

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 #2

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This paper describe the ability of an ensemble of regional chemistry-transport models to reproduce surface ozone pollution in East Asia as well as NO_x concentrations. Indeed, recent observations do show that surface ozone concentrations are still increasing in China which underline the necessity to have good forecasting tools and means to set-up and control mitigation policies. This intercomparison is conducted in the framework of the Model Inter-Comparison Study for Asia phase III (MICS-ASIA III) which is the follow-up of MICS-ASIA II (2003) and MICS-ASIA I (1998). 13 models are cross compared for a one year simulation (2010). The simulation suits are based on state-of-the-art CTMs. Simulations are compared to available observations with especially observations available on industrialized China which was not the case of

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MICS-ASIA II. Also the dispersion of the simulations are investigated to understand what reasons could explain models differences. Compared to European or American areas, the models have more difficulties to reproduced observed concentrations and the median of the ensemble do not always over skilled single models like it is the case for European ensembles. Such exercises have been proven useful to improve modelling suits and for this reason this paper is interesting for the community. The work conducted in that case is important and this study deserved to be published in ACP journal but corrections are probably needed to make the paper more efficient and to fulfill the high level standard of quality of the journal. I will list the comments and questions I still have on this work and that could help, i hope, to improve it. 1/ The analysis of the skills of an ensemble is always complicated. To be more clear and to have stronger messages, i suggest you to first analysis skills using the average of the ensemble and then to discuss the single models. By this way, it will allow to clearly identify the main biases either for seasonal analysis either for diurnal analysis and then discuss singularities . 2/ Maybe also it would nice to have a more explicit but still short reminder of the physical processes driving the variability in each sub-region (i.e late maxima of ozone in EA3 quite different than EA1 and even EA4). 3/ More informations about the nature of the stations and specifically about their representativity is needed. It is a key element of the model skills. Also for NO₂ it exist sometimes biases (especially for stations far from sources) in the measurements when using molybden convertors devices since all nitrogen oxydes are measured instead of just NO₂, do you have checked this ? 4/ I have the impression that authors do not need to include the EA2 region in the paper, you never use it in your discussions. 5/ Authors do evaluate several parameters relevant for model evaluation. It would have be better to have observations to put against models. It is often complicated to get all needed observations but maybe you can at list mention that in the prospectives. It become possible to have network ceilometers for PBLH evaluation. A lot of satellite observations are available to evaluate NO_x or ozone at larger scales. What about vertical profiles ? Other comments etc . . . Page 3-
Line 7 – Please remind the value of the threshold Page 10 - Line 4 – Please suppress

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“4)” Page 10 – Line 18 – A good example where using the ensemble average allows to better structure the discussion and to be more precise on the model skills. Page 10 – Line 24-25 – “. . . due to difficulties in dealing with vertical mixing”: how do we know that ? Page 12 – Line 16 – How statistics are calculated ? on hourly values ? Page 13 – Line 16 – Why choosing a sub selection of models ? It would be interesting to have all models. Page 14 – Line 3 – Von Engel n o ? Page 14 – Line 7 – You do not discuss VOC emissions. Would you suggest that models have no sensitivity to these emissions ? Page 14 – Line 15-20 – The discussion and the links between arguments are not that clear. Page 14 –Line 22 – I would say “net sink” since chemistry is a much higher absolute sink than deposition. Page 16 – Line 4 to 6 – Seems contradictory to have a small sink with considerable effect on oceanic surface. I would rather say that even if dry deposition velocities are small over oceanic surfaces, the impact of dry deposition over ocean is globally important because of the large surface ocean are representing. Page 16 – Line 6-8 – Why can we do the assumption that dry deposition is specifically important for EA4 ? Page 17 – Line 1 – I observe that range of concentrations for O3 and NOx can be very different between models but it is not clear if slopes are that different. Page 18 – Line 2 to 5 but also Line 7 to 20 – The variability authors are mentioning is not clear from figure 9. Also for differences between winter and summer, we need to have numbers to better evaluate this variability. Page 18 – Line 5-6 – Authors do have this information, it should more than an suggestion, no ? Page 19 – Line 8 – 9 – It is mention that dispersion between models is higher here than for the European case and authors suggest the models do not represent uncertainties, could you develop ? Also authors mention that key processes could miss, what kind of processes are they thinking to ? Page 20 – Line 11 to 15 – Do we observe same differences for higher levels ? Maybe in some models plumes are also present but at different altitudes. Page 21 – Line 2 – I’m not sure that author do define mathematically the coefficient of variation. Page 21 – Line 13 – Like in table1 authors do mention that “default” is used as boundary conditions. Default values should be more clearly defined ? climatology ? from where ? Page 22 –Line 7 – “. . its relevant species ..” I also see VOC or even

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radicals as relevant species for the tropospheric ozone cycle then it is better to mention O3 and NOx instead.

About Table and Figures Table2 – Maybe it is mandatory to mention how statistical indicator are calculated (i.e formula). Be careful “suqare” in the title instead of square. RMSE do have units, please mention it. Figure 1 – as mention earlier I would have removed EA2 that is not discussed. Figure 2 – probably too small as it is. The full black line does not seems necessary. Figure 9 – Maybe it is possible to reduce horizontal scale down to 10 ppb to have more space on the right and to better evaluate the ensemble dispersion. Figure 10 – Maybe too small also Figure 11 – Same as Figure 10

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