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Geoscientific Model Development Discussions

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 #1

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General Comments

This paper outlines air-quality evaluation of simulations conducted with the WRF-Chem model over China. Sensitivity to emissions is tested by using two commonly used anthropogenic emission inventories (REAS and EDGAR), finding large differences between the two for certain species. This is followed by an evaluation of key species relevant for air quality vs. ground site measurements across China and Japan, using output from the WRF-Chem model run with the REAS emissions.

In general, I think the paper is well written and easy to follow. However, there are several points I would like to see addressed before it would be suitable for publication.

I think the paper would benefit from stronger justification (explaining the novelty of the study) and discussion of the impacts and implications of its findings. This is particularly true in the conclusions section, which mostly just summarises the results and only offers a short last sentence of the form "These findings suggest future work is needed. . ." as implications of the study.

Some more details are needed in the description of model setup for the simulations to be reproducible (details given in specific comments below).

As far as I can see there is no evaluation of the meteorology in the model in the paper. Representing the meteorology reasonably is a first-order pre-requisite for simulating the air-quality accurately. Please show some comparisons of key meteorological variables with some ground sites and/or satellite products (e.g. TRMM), either in the main paper or supplementary material.

In section 4, there is no evaluation of PM2.5 against measurements. I appreciate that for section 3, this is not possible for the emissions sensitivity comparison without PM2.5 emissions in EDGAR. However, this is not a limitation in the later half of the paper, where you are just using the REAS case study. It looks as though the PM10 results you present are being strongly impacted by dust (given the highest readings are in the North West), so looking at PM2.5 should be more representative of anthropogenic pollution. Amalgamating PM2.5 comparisons into section 4.1 as a single PM section would be beneficial.

Specific Comments

Pg 9377, ln 11-13: You should not be presenting your findings in the introduction. Replace this sentence with a reference which says supports your statement, i.e. which highlights the variation in PM emissions between inventories and the uncertainties in emission estimates.

Pg 9377, ln 23-24: Are there really no other studies investigating PM dependence to emissions inventory in Asia?

Pg9378, ln 10: Zhang et al., 2015 is at least one paper I know of which compares WRF-Chem model output with a wide array of measurements across China (albeit mostly just for PM2.5). As one of the main justifications of your study was that it is the first WRF-Chem study to compare against an extensive network in East Asia, please expand on why your study is novel in light of this (and possibly other) papers.

Section 2.1: some questions on model setup:

- Do you run with aerosol-radiative feedback?

- Do you periodically re-initiate the meteorology, or run with nudged met?

- Do you know how sensitive your model simulations are to the boundary conditions? It seems as though you are using 2010 data from AM3, is that representative of the 2007 period you are running over? Is there any reason why you are not using a product driven by meteorological reanalysis, e.g. MOZART-4 (Emmons et al., 2010)?

Pg 9830, section 2.2: Do you have any seasonal or diurnal variation in your emissions? If not, you should also mention this when discussing potential reasons for model discrepancy, particularly in terms of different magnitudes of error in different seasons in section 4.

pg 9831 ln 25 – pg 9832 ln 3: The sentence “For PM10, we use the performance goals and criteria of Boylan and Russell (2006).” Is redundant. Please delete and put the reference at the start of the next sentence :”Following Boylan and Russell (2006), we set. . .”

When you say “. . .goals and criteria for MFB to be less than or equal to 30 and 60%, respectively”, it is not clear what the ±30% and ±60% refer to, as you are only discussing MFB for PM10. Please clarify.

Add “±” in front of “50%”, “75%” and “35%”.

Pg 9832 ln 13: Please make explicitly clear here that the similarity in SO2 emissions is purely coincidental, and you would probably get very different values if the domain was different.

Pg 9383 ln 4-5: While I appreciate doing a detailed analysis on the causes of differences in emissions between inventories is out of the scope of the paper, it would be interesting to have some more detail on why the two are different. This discussion may be better placed in the introduction or section 2.2 than here.

Pg 9383 ln 22: change “large” to “largest”.

Pg 9383 ln 3-4: It would be helpful for those with less knowledge of the geography of China to have a visual representation of the different Chinese regions, rather than just a list in the supplement. Perhaps boxes drawn over the different regions in Fig. 1, or a similar map in the supplementary material?

pg 9384, ln 11: I am confused by the line “More detailed comparisons, assessing the impacts due to various inventories, will be conducted in our future work.”

This stuck me as a strange way to structure a set of papers. I agree that having more emission inventories to compare would be of benefit, but I would have thought the best place to present that would be in this paper, rather than another one. This would make the emissions sensitivity section much more interesting. A second paper focused on a more thorough evaluation with the chosen inventory (REAS) and/or investigating other scientific questions (e.g. impacts on health/climate) would make an interesting follow up. Please comment as to why you have written the papers in this form.

Pg 9385, ln 24-25. I see you have measurements from Lhasa, please can you comment on whether your modelled seasonal trends of PM10 in Tibet are seen in the measurements as well. If they diverge, what does this say about the dust emission scheme?

Pg 9386, ln 15-17. This can be evaluated by comparing the wintertime model meteorology with measurements.

Pg 9388, ln 2-3. Please comment on the seasonal variation of your emissions in the model. If there is none, the difference in modeled concentrations must be due to meteorological changes between the seasons.

Table 3, 4, 5 and 6. Please say in the caption that you are using data from the WRF-Chem-REAS simulation. I know you say this in the text, but it is good to be reminded here to avoid confusion.

Figures 4, 7, 9 and 10. These figures will look clearer if lowest value of the scale is set to the first value above 0 (e.g. 20 instead of 0 for figure 4), as this will make the background white and make it easier to pick out regions of higher loadings.

References

Emmons, L. K., Walters, S., Hess, P. G., Lamarque, J.-F., Pfister, G. G., Fillmore, D., Granier, C., Guenther, A., Kinnison, D., Laepple, T., Orlando, J., Tie, X., Tyndall, G., Wiedinmyer, C., Baughcum, S. L. and Kloster, S.: Description and evaluation of the Model for Ozone and Related chemical Tracers, version 4 (MOZART-4), Geosci. Model Dev., 3(1), 43–67, doi:10.5194/gmd-3-43-2010, 2010.

Zhang, B., Wang, Y. and Hao, J.: Simulating aerosol–radiation–cloud feedbacks on meteorology and air quality over eastern China under severe haze conditions in winter, Atmos. Chem. Phys., 15, 2387–2404, doi:10.5194/acp-15-2387-2015, 2015.

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