was actually averaged over only the summer period of June to August (O3 season) at monitoring sites from CASTNET. CASTNET consists primarily rural and remote sites, we think that averaging predictions at the CASTNET sites is technically sound (note that the performance statistics is also calculated separately at sites from each network); their comparisons with averaged observations over the same dataset can provide an assessment of the sources of model biases at the CASTNET sites. In addition, such an evaluation can shed light on whether the underprediction of O3 mixing ratios is a systematic bias, i.e., day and night, or if it was just a portion of the day. To address the reviewer's comments, we have revised the paper to explain why we averaged model predictions and observations at those sites and the purpose of such diurnal assessments.

In Section 4, I expected to see some in-depth analysis about the model's response to the changes in emissions and meteorology and this should be the central point the authors are trying to make in this manuscript. But after I read the entire section, I was disappointed, because it simply listed the increase or decrease of the species from one year to another with very basic speculations (and some of them are known facts) and the connection between model response and input changes simply wasn't made. The model's response should be reflected (for example) under the percentage changes in



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emissions, under the similar weather conditions, does the model respond to the same percentage changes in pollutant levels as it was revealed in the observations.

Reply:

Section 4 has been extensively revised to include more in-depth analyses on the sensitivity simulations, which include the effect on the model response when using different sets of emissions, meteorology and chemical initial and boundary conditions.

To address the reviewer's last comment, we calculated the changes in the simulated meteorological and chemical variables due to changes in meteorology and emissions individual and collectively in Jan and July and compared them to the observed changes in the trends in those variables in a new table (Table 4). We also calculated percentage changes in emissions between 2010 and 2006 and added this info in a new table (Table S1 in the supplementary material). Relevant discussions along with percentage changes in emissions and meteorology have been added in Section 4.

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