

Interactive
Comment

Interactive comment on “Air Quality Modeling with WRF-Chem v3.5 in East and South Asia: sensitivity to emissions and evaluation of simulated air quality” by M. Zhong et al.

Anonymous Referee #2

Received and published: 2 January 2016

General comments:

This research presented the WRF-Chem model evaluation over East and South Asia. For model evaluations, the authors uses EANET observation network, Kathmandu valley site, Mt Lulin site in Taiwan. Moreover, API was used as a proxy of PM10 concentration in China. Such synergetic analysis over Asia, where the air quality is concerned issue, are needed to further promote our understanding. Before the model evaluation, the authors provided the sensitivity simulations on the basis of two different emission inventories of REAS and EDGAR. This is also the attractive point of this study. However, some discussion points would not be robust. I would like to recommend the authors to

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revise the manuscript.

1) 'South Asia' is included in the title of this manuscript, and discussed (P9386. L17-25). However, the model evaluation was done at only one site of Kathmandu valley and for only PM10. It would not be appropriate to include 'South' in title of this manuscript. Are there no observation dataset as network or literature over India or any other countries in South or Southeast Asia?

2) The concluding remarks of 'The findings suggest that future model development and evaluation of emission inventories and models are needed for particulate matter and gaseous pollutants in East and South Asia' on Abstract and Conclusions section is general and ambiguous. This sentence will not provide the authors findings through this study.

3) Discussion on the sensitivity simulation with different two emissions of REAS and EDGAR will not be robust. The authors concluded that the REAS inventory showed better performance, and the further study was conducted with REAS. However, this conclusion was simply from the sensitivity simulation on the 14-day simulation in July 2007. What about other period? At least, I recommend the authors to provide the specific reason to select this 14-day simulation to evaluate two different inventories.

Specific comments:

1) P9377, L9-11: Does this sentence refer the finding from this study or other study? If the former, it should not be mention here. If the latter, please insert the appropriate references.

2) P9378, L9-12: What is the necessity to use WRF-Chem model? What are the advantages?

3) P9378, L21: Was the aerosol-radiation feedback tuned on in the WRF-Chem simulation?

4) P9379, L13-21: Why the lateral boundary condition was not corresponded to ana-

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lyzed period?

5) P9380, L3: Does 'PM10' in emissions (shown in Table 1 and Fig. 2) include PM2.5? Also, 'PM2.5' in emissions include BC, OC, and other primary particulate matter which aerodynamic diameter less than 2.5 micrometer?

6) P9380, L17-19: So, what is the treatment of BC, OC, and PM2.5 on the simulation using EDGAR? Taken from REAS?

7) P9380, L22: Please specify the temporal resolution (e.g., hourly, daily, monthly, annual) on each emission inventory used in this study.

8) P9386, L14-17: However, better performance in weather will be the advantage of the 'online' coupled WRF-Chem model compared to other model (e.g., CMAQ).

9) P9387, L13-15: But Haplo in Japan is also mountainous site (Table S2b). Any other reasons?

10) P9388, L2-4: What is the source of coal combustion?

11) P9390, L7: Correct the typo 'mode'.

12) Figures 8 and 11: X-axis have different months, so these figures have some difficulty to understand.

Interactive comment on Geosci. Model Dev. Discuss., 8, 9373, 2015.

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