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Atmospheric Chemistry and Physics
Discussions

# *Interactive comment on* "One-Year Simulation of Ozone and Particulate Matter in China Using WRF/CMAQ Modeling System" *by* Jianlin Hu et al.

**Anonymous Referee #2**

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The authors presented their efforts in examining the spatial and temporal distributions of air pollutants over China, with a full year simulation and local observations. Although the WRF/CMAQ model has been applied in many studies over East Asia recently, this full year simulation helps to probe into the modeling discrepancy and stability. The manuscript generally introduced the performance but lacked sufficient discussion or emphasis on the findings as a research paper. More explicit discussion and demonstration of the findings are necessary to illustrate the contribution of this manuscript. Therefore, I recommend the manuscript to be accepted with minor revision if the following questions/issues are being addressed in details by the authors.

(1) General comment: The manuscript spent a lot efforts describing the results of model evaluation, but lack of detailed discussion about the factors that are/may responsible for the good/bad model performance. For example, section 3.1 describes



the statistics summarized in Table 1, but it would be much more informative for the research community if the authors can discuss about why T2 is overestimated in winter. Is it because of physical options in WRF or bias induced by FNL? Section 3.2 describes the performance of O3 and PM2.5 predictions at different sub-regions in China, but there some important issues remain unknown. The research domain is divided into several sub-regions, thus the authors are expected to point out why the performance varies among sub-regions, is it correlated with bias from meteorology, emission, or chem/physical mechanisms applied? These are the real important findings this paper can contribute to the research community rather than demonstrating that this specific model application meets the benchmark.

(2) General comment: Some of the discussions in section 4 lack detailed explanation thus are not persuasive to support the conclusions. For example, line#440-448 states the bias from met prediction may affect the chemistry of CMAQ, but line#451 attributes the bias of SO2 and NO2 to anthropogenic emissions. Although CO is a good indicator for estimating bias induced by anthropogenic emission, SO2 and NO2 are often affected by other factors, such as meteorology, biomass burning emission as well. How does the overestimation of T2 affect the chemistry of SO2 and NO2, is it pushing the bias induced by anthropogenic emission towards or away from the benchmarks? Are there any evidences, such as satellite products/surface monitors/field studies, that can help to identify the location and intensity of emission bias? There are some published modeling studies using nested domains with different grid resolutions, are their findings support the statements in line#478-486? As the manuscript mainly focused on model performance, it is necessary to probe deeper into at least some of these issues, to investigate the contributions from different sources of uncertainties, such as meteorology, anthropogenic emission, biomass burning/biogenic/dust emission.

(3) Minor comment: Please briefly describe why sub-regions are defined to evaluation model performance.

(4) Minor comment: Table3, in Mar OBS and PRE for PM2.5 is 81.68 and 66.12 re-

spectively, while the MNB is only 0.04, please double check this statistics as it indicates large bias but strong correlation between observation and prediction.

(5) Minor comment: Please briefly describe how the MEIC emission is temporally/spatially allocated as CMAQ-ready inputs, since these factors have large impact on regional model performance.

(6) Minor comment: line#115, “interaction of” should be “interaction between A and B”

(7) Minor comment: How are the initial/boundary conditions generated for CMAQ ?

(8) Minor comment: line#175-178. The default inline dust emission module in CMAQ was reported to significantly underestimated the emission of total dust by Fu et al. (2013) and Dong et al. (2015) due to the double-count of soil moisture effect. But dust mainly dominates the coarse mode aerosol so it may not influence the performance for O3 and PM2.5 which are the focus of this study. Still I would suggest the author take a look at their PM10 results especially in spring, since PM10 is also in the criteria air pollutants.

(9) Minor comment: line#286, “Figure ?”

(10) Minor comment: line#504: “this is the first study”. Zhao B. et al. (2013, ERL) did a full-year simulation with WRF/CMAQ in China, and there are some studies with MM5/CMAQ in China prior to 2013 too.

(11) Minor comment: line#205-206: “WRF model has acceptable”. Table 1 indicates many of the variables failed to meet the benchmark. The Zhao B. et al. (2013, ERL) shows WRF performance all falls in the benchmark, so I would suggest the authors check their configurations of WRF namelist to either improve the WRF performance or specify the reason for relatively large bias in this study.

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