

# ***Interactive comment on “Discrepancies between MICS-Asia III Simulation and Observation for Surface Ozone in the Marine Atmosphere over the Northwestern Pacific Asian Rim Region” by Hajime Akimoto et al.***

## **Anonymous Referee #1**

Received and published: 30 April 2020

The paper presents the results of an analysis and identifying the causes of overestimate of the surface-level O<sub>3</sub> mixing ratios generated from among three models, and also compared them to the EANET observations at Oki, Hedo, and Ogasawara in July 2010 in the context of MICS-Asia.

They derived discrepancies between simulations and observations for surface ozone mixing ratios in the marine atmosphere, and as well, correlate high ozone simulation results with 3 factors: (1) Long range transport, (2) Local photo-chemical production, and (3) Dry deposition velocities. They explain the high estimate of ozone can be

Printer-friendly version

Discussion paper



derived by lower dry depositions over marine surface, together with some different simulations over the coastal or near-marine area which is also relevant to the unsuitable dry deposition velocities. Reading the present manuscript, a well-expected correlation between deposition vs. ozone mixing ratios, were presented and organized here as a major conclusion, and also explore new aspects to gain fresh insights through MICS-Asia conference program.

However, several sentences are unconvincing to me, and it is not clear to me, why only choose those periods because it should be based on the overall general or a year-round period to secure the generality of characterization. In this sense, this conclusion needs to be very carefully characterized and the re-analysed according to the separate specific events and their differing conditions. Here are my concerns for this manuscript.

(Major comments)

(1) Under- or over-estimation by model is presumably caused by very complicated factors in the regional CTM models. Authors employed two models with different versions of CMAQ. I thought it may make sense to compare it to a completely different and diverse models, such as the CAMx or WRF-Chem or GEOS-Chem model. This is because, as we do not know the “true” values of dry deposition velocities, and thus authors should open to the different possibilities of other controlling factors. For example, as depicted in Fig. 8, NAQM showed considerably lower ozone mixing ratios over the whole domain than CMAQ, which in turn could derive the relatively lower ozone than CMAQ over Hedo and Ogasawara as well (it would be quite natural to me), rather than pointing out relatively higher dry deposition velocities than CMAQ. As authors are aware, models employ their different dry deposition parameterizations, mostly generating different values.

(2) As authors described in page 7 (Long range transport of O<sub>3</sub>), Oki is influenced by O<sub>3</sub> inland area (and thus mostly urban O<sub>3</sub>). Probably CMAQ which usually simulate

[Printer-friendly version](#)[Discussion paper](#)

“higher-than-observation” ozone mixing ratios over inland, and CMAQ model improvement over “land” area may automatically remove ozone biases in “marine” area where authors claim that dry deposition velocity should be higher.

(3) (Line 355-375) It is also confusing that, among three models, NAQM reproduce ozone well over Hedo and Ogasawara primarily due to the higher dry deposition velocities than those of CMAQ. However, in the case that dry deposition velocities of Bohai Bay and Yellow sea have to be raised, then Oki will be expected to be also down, but ozone will be also down simultaneously in Hedo and Ogasawara where originally there were no biases of ozone mixing ratios. I guess increasing dry deposition velocities in model NAQM over Bohai Bay and Yellow sea will not satisfy both.

(4) Finally I recommend to secure some more simulation cases. Authors used at least global model for initial condition such as GEOS-chem and CHASER: dry deposition velocities from two global models vs. over- (or under-) estimation of ozone mixing ratios can be also useful to justify authors’ conclusion from multiple cases. Or in some cases, authors may reach a different conclusion (i.e., model internal errors instead of different dry velocities).

(Specific comments)

1. Specify the dry deposition parameterizations (with references) for all two (or three) models. 2. During July 23-26 in Fig. 3(c), observation of ozone in Ogasawara showed strong diurnal variations with big differences between max. in daytime and min. in night time. Should there be a photo-chemical reactions? because Ogasawara is thought to be a real background site: it has nothing to do with both local photo-chemical formation and transport of NO<sub>x</sub> from other areas I guess. 3. Line 230. .. three models reproduce observation reasonably well.. It is confusing because, in the previous sentences, only NAQM’s results matches well with observations in Ogasawara. 4. Please indicate the locations of Bohai Bay and Yellow sea areas. 5. is it possible to analyze the case for different periods like May or June where there are high ozone mixing ratios with

(or without Long-range transport processes) over East Asia?, because just one single month test might have a possibility of sometimes misleading the conclusions.

---

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-228>, 2020.

ACPD

Interactive  
comment

Printer-friendly version

Discussion paper

