

Interactive comment on “Air Quality and Climate Change, Topic 3 of the Model Inter-Comparison Study for Asia Phase III (MICS-Asia III), Part I: overview and model evaluation” by Meng Gao et al.

Anonymous Referee #1

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This paper summarizes the overall performance of several fully-coupled air quality models that participated in the MICS-Asia III intermodal comparison study. It is the first part of a multi-part study. While the paper is well organized and the discussion is straight-forward, there are numerous places where the grammar needs to be fixed. I have tried to make some suggestions in my specific comments; however, the authors should not assume I have found all the problems. There are some aspects of the manuscript that are not explained well, such as the rationale for this paper in relation to future parts and the rationale of the intercomparison framework. By the time I finished

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the paper, I feel like I did not learn anything substantially new; therefore, the authors have not adequately highlighted the new results of this study.

Major Comments:

1) In the introduction, the authors talk about Topic 3 of MICS-Asia phase 3 which is the subject of this paper. At the end of the paper I felt like I did not get any information regarding the feedbacks. Perhaps the paper title is implying that those details will be included in subsequent parts. It would be useful at the end of the introduction to have a clear understanding of what the objectives are for this paper, versus subsequent parts that will appear.

2) The authors do speculate why there are differences among the models; however, the paper would be improved significantly if they went into more detail into a few instances to find more concrete reasons for the differences. This might require more analysis of the results. But as the paper stands, it does not shed any new light on why the air quality models could differ. In the conclusion, the authors state that the paper provides “some directions of future model developments”, but I see no evidence of that in the paper. The authors could also do a better job at citing papers that examine processes that might be missing or poorly represented in the air quality models.

3) The purpose of the MICS-Asia phase 3 is to look at feedback effects. I gather that is not that subject of this paper, and this paper is showing the initial evaluation of the aerosol simulations that will be important when looking at feedback effects later. The authors go into some detail on evaluating aerosol composition, but do not say anything about size distribution. Size distribution will be just as important for optical and CCN properties. I suggest adding a section that compares the predicted size distribution in some manner. I assume there are some sort of size distribution measurements that could be utilized. If not, it would still be useful in the context of the subsequent papers.

Specific Comments:

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Line 43: Change “resolutions” to “resolution”.

Line 56: I would change “are consistent” to “are similar”. “consistent” can imply that the model results are good, but they could all consistently disagree with data. Change “haze event” to “haze events”.

Line 58: “some brief senses” is an awkward phrase and should be replaced.

The abstract could be shortened so that it contains only the most important findings. For example, the sentences in lines 44 – 48 could be removed. The whole abstract is rather weak.

Line 66: Change “but primarily in Asia” to “but most deaths occur primarily in Asia.”

Line 74: I would not use a semi-colon here and just have two sentences, although the second would need to be rephrased slightly.

Lines 134-139. There appears to be no underlying motivation for how the air quality models are compared. The only constraint on the models was the use of the same emissions inventories and they had to provide a set of variables. To better isolate the differences among the models, it would have been useful to have similar domains, grid spacings, and boundary conditions. I understand it would make the setting up the models a bit more difficult, but it would significantly reduce the differences arising from boundary conditions and spatial resolution. There are already many differences associated with the internal treatments of meteorology, chemistry, and aerosols. What I am looking for here is some further explanation as to why MICS-Asia organizers found the current configuration sufficient.

Line 153: The Grell reference is correct, but it only describes the initial model which did not discuss any of the feedback processes – which seems to be the focus of MICS-Asia Phase III. Those feedbacks were first implemented in Fast et al. (2006) and revised in subsequent manuscripts.

Line 182: For VBS, need to cite Adhamov et al. (2012) in which it was developed and

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described.

Lines 190-191: The sentence regarding SOA is not correct and misleading. A VBS SOA treatment has been available in the public version of WRF-Chem for several years. What the authors mean to say is that the version of MOSAIC used in this study includes no SOA. The correct language here should imply that the users have chosen not to include SOA.

Section 2.1, in general. The description of the models is uneven. Some sections go into some details about the aerosol model, such as noting the geometric means of the modes (e.g. M5) but not going into the same detail as another model (e.g. M1). For one model the details of how optical properties and hygroscopicity are discussed, but then another model the same level of detail is not discussed. The authors need to revise this section to have the appropriate level of detail for all models.

Lines 232 – 239: The text discusses differences in the physics configurations as it should, but I assume the other models have physics differences too. Why not state that?

Line 286: What does regridding mean? To handle the emissions inventories properly, the emissions need to be reappportioned so that mass is neither gained or lost. Regridding implies interpolation, that to me indicates a poor method of handling the emissions from one domain to another.

Line 409: Change “are frequently happening” to “frequently happen”.

Section 4, in general: this study relies on comparing model output to relatively few (at least for the PM data) point measurements. However, some discussion is needed to put the proper context of this type of comparison since the grid size differs among the models so there are issues of representativeness that must be considered.

Line 442: Turbulent mixing is missing from this description, which is not the same as transport.

Lines 469-483: What is missing from this discussion is how clouds affect the prediction of downward shortwave radiation. I assume that the clouds are the main factors controlling clouds, but there is no mention of this. Would be useful to include what the clear-sky values are in Figure 5.

Line 493: Change “larger near surface” to “larger near the surface”.

Line 498: Awkward sentence – need to revise.

Line 513-514: Change to “All models produce similar CO predictions” based on how I understand this sentence.

Line 522: It is rather surprising that the models produce better PM than ozone. Usually it is the other way around.

Line 548: I doubt that sea-salt emissions are responsible for differences in PM10.

Line 569-580: Chen et al., ACP (2016) is just but one paper that describes possible missing reactions associated with sulfate. It would be useful for the authors to delve a bit deeper into the literature to find such issues associated with models. Most community models are inherently dated and do not necessarily have the most up-to-date chemistry treatments since it takes time for new research findings to make there was in to those community models.

Line 593-594: There are probably other reasons as well for errors in nitrate predictions.

Line 607: The authors list deposition, but this usually means dry deposition. What about wet scavenging? Same comment applies to line 610.

Line 607: Change predicted BC to “predicted BC at the surface”

Line 609: I think the authors mean horizontal grid resolution and not “horizontal grid interpolation.” I have no idea what the latter means in this context. Please be more specific.

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Line 612: Since POC is about the same from the models, then BC should be as well. So it is a bit of a mystery why BC from M2 and M7 are higher than the other models.

Line 621: Find a reference for this comment – there are lots of papers to cite here.

Line 638: This implies the model is missing a feedback, and I thought this study was about in the inclusion of feedbacks (see line 106 on topic 3).

Line 642: “dust deflation” is an odd phrase. What is that?

Lines 650-652: These sentences are poorly written. Suggest changing to “Only the sulfate predictions from M5 are close to the observed values. Sulfate is much lower than observed for all other models, except M6 which is too high. M2 and M5 predict reasonable nitrate concentrations. M3 and M4 overpredict OC during the haze period, but other models underpredict OC concentrations.”

Line 677: What about clouds? How often were AOD retrievals not possible due to cloudy conditions?

Line 680: The figure captions should also state that the AOD is a daily (daytime) value.

Line 686: Change “it’s” to “it is”.

Line 718: Change “shows overprediction” to “shows an overprediction”.

Line 724: Change “lower RH simulation” to “lower simulated RH”.

Line 726: Change “OC concentration” to “OC concentrations”.

Line 736-737: The authors have not shown this. It is very likely the size distribution and mixing state is treated differently. In this sense, the explanation provided previously in the paragraph is incomplete. I doubt one can really attribute the difference in AOD without a more rigorous analysis than the simple explanations presented here. At best, they are showing the range of AOD associated with all the differences among the models.

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Lines 773-775: I don't see how interpolation of emissions to the grid should lead to model uncertainties. Of course, there could be errors introduced to reapportion emissions from one grid to another. But these would only be large if the mathematical method of reapportionment is poorly treated. There are ways to ensure that such uncertainties are small.

Line 776: "Manifold" is a strange word to use in this context.

Line 783: And what are those improvements? I would that that such an inter-comparison study such as this would provide more concrete recommendations.

Line 788- 792: This sort of general conclusion about model inter-comparisons studies is rather tired. It could have been written based on results already in the literature and by speculation, without even conducting this inter-comparison study. I wish he authors could be more specific here regarding the findings specific to this study. The authors have not investigated all these possibilities and only barely scratched the surface at isolating and ascribing the uncertainties to specific processes.

Conclusion: It would be useful to have some text that looks forward to the next part of the paper. Will the authors be looking at evaluating the feedbacks, which were not examined in this study? Another reason I am looking for some more concrete explanations in this paper for why the models differ is that the situation will only become more complicated when examining feedback effects.

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