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Discussions

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Interactive comment on "Sensitivity of WRF-Chem model resolution in simulating particulate matter in South-East Asia" *by* Adedayo Rasak Adedeji et al.

Anonymous Referee #5

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This paper uses the WRF and WRF-Chem model to simulate the severe haze events in June 2013 in the SE Asian region. It calculates the biomass burning emissions using the 3BEM model with inputs from the MODIS and VIIRS fire "hot spots" and the WFABBA database while takes other emission inventories for anthropogenic and biomass burning emissions. Model simulations with two horizontal spatial resolutions, 100-km and 20-km, are conducted; results of a few meteorological fields and concentrations of PM2.5 and PM10 are compared with surface measurements at two ground stations and model calculated AOD is compared with the reanalysis product from MERRA-2. It concludes (a) the model is adequate, (b) the 20-km resolution results are somewhat better than the 100-km resolution counterparts, and (c) the biomass

burning emissions should be enhanced by a factor of 1.3 to 6.

This paper presents a routine model evaluation instead of a thorough analysis, which is fine and a needed exercise. However, I have several concerns of the model evaluations and think a major revision is necessary.

1. Purpose: From the introduction, it seems that the present study is motivated by the recent MICS-Asia Phase III study pointing out that most air quality model were developed mostly in Europe and USA such that the parameterizations and assumptions may not be suited for Asia, and calling for "rigorous investigations on the performance, sensitivities and uncertainties of these models in Asia". However, the present study does not test any of the parameterizations and assumptions in the WRF or WRF-Chem model, but only choose to examine the spatial resolutions.

2. Sensitivity: the title suggests that the major focus is to address the sensitivity of the model performance to the model spatial resolution. But at the end, the model results show little sensitivity to the spatial resolutions. Rather, the emission amount dictates the simulation quality. This should be explicitly quantified.

3. Model resolution: 100-km and 20-km are very coarse resolutions for a regional model. Most global models are using similar resolutions nowadays. 20- and 4-km (or finer) resolutions would make a much better sense.

4. Biomass burning emission: Because the biomass burning emission is such a key parameter determining the model simulation quality, it grants a much more thorough assessment in the study. The description of emission needs to be in more detail. For example, what are the input quantities used in 3BEMS? How the MODIS, VIIRS, and WFABBA datasets are implemented in 3BEMS? What species are emitted? And finally, how does the biomass burning emission calculated in the present work compared to other readily available emission datasets, such as GFED4, GFAS, FEER, FIN, etc. for the same regions during the same time period?

5. Model evaluation: The evaluations are carried out mainly by comparisons of data from two ground stations, one in Singapore and one in Brunei. The model results from two different spatial resolutions and two different interpreting methods (nearest neighbor and bilinear) are evaluated using correlation coefficients, NRMSE, RMSE, and NMBF and resented in Table 2, 3, 4, and 5. Given the slight differences of these parameters between the results from 100-km and 20-km resolutions or between the two interpolation methods, the question is: are these difference significant enough that you can conclude that one resolution or one interpolating method is definitely and consistently better than the other one? Also, what lacks is an overall "skill score" that considers all the above quantities, which is commonly used in the weather and air quality forecast communities.

Specific comments:

Page 2, line 43: "numerically studied" – what does it mean?

Page 2, line 44-45: "The correlation coefficient" – between what?

Page 4, line 118: "...could be used" – did you actually use the daily fires detected from different satellites? If so, it should be "...were used".

Page 4, line 126-128: How did you use the two sets of emissions (RETRO and EDGAR)? Did you average them?

Page 4-5, line 133-134: What is "NALROM" simulation? Why do you have to use the "idealized" northern hemispheric mid-latitude and clean environment conditions, which has nothing to do with your study area?

Page 5, line 139: "The emission inventories were updated for each day": How do you update the emission inventories for each day for anthropogenic? I don't think the RETRO or EDGAR provide daily emissions.

Page 6, line 169-174: I find these paragraphs and associated Figure 1 are confusing and seem unnecessary. Why assuming a series of grid cells with different horizontal

resolution is necessary while your model uses even grid space? To me, just describe briefly how the evaluation of model output in a grid with a point measurement was done is enough.

Page 7-9, subsection 4.1.1-4.1.4 and Table 2-4: The organization of section 4.1.1-4.1.4 and Table 2-4 should be consistent. At the present, the Tables are organized as one table per station with all four met variables, whereas the subsections are organized as one met variable per subsection at all stations. This makes the readers going back-and-forth with the tables while reading the text. I suggest reorganize either the subsections or the tables to make flow together.

Page 7, line 209 and Figure 2: Figure 2 shows all four met variables, but it is only mentioned temperature here, not in the other subsection.

Page 7, line 214 and Figure 3: Figure 3 also shows 20kmX and MERRA-2, but they are not mentioned here! Why are they used in Figure 3?

Page 9, line 282-283: The statistics of evaluation from the 100- and 20-km simulations are very comparable according to the numbers listed in Table 2-4. Repeating what I said at the beginning, there should be overall skill scores to quantify the significance of the differences.

Page 9, line 283: "...also very good": What is the standard of being "very good"? These subjective words should be avoided in the model evaluation. Instead, quantitative information should be given, such as "within a factor of xx%".

Page 9, line 287, Figures 4 and 5: I suggest swap Figure 4 and 5, i.e., show PM2.5 first in Figure 4 and then PM10 in Figure 5 to be in the same order of Figure 6 and 7.

Page 10, line 291: Again, please avoid using subjective phrases such as "good results".

Page 10, line 294: What is "over-approximation of topography"? How was it done? It is very unclear.

Page 10, line 299-300: But the 20-km simulation has larger bias than the 100-km simulation. Why is it better? R is not everything. In many cases, bias is as or more important.

Page 10, line 304, GFAS: Why is GFAS relevant here since you don't use GFAS?

Page 10, line 308: another "very good". Please be quantitative and objective.

Page 10, line 309-310: What is the size range of biomass burning aerosols? Any fraction in the coarse mode with diameters greater than 2.5 um?

Page 10, line 311, regarding Table 5 and Figure 7: Table 5 shows that 20kmX simulation resulted in a positive bias (0.180) for PM10 in Brunei, but Figure 7 (bottom panel) seems to suggest an overall negative bias. Please check.

Page 10, line 313: Describe MERRA-2. It just suddenly shows up here without any description. In fact, as I said earlier, Figure 3 showed a figure from MERRA-2 without any context.

Page 10, line 317-318: a factor of 1.3-6 is a very large range. Any constraint? It seems your study suggest a factor 6 since the default 1.3 does not work for PM2.5 and PM10; on the other hand, using a factor of 6 will substantially overestimate the AOD compared to MERRA-2.

Page 11, line 340: What is the criterium for "adequately"? Be quantitative, such as "within a factor of x".

Page 12, line 361-362: "...without fire emission enhancements, the model succeeded in capturing PM emission across the region": How did you reach that conclusion? You only evaluated the PMs at two stations, not many stations across the region.

Page 12, line 363: "...may be as a result of errors present in the biomass burning emission inventories": It is important to compare the biomass burning emission from this work to other emission datasets to at least see how they compare.

Page 12, line 369: It is premature to suggest an enhancement factor of 1.3-6 used in this particular work for all emission inventories, unless you have compared all the available ones.