

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 #2

Received and published: 3 November 2017

The paper describes the setup of the MICS Asia Phase III model experiment. The results of seven simulations with online coupled meteorology-atmospheric chemistry models are shown and compared against observational data. The evaluation of online coupled air quality models and the outcome of the MICS-ASIA III model inter-comparison exercise are certainly worth to be published. However, the quality of the paper must be improved significantly before it can be published in ACP.

In the first instance, the 'Results and discussions' section must be enhanced considerably. Attempts should be made to explain the reasons for the observed differences

C1

among the models. In particular, a more in depth discussion is necessary for those model results which look like outliers (for example solar radiation and ozone in Figure 5 and 6).

Although the paper is overall well organized, it was nevertheless a though read for me. It is sometimes difficult to keep track of the different models and their respective setups. Repetition of the model name along with the label once a while could improve this situation with only little effort. Enhancing the figure captions would also help. Finally, several sentences are quite convoluted and hard to understand. Splitting long sentences may improve the readability.

For these reasons and further reasons mentioned below, major revisions are required. The specific comments include some suggestions how to improve the paper.

Specific comments (also including minor points):

Abstract: In its current form the abstract raises the expectation that aerosol meteorology interactions will also be a topic of this paper. The reader may also expect that both episodes will be evaluated in the paper. Generally, the first part of the abstract is raising expectations, which are not fulfilled by the paper. Nevertheless, it is also OK to restrict the paper to the evaluation of first episode. However, the abstract must be reworded in this case in order to avoid raising expectations which are not met later. It is not clear why the model evaluation is restricted to the first episode of the model inter-comparison and why the second episode is not considered.

Introduction: The authors should also consult publications describing related work, e.g. the model evaluation papers related to HTAP as well as AQMEII Phase 2 and Phase 3.

Line 106: 'Various multi-scale models . . .' Are there more than the five model and seven simulations described here?

Lines 114 –119: The references should not be restricted to Chinese authors.

C2

Line 125: The paper claims to be 'serving as the main repository of the information linked to Topic 3 simulations and comparisons.' To achieve this aim, more details must be added to the descriptions of those models where the description is quite. Please add also some paragraphs in section 2.1 (similar to section 2.5) in order to make this section better readable.

Table 1: The numbers attached to 'WRF-Chem' and 'NU-WRF' are unnecessary and confusing. Therefore, they must be removed. On the other hand, the model version is an important information which must be added wherever applicable.

The contents of the table are not precise: For example, for M1 'RACM' must be replaced by 'RACM-ESRL' and 'MADE' by 'MADE/VBS'. What climatological data are applied as boundary conditions for M6?

Please consider to add also information about the details of the radiation calculation. As shown by Curci et al. (2015, Atmospheric Environment, Vol. 115) the inherent assumption have a strong influence on the calculated AODs.

Table S1: Please check also whether this table needs to be more specific (similar to Table 1). Please add also information about the two models which are missing in the table.

Please consider also moving this table into the main part of the paper.

Line 338: Please mention the source of the climatological data here or in Table 1.

Line 281: Please add the information which model uses the prescribed BVOC emissions and which one does the internal calculation (could also be added to Table 1).

Section 2.1: Please mention clearly in the text which aerosol meteorology interactions are switched on for each model.

Section 2.2: Please add some information on soil dust emissions (also to be included in Table 1).

Section 2.3: Please include some information about the meteorological boundary con-

C3

ditions.

Section 2.3: Please consider to add some information about the differences between the boundary values from the different data sources for selected variables (eventually to be included in the supplementary material). Looking into the boundary values may also help to understand differences between results of the different model runs.

Line 389: This sentence is confusing. According to Table S1 all simulations considered here are performed with aerosol meteorology interactions switched on. Were the simulations additionally performed also without aerosol meteorology interactions for the investigation of feedback effects? Anyway, this could be mentioned in the introduction as well as in the outlook, but not at this place.

Line 392 –393: Why are abbreviations given for temperature, humidity, and wind, and units for the shortwave radiation?

Section 3: Why is the year 2013 described here, if it not discussed in the rest of the paper? Figure 3 could eventually be moved to the supplementary material.

Section 4.1: Cloud optical depth and integrated liquid water are important and should also be discussed (even, if no observational data are available).

Line 427: This topic is not addressed in this paper.

Line 473: What is the reason for the bad performance of M6 and M7? Please check also cloud cover. And why is this huge difference in radiation between M6 and M7 not reflected in T2? Why does M7, which is also WRF, show such a large difference to M1 and M2?

Line 480: Why is this the case?

Section 4.2: Spatial distributions of the gas phase pollutants (similar to figures 8 and 9) could be shown also in the supplement.

Line 498: The 'only' is probably placed wrong here?

C4

Line 504: Please give some evidence for this.

Line 521: Please give more evidence for this statement. And what is the contribution of soil dust to PM10? What is the contribution of different of the dust emission parameterizations?

Line 524 – 526: What is the reason for the negative correlation? On an hourly basis, the diurnal course of ozone should reflect the course of the solar radiation. Or are the correlations just calculated from on daily values? Please clarify. How do the diurnal courses look like for the individual models? Could the parameterization of dry deposition or differences in the lateral boundary conditions or differences in biogenic VOC emissions explain the overestimation of simulated ozone for M3 and M4? It is surprising, that the overestimated ozone seems not to be related to solar radiation, so there are probably other reasons for this overestimation. How do the ozone profiles look like, can they contribute to an explanation?

Line 530: If the citation does not fit, it should be removed. Knote et al. (2015, Atmospheric Environment, Vol. 115) may probably suit better as this paper includes also a winter episode.

Sections 4.3 and 4.4: It would be nice if soils dust were also included in the discussion.

Lines 550 – 554: These general remarks are not necessary here.

Line 561: 'M5 and M6 shows ...': Please observe the proper use of singular and plural (not only here but throughout the paper, e.g. line 582).

Lines 562 – 578: It is not clear in how far these statements apply to the models which are discussed here.

Line 568 – 572: This sentence is quite hard to understand. Please split it into two or more sentences. Please also split other lengthy convoluted sentences.

Line 593: Is this just a general remark or is there some evidence for this? Does M7

C5

really include heterogeneous nitrate formation?

Line 608 and line 772: How can this be? Emissions were supposed to be the same for all models.

Line 609: Was the vertical distribution of the emissions not prescribed? If there are differences in the vertical distributions, they must be described.

Line 616: According to line 191, M2 does not include SOA formation ...

Line 616 – 617: The statement about SORGAM does not fit here as M1 includes a VBS approach (Ahmadov, R., et al., 2012, J. Geophys. Res.).

Line 619: 'volatile' seems to be missing here

Lines 619 – 624: These lines include quite general statements. How are they related to the models discussed in this paper?

Lines 642 – 643: This should have been mentioned in section 2.2.

Lines 669 – 672: Please avoid this kind of redundancies (not only here).

Lines 677 – 680: What is the contribution of soil dust during these situations?

Line 688: Please split this sentence.

Line 690 – 691: This statement is true, but unnecessary. Lines 692 – 798: These are quite general statements, which must be related to the applied models.

Line 710: M2 does not have modes ...

Line 712: What are the consequences of this? If necessary, this can be discussed based on the findings by Curci et al. (2015, Atmospheric Environment, Vol. 115).

Figures and figure captions:

Figure captions in general: More detailed descriptions must be given in the figure

C6

captions (are daily values or hourly values shown, relevant area, etc., depending on what is shown in the figure).

Figure 1: It looks like the labels M3 and M4 in the legend of Figure 1 are mixed up (According to Table 1 and Figures 8 and 9 M4 is the small domain). Furthermore, using the same colors for the model domains as for the curves in Fig. 5 etc. would make the reading a bit easier.

Caption of Figure 1: Repeating the model names in the caption (for example 'M1: WRF-Chem, 45 km; M2 WRF-Chem, 50 km, M3 ...') would make the paper a bit 'reader-friendly'.

Caption of Figure 5: Please mention that this is a spatial average (for a, b and c: over which area) of daily values and explain error bars.

Figure 7: Please show also CO and PM2.5 although no measurements are available.

Figures 8 and 9: Please include also PM10.

Figures 8 and 9: The split into two figures appears quite arbitrary. Perhaps it would look better if the figures are organized differently: One figure with 7 rows (M1 – M7) and 3 columns (PM10, PM2.5, BC) and one figure with 7 rows (M1 – M7) and 4 columns (SO_4^{2-} , NO_3^- , NH_4^+ , OC) – no obligation to do this, just a suggestion.

Spatial distributions of the gas phase pollutants (similar to Figures 8 and 9) would be nice in the supplement.

Figure S4: This figure seems to be contorted. Please improve the quality. Why are all the lines within the single 'height groups' (e.g. at '1 km') at different heights? Please explain in the figure caption. Also: explain what is shown here (daily values, hourly values, ...?)

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-731>, 2017.