Review of the manuscript titled "Discrepancies between MICS-Asia III Simulation and Observation for Surface Ozone in the Marine Atmosphere over the Northwestern Pacific Asian Rim Region"

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In Comparison with the previous manuscript (MS), I noticed that there are big improvements for interpreting the O3 modeling results against observations. Nevertheless based on three referees' comments and respones, I found a couple of deficiency problems with their responses. I think if the authors make a couple of additional analysis for quantitative discussion, the current manuscript will be much more improved. (I guess others raised by three Referees were now mostly answered by authors). The primary deficiency problem is that the authors haven't fully answered Referee3's question yet, and I agreed on the raising the question (by Referee 3), regarding marine boundary layer (MBL) fluctuations (i.e., how high the MBL is in meters) association with LRT vs. local photochemical reactions at Oki. Authors need to rule out the possibility of errors in how well model simulated MBL heights against measurement (if multiple models would not be possible, just 'one remote (island) measurement against a single model approach would be enough for this purpose). This will make the role of the surface dry deposition be emphasized more.

Another deficiency is the sensitivity result (i.e., applying doubling and decupling of dry deposition velocities : it would be also probably possible to carry out based on just a single model (or employing a simple box model and note the results briefly), rather than only employing MICS-Asia III frame. This is because probably authors check to what degree the diurnal \triangle O3 variations (i.e., daily O3 max. - daily O3 min, as indicated in Fig. 3a and 3b at Hedo and Oki) might become observed levels. This process will check the long-term levels of O3 as well as model's local photochemical reaction build-up capability and in turn authors may reach the conclucion in more robust way.