

Interactive comment on “Multi-year Downscaling Application of Online Coupled WRF-CMAQ over East Asia for Regional Climate and Air Quality Modeling: Model Evaluation and Aerosol Direct Effects” by Chaopeng Hong et al.

Anonymous Referee #1

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This study concerns the application and evaluation of a regional climate-chemistry modeling system. This is certainly an interesting topic and represents an advancement in climate-chemistry modeling. The evaluation of the system for both climate model driven simulations and re-analysis driven simulations are reasonably thorough and successful. My primary criticism of this paper is the lack of detailed description of the GCM model, the downscaling, regional model configuration, and execution. Even though references are given for the CESM modeling and the chemical and aerosol processes are briefly described I would like to see further description of the CESM physics, spin-up, constraints, etc. I do not understand how this represents a climate scenario

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when it is for past years and is evaluated against observations. What does RCP4.5 for these years represent. Do these runs use observation based SSTs? If these runs were spun-up from pre-industrial times without any observed data constraints, there would be no reason to expect agreement with observations. If bias corrections are made to both the meteorology and chemistry, then how do these runs substantially differ from re-analysis driven runs? Please explain the rationale and expectations of these runs.

I'm also wondering about data assimilation in WRF. Our experience has been that long runs of WRF (one month or longer) need some sort of DA or frequent re-initialization. If not in this case, how were the meteorology statistics this good? Even downscaling from GCMs often use data assimilation from the GCM. Also, an important omission from the WRF physics description is the LSM. Overall, I think that this study is worthy of reporting in GMD, especially the sensitivities of AQ and meteorology to aerosol direct radiative effects, and also the effects of dynamic BCs and biogenic and dust emissions. However, more explanation and description is needed particularly to help the reader understand the significance of the climate runs.

Specific comments:

P4Ins21-22: This statement about “correcting the roughness length by increasing the friction velocity by 1.5 times when calculating wind speeds in the ACM2 PBL scheme to reduce the overpredictions of wind speeds” needs more explanation. First, if the roughness lengths need correcting why not change them and not the friction velocity. Second, what is the problem with roughness lengths? How are they specified and what are they? Our experience has not shown general overpredictions in windspeed. Wind-speed and friction velocity are strongly affected by the LSM and surface layer scheme which are not even mentioned here. Also the LU scheme and data are important. The USGS 24cat data is way out of date especially for China where urbanization has been dramatic. Why not use MODIS LU?

P5In11-12: Why not use same vertical structure for WRF-CMAQ as CESM?

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P5In27: what is TOR?

Page 6: I don't understand what is the point of using RCP projections when modeling retrospectively. It seems that 2008 emission inventories are used for more detailed spatial-temporal allocation. Then why not just use these inventories? What is an RCP projection for past years? Please explain the logic here.

P8In1-2: Should also report RMSE or MAE. Small biases don't tell whole story. Large over and under predictions could cancel out.

P8In15-16: saying that large errors could be attributable to KF and Morrison schemes is pretty meaningless.

P8In28: Are the results shown in Fig5 averages for all 5 years?

P9In16: what are "upper BCs"? and where do they come from? And why are they particularly uncertain?

P9In16-17: Another meaningless statement about uncertainties in about everything possibly causing errors in NO2 column. Can you provide more insightful analyses?

P9In28-30: Please clarify this sentence.

P10In23-24: If a figure is important enough to be discussed in the text (S2) it should be in the main paper and not in the supplement. The reader should not need to see the supplement to follow the discussion.

P11In12-13: The names of the simulations are confusing. The "baseline" is NCEP_BASE_Imp but the sensitivity is NCEP_BASE which sounds more like it should be the base.

P11In25-26: How are the fixed BCs derived?

P12In4: S4 should be in main paper.

P12In15: "close" should be "closer"

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P13In11-12: Aerosol effects on photolysis in CMAQ do not depend on aerosol feedback in the WRF-CMAQ system. The more likely cause for ozone decline in the feedback run is increased NO_x titration in cities due to reduced PBL mixing. Table 1: what LSM and surface layer scheme? Table 3 and 4: Better to have un-normalized error for T2, RH2, WS10, WD10

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