

Interactive comment on “Application of WRF/Chem-MADRID and WRF/Polyphemus in Europe – Part 1: Model description and evaluation of meteorological predictions” by Y. Zhang et al.

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Reply to Interactive comment on “Application of WRF/Chem-MADRID and WRF/Polyphemus in Europe – Part 1: Model description and evaluation of meteorological predictions” by Y. Zhang et al. Anonymous Referee #1 Received and published: 23 March 2013

In the two manuscripts, Zhang et al. compare two air quality models, an offline-coupled model and an online-coupled model to assess their capabilities and differences in simulating air pollutants and aerosol-meteorology interactions over Europe. In the first

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part, the model settings are introduced and the meteorological performance against various observations is presented.

Generally, the purposes of the study are interesting, the methods are valid, and the results are reasonable. However, I have several concerns to the manuscript. I do not recommend the publication of this manuscript in its current version. Major revisions are needed to improve the manuscript. Reply: We thank the reviewer for valuable comments. We've thoroughly revised our paper and addressed nearly all review comments into the revised manuscript. Please see our point-by-point replies below. Major comments:

1) In this study, the authors try to compare an offline-couple model and an online-coupled model. However, the meteorological evaluation only focuses on WRF results which are only used for the offline model. Due to the feedbacks of air pollutants and complex interactions between meteorological variables with pollutants, the online-coupled model should have different meteorological predictions. The differences between meteorology predicted by offline and online models are what I expected to see in the manuscript. In fact, the authors only briefly discuss the difference in T2 and Precipitation between offline and online models in Page 17 Lines 10-20. If the meteorological differences due to aerosol feedbacks are only “decrease near surface temperature and precipitation”, I highly doubt about the merits of this study. It is only an evaluation of WRF outputs, and does not worth it to be published separately on ACP. Furthermore, without assessing the differences between the online and offline meteorology carefully, the comparison of offline model and online model in Part 2 tends to be less interesting: the differences in air pollutant concentrations are due to the air quality models unnecessarily they are offline or online.

Reply:

The feedbacks from chemistry affect all meteorological variables. We mentioned the effects on T2 and precipitation because those effects are most noticeable among the

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meteorological variables that we evaluated at monitoring sites, and the domainwide performance statistics for major meteorological variables at the observational sites predicted by WRF/Chem-MADRID are overall similar to that of WRF. The feedbacks from aerosols to meteorological variables were indeed shown in Figure 13 in our original Part II paper.

To address the reviewer's comments, we moved Figure 13 and relevant discussions from Part II to Part I to show the impacts of aerosols on several major meteorological variables, see Figure 5 in the revised Part I paper. This was also the suggestion made by the reviewer 2 to show the feedbacks of chemistry to meteorology.

2) Why mean bias (MB), the root mean squared error (RMSE), the normalized mean bias (NMB), the normalized mean error (NME), and correlation coefficients are used in this study? Emery et al. (2001) have proposed different statistics merits for different meteorological variables together with benchmarks. Using different statistics merits avoid a possible comparison with the suggested benchmarks and cannot give readers how good the performance is.

Reply:

MB and RMSE are common statistical measures for meteorological evaluation. NMB and NME are common measures for air quality models, but have also been used for meteorological evaluation in the literature. The inclusion of those statistical measures would allow an intercomparison of model performance across various studies. Those statistical measures provide a good indication of the overall model performance.

The statistical measures suggested by Emery et al. (2001) include Mean Observed Mean Predicted, Bias, Gross Error, RMSE, RMSES, RMSEU, and IOA for Wind Speed, Temperature, and Humidity and Mean Observed Mean Predicted, Bias, and Gross Error for Wind Direction. Our statistical table included Mean Observed Mean Predicted, and RMSE for all those four variables and our text discussions also include MB (which is the same as Bias in Emery et al., 2001). While RMSES and RMSEU are useful

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measures to understand systematic and unsystematic RMSE, they are not commonly used, we believe that RMSE is sufficient to illustrate the model performance, given the scope and objectives of our papers.

To address the comments, we added MB, Mean Gross Error (MGE) and IOA in Table 4. We also added a one-paragraph discussion to compare WRF performances with the benchmarks suggested by Emery et al. (2001) and Tesche et al. (2002).

3) In addition to the variables evaluated in this study, there are other important meteorological variables having substantial effects to air quality simulations such as solar radiation, PBL height, or the vertical distributions of temperature, humidity, and wind fields. How about the model performance on these meteorological variables?

Reply:

We agree that the meteorological variables included in our evaluation were only a subset of variables that affect air quality predictions. Those are, however, the most commonly used variables included in meteorological evaluation.

To address the comments, we looked into additional meteorological observations for January and July 2001 over Europe. There were no observational data for PBL height. BSRN contains downward shortwave and longwave radiation fluxes (SWDOWN and LWDOWN) at a few sites over the simulation domains, D01 and D02, but no sites are located in D03. NOAA-CDC contains satellite-derived outgoing longwave radiation (OLR) data with sophisticated spatial and temporal interpolation for all grid cells. We've now included statistical evaluation for SWDOWN and LWDOWN against BSRN data and for OLR against NOAA-CDC data in Table 4. NCAR DS353.4 ADP contains the vertical profiles of temperature, dew point, and wind vector (speed and direction) at midnight and noontime each day at 151 sites in D01, 20 sites in D02 and 1 site in D03. We selected 8 sites that are close to the selected meteorological sites and added two new figures to compare monthly mean vertical profiles of T2, dew point, and wind vector at those sites (see Figure 18-19). We also added relevant discussions in the

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text.

4) For the time series at specific sites, different selections of sites will lead to different conclusions. Also, from the time series, it is difficult to tell how close the predictions to the observation or which domain has better results. A statistics merit for each variable at each site will help the readers.

Reply:

We selected 14 sites (including 4 co-located sites) for time series comparison for T2, Q2, WS10, and WD10 and 13 sites (including 5 co-located sites) for time series comparison for Precipitation. Those sites cover a broad range of geographical and topographical locations in multiple countries including urban vs. suburban background and mountain, hill, and high plain sites vs. flat sites. We believe that such selections are representative. In addition, Figures 6 and 7 showed daily average of domainwide mean values of those meteorological variables, which were averaged across all observational sites. Further, the evaluation and conclusion of model performance were not based on time series comparison only, instead, they were based on statistical evaluation (Table 4), spatial overlay plots (Figures 3 and 4), daily bar plots (Figures 6 and 7), time series plots (Figures 8-17), and the vertical profile evaluations (Figures 18-19).

To address the comments, we added mean bias at each site for those variables shown in the time series plots in Figures 8-17 along with relevant discussions.

Specific comments: 1) Table 3 and Table 5 can go to appendix. Reply:

Table 5 was moved to the Appendix. For consistency, Table 3 in Part II was also moved to the Appendix in Part II. Given the focus of this paper (evaluation), we believe that Table 3 should be kept in the main text.

2) Based on Table 4, not all meteorological are improved using finer grid resolutions. Is there an explanation to this? Reply:

Some reasons were indeed given in our discussion. For example, on page 4010 in the
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original paper, lines 20-24, we mentioned “The high WS10 bias in the model simulations at all resolutions, in particular, 0.5° over D01 and 0.125° over D02, is due to the fact that WRF does not resolve subgrid-scale roughness elements (e.g. the surface roughness length or the friction velocity at the surface) even at the grid resolutions of 0.125° and 0.025° .”

The accuracy of the meteorological predictions depends on many factors including the accuracy of the input data such as land use and boundary conditions, the accuracy of model algorithms for all major meteorological processes under all meteorological and topographical conditions, as well as the uncertainties in model configurations (e.g., horizontal and vertical grid resolutions, nesting options, and data assimilation options). Table 4 shows a performance improvement for most variables in terms of NMBs when finer grid resolutions are used. Only a few variables show slight deteriorations (e.g., T2 and RH2 in Jan. 2001) in terms of NMB, but with reduced RSME and NME in D02 comparing with D01 (see Table 4b). As shown in Table 4c, comparing statistics from D03 with D02, a few variables show slight deteriorations (Q2 in Jul with 2.1% vs. 1.4%, WS10 in Jan. with 16.6% vs. 9.9%, WD10 in Jan. with 4.7% vs. 4.3%), but with reduced NME for Q2 in Jul. Comparing statistics from D03 with D01, a few variables show slight deteriorations (Q2 in Jan with -3.8% vs. -1.6%, RH2 in Jan. with -8.0% to -6.8%, WS10 in Jul. with -25.5% vs. -24%, WD10 in Jul. with 4.7% vs. 3.9%), with reduced NME for Q2 in Jan. The magnitudes of those deteriorations are very small.

Nevertheless, to address the comments, we added some discussions on this point in the revised paper.

3) Why wind direction is not shown in Figure 3 and Figure 4? Why no time series of wind direction shown at selected specific sites? Reply:

One reason why wind direction was not presented is because of the discontinuity from 0° to 360° on scales such as color plots and because showing wind vector arrows over Europe is cumbersome. To address the reviewer’s comments, we’ve now added the

overlay plots for WD in Figures 3 and 4. We also added two new figures (Figures 14-15) to show observed and simulated wind directions and relevant discussions in the text.

4) The value range of 0 to 360 for wind direction is not real direction. Difference between 0 and 360 is 0 instead of 360. Is this feature considered in the calculation of wind speed statistics? If it is used, it should be pointed out in Table 4. Otherwise, the values will be misleading. In Figure 5 and Figure 6, this situation should be considered as well. For example, July 28, 2001, the difference between prediction and observation at D02 and D03 is not as large as what we see.

Reply:

The wind direction is a vector, but it was treated as a scalar in our original statistical calculation in Table 4. The reviewer is correct that using this approach, the overpredictions or underpredictions that are larger than 180° may be misleading, which was indicated in our original paper.

To address the reviewer's comments, we have recalculated the WD statistics in Table 4 by accounting for situations when the differences between simulated and observed WD are greater than 180°. We also replotted Figures 6 and 7 (originally Figures 5 and 6) in Part I by accounting for such situations.

5) Add statistics to time series figures from 7 to 14?

Reply:

The mean bias has been added in all times series plots in Figures 8-17.

Reference: Emery, C., Tai, E., Yarwood, G., 2001. Enhanced meteorological modeling and performance evaluation for two texas episodes, in: Report to the Texas Natural Resources Conservation Commission, p.b.E., Internatioanl Corp (Ed.), Novato, CA.

Reply:

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We added this reference and also Tesche et al. (2002) for recommended benchmarks for meteorological evaluation.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 3993, 2013.

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