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Atmospheric Chemistry and Physics Discussions

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 #1

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General comments:

The manuscript has applied the WRF-Chem model to study the biomass burning haze episode in Southeast Asia from 15th to 30th June 2013, with research focus on grid resolution and interpolation approach. The research scope of the manuscript fits well with the interest of the readership of ACP journal. However, several improvements need to be made before the manuscript is deemed suitable for publication.

The sensitivity analysis of the model performance is the main highlight of the work, with focus on the grid resolution test. However, 20 km used as the fine grid resolution is comparatively coarse compared to previous work done in the region that have done

into 9 km grid size. Given sufficient computational ability, 20 km can be better applied as the nest to the coarse grid (100 km) which will be a better option for more accurate modelling performance. For the sensitivity study to be conducted in this rather short period of study, the number of verification stations and attempted physical settings are insufficient. On top of that, the scientific basis on the grid resolution selection and enhancement ratio is lacking to convince on the applicability of such method. The limitation of these method should also be highlighted in the manuscript. In order to improve the performance and reliability of the output data, it is advisable to test and choose the grid settings wisely to achieve desired goal.

The paper has highlighted several aims that are not attained especially aim 1 and 2 that are briefly discussed with subjective statement from the modelling output. Missing references and syntax error are easily sighted in the manuscript. Detailed and specific comments are given as below. Therefore, the manuscript needs to go through major revision before it is ready for acceptance and publication.

Specific comments:

1. L57-59: Perhaps you can discuss from the point of view of emission composition from the burning materials

2. L83-39: Case June 2013 were studied in Oozeer et al. (2016) (cited in manuscript) with a higher resolution of 9 km and the model performance is reasonably well. How do you argue that the 20 km setting in current work is known as high resolution compared to theirs? Also, with 100 km to 20 km, it is already possible that you are able to have a mother domain (100 km) with nest (20 km), this is able to further improve your weather prediction result seeing that you are using a 1°x1° (∼100 km) NCEP FNL dataset (L111-114).

3. L92-107: Detailed description of the options are not necessary, instead please state the reason and basis why these chemistry and physics settings are chosen.

4. Please include Section 3.1 into Section 2 because they are part of the WRF-Chem settings.

5. L125-127: Can you please cite this statement and explain why you are still using the WFABBA dataset if it is inaccurate? If not, please kindly explain what improvement have been made to tackle this issue.

6. L188-200: This section should be moved to Section 3.2.

7. L218-221: The model performance is less satisfactory, especially for temperature that could be easily meet the standard of RMSE belowïĂăïĆś1.5ïĆřC in the stable weather condition in tropics. The performance of predicted weather variables have a big impact on the chemistry field, so please kindly consider using the nesting option and nudging function to improve the result for the weather field in the first place.

8. L250-289: Same applies for humidity and wind speed. The verification result of upper layer using radiosonde is missing from the content.

9. L310-312: Please explain how to obtain 6 as a factor and what do you based on?

10. L306-312: Previous work conducted in the region has suggested that the fire injection height in the region has to be adjusted. Please kindly refer to Wang et al. (2013) Atmospheric Research 122, 486–503.

11. L320-323: The argument is less convincing for just testing with one of the values, please kindly test with more settings to confirm that the enhancement factor are selected appropriately.

12. L325-335: The “formation” and “deep convection” aspects of the biomass burning emissions are totally not discussed in the result and analysis.

13. Section 4.2: Figure 4,5,6,7,8 showed that the enhancement factor for 20km improved the prediction because they produced larger amount of PM2.5. If comparing 100 km and 20 km, 100 km has also produced larger emission of PM2.5, hence, why

100 km case doesn’t give a much better result compared to 20 km case?

14. Figure 1,3,4,5,8: Please properly label a,b,c,etc. next to each figures and describe them in the figure caption.

Technical corrections: (only listed a few)

1. L24: correct progronostic to prognostic

2. L29: correct enhacement to enhancement

3. L49: Make sure the citation is done properly for both microphysics and boundary layer schemes.

4. L92: Remove “list references”

5. L139: Missing reference for Mozart boundary condition.

6. L216: correct time-avergaed to time-averaged, distrubtion to distribution

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