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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Why models perform differently on particulate matter over East Asia? – A multi-model intercomparison study for MICS-Asia III" *by* Jiani Tan et al.

Anonymous Referee #2

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General Summary: This study compares performances of 12 regional models from MICS-Asia III in simulating particulate matter during 2010. Performance is evaluated in terms of PM10, PM2.5, sulphate, nitrate, and ammonium. The mean bias of multi-model mean range from -30% to 12% for different species. The analysis focuses on four processes that are likely contributing to the inter-model differences namely natural emissions, gas to particle conversion of sulphur and oxidized nitrogen, role of dust emissions, and deposition parameterization. I think the subject of this paper is suitable for publication in ACP but I have several concerns about the organization and analysis in the paper. Therefore, I recommend major revisions before the paper can be published in ACP.

Firstly, the supplementary information is overwhelming and contains a lot of the material that can be moved to the main paper. Specifically, I suggest moving the evaluation part to the main paper and several key discussions (e.g., supplementary sect. S2.5, table S1 and fig. S8) related to the process analysis.

Second, I have some concerns using CO and BC as surrogates for anthropogenic emissions (see specific comment on lines 258-259).

Third, I have some concerns regarding evaluation with MODIS AOD (see specific comments on the evaluation part).

In addition, there are many grammatical errors that I believe can be removed upon reading by a native English speaker.

Here are my specific and minor comments:

Abstract: Line 32-34: Why would natural emissions affect BC concentrations and CO loading?

Minor comments: Line 58: Change "pollutions" to "pollution".

Line 61: Change "impact evaluation on" to "impact evaluation of"

Line 86: Change "view on" to "view of"

Lines 88-89: Spell out different acronyms.

Table 1: Do you mean AERO5 instead of AEO5 in 6th row.

Line 154-155: This is not correct. GOCART in WRF-Chem accounts for ammonium mass in calculation of PM2.5 and PM10 by multiplying sulfate concentrations by 1.375 to account for missing ammonium. GOCART also includes fine dust and sea-salt in PM2.5.

Line 161: Change "transport" to "travel".

Line 163: Change "taken" to "take"

Line 176: How about turbulent diffusion?

Line 174-180: Please discuss wet deposition parameterization as well.

Line 184: Change “summaries” to “summarizes”

Figure 1: Except for panel (a), I am unable to see the markers for observations in the maps of PM2.5, sulfate, nitrate, and ammonium. I suggest showing standard deviations in absolute values as well because the current plots give an impression that large variability (more than 80%) exists over the regions of lowest concentrations (e.g., Tibetan Plateau) but this is simply a result of division by a very small number.

Line 195: Can you explain in more detail why dust emissions are so different among different models? Is it because of wind speed or soil moisture or source functions?

Line 215: Remove “slightly”.

Figure S6: Why isn’t there a complete seasonal cycle of MODIS AOD at many locations?

Figure S7: Do white spaces in the maps correspond to AOD below 0.1 or do they also correspond to missing data? Have the model and MODIS AOD been collocated before comparison? Did you use Level 2 MODIS AOD in this comparison?

Lines 219-225: Again, could you please say more about the inter model differences here? Are differences in AOD controlled by differences in aerosol chemical composition or by differences in aerosol size distributions or assumed aerosol optical properties?

Line 234: At line 190, MB in PM10 for this study is reported as -25 ug/m3 but here it is reported as -11.2 ug/m3. Which one is correct?

Line 240: Change “so-call” to “so-called”.

Lines 258-259: It is difficult to understand why it is so hard to get access to the emissions used by different groups. Using model simulated CO and BC as surrogates for

emissions is not a good approach because they are strongly influenced not only by emissions but also by transport, deposition, and chemistry (in case of CO). Use of column CO is especially concerning because CO distribution in the free troposphere is primarily controlled by transport (inflow from domain boundaries in case of regional models) and not by emissions. Similarly, regions downwind of strong emission sources will exhibit disproportionately larger concentrations of BC and CO than the emissions. Therefore, I recommend using actual emission fluxes in your analysis in Section 3.2.

Lines 325-326: If the models were able to capture SO2 concentrations, would they have overestimated SO4 considering that they are simulating SO4 reasonably well even with underestimated SO2 concentrations.

Line 364: Sea-salt emissions are controlled via a namelist option in WRF-Chem. Was there a specific reason for turning off sea-salt emissions in M7 and M8?

Line 390: μg cm-3 or μg m-3?

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