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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Model evaluation and inter-comparison of surface-level ozone and relevant species in East Asia in the context of MICS-Asia phase III Part I: overview" *by* Jie Li et al.

Anonymous Referee #1

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General comments:

This paper presents overview about Phase III of the chemical transport model inter-comparison sutudy MICS-ASIA for East Asia region. The atmospheric models participating in Phase III and its simulation framework have greatly improved from the previous MICS-ASIA Phase II. And, the calculation results are compared with the observations in industrial China, which was not done in the Phase II. So, this paper introducing MICS-ASIA Phased III is believed to have certain academic value. However, in the manuscript at the present time, there are many problems such as the sentences being too long, and the lack of the necessary information to convince the authors' in-

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Discussion paper

terpretation to the results. Then, the manuscript should be revised according to the following comments as well as many other specific comments before the publication in ACP.

1. About the length of the manuscript

It seems that the manuscript is too long compared to its contents. The things to be claimed should be focused (probably on what is stated in summary or the abstract), and the descriptions not related to those should be removed or simplified. The figures or their contents which are not necessary for the main line should be also omitted.

2. On the comparison of model results and measured values

Most models have rough resolution (horizontal direction: 45 km, vertical direction: 58 m near the ground), and it is not shown whether the observed values to be compared represent the extent of that range. If many measuring stations are unevenly distributed in a grid cell at locations with high NOx emissions, the effect of titration there is greater than the grid cell average. So, actually the models overestimating the measured ozone concentration may be correct.

3. About the investigation of intermodel variability on O3ãĂĂ(chapter.4)

In phase II of the MICS-ASIA, because input data (weather, emissions, boundary condition) are different, it was not possible to specify how much each process of chemistry, vertical diffusion, and dry deposition in the model contributed to calculated ozone variation among models. In the Phase III of this time, although common input data were provided to avoid it, it seems in this paper that the contribution of each of the above processes could not be specified again because the post process of these data differs between models. If the above guess is true, it seems better to clearly state it and to give up the brute forth evaluation of the contribution of each of the above process in sections 4.3-4.5. On the other hand, if you stick to say that you could specify the contribution of each of the above processes, you should add thoroughly the information

described in the following so that the reader can understand its rationality.

4. About authors' interpretation of the results

Many parts can not be convinced about the interpretation of the results by the author mainly because the differences among each model (e.g., differences of boundary conditions estimated with Mozart, Chaser, and by default settings, differences in dry deposition model, differences in sub-grid scale parameterization such as convection, differences in PBL model, and differences in spatiotemporal distribution of emissions) are not specifically mentioned. For relevant parts other than chapter 4, I will point out each of the following "other specific comments"".

Other specific comments:

p.5 L2-3

Is the problem (3) really addressed ? I don't think so, as I already mentioned in the general comments.

p.5 L10-11

You mean to interpolate model outputs to locations of observations both horizontally and vertically ? If yes, please show that method in detail. It may get rid of my concern mentioned in the general comments.

p.5 L24

Fig.1 does not introduce WRF model.

p.6 L28-p.7 L1

Please identify which model adopt the projection by themselves.

p.7 L5

I think two references should be moved after the names of the universities are introduced in L6.

p.7 L9

Are the models making boundary conditions depending on their own previous experience denoted by "default" in table 1 ? If yes, I think the phrase such as "their own" is better in table 1.

p.9 L4

Is the word "total" necessary ?

p.9 L5

M12 seems also an exception as well as M11.

p.9 L11-12

Is a two-peak seasonal cycle for O3 ? If yes , I see there are three peaks but not two. And I see observations show three-peak but not one-peak.

p.9 L22

"Similar results have been found in MICS-Asia II" seems contradict to the statement in L5-L7 of p.4.

p.10 L24-25

Show the evidence for the slight overestimation of 10 ppbv in M11 due to difficulties in dealing with vertical mixing.

p.10 L25-26

Show the evidence for the significant improvement of the model performance in winter, compared to in summer, due to the weak intensity of photochemical reactions.

p.11 L17

Add explanation how to derive the statics in table 2, 3 and 4 to clarify which part of the spatiotemporal deviations from the observations are included in the statics.

p.12 L12-13

Show the evidence for that the treatment of models on chemistry, vertical diffusion and dry deposition have contributed to the underestimation of NO.

p.13 L8-10

I can't understand why you selected the PBLH, emissions fluxes, dry deposition velocities, relationships between NOx and O3, amd the vertical profiles of O3 and its precursors to compare.

p.16 L23-L24

Jin et al (2015) perhaps showed the ozone formation regime at 1330 LST (overpass time of OMI) while you show that between 1000-1800 LST. Also, your results includes NOx titration effect while Jin et al (2015)'s results did not. So, I think it is not appropriate to compare them directly.,

p/17 L8-9

In M11, O3 does not seem positively correlated with NOx.

p.18 L17-18

Show the evidence that difference of concentrations are related to the treatments of convection and cloud activity among models.

p.19 L22-23

The locations of the place names shown in the text are not known for the foreign readers. So you should show these place names in Fig.10.

p.20 L16-17

Before you have the statement in L16-17, you should show that the wind fields are actually the same between the models which estimate 30 ppbv or higher O3 mixing ratio and those which estimate lower O3 mixing ratio. And, how do you think about the

difference of emissions that was discussed in section 4.2

p.33 L9

I guess the meteorological model used for providing meteorological fields with most models also use the domain in Fig.1. If yes, please mention about that too.

p.33 L14

Please add a description of the symbol such as "+" or "-" in Fig.2.

p.46 Fig.3 and p.47 Fig.4

The kinds of color of the curve in the figures is too many to distinguish. Are all the models need to be distinguished by different colors ?

Technical corrections:

p.3 L15

You need space between "2013" and "(Wang et al., 2017)". You can find the similar mistake to miss spaces elsewhere in the manuscript.

p.10 L4

"4)" should be removed.

p.19 L23

I think "predicated" should be "predicted".

p.20 L1

"EA1" should be moved right after "source regions"

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