

Interactive comment on “An evaluation of O₃ dry deposition simulations in East Asia” by R. J. Park et al.

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(1) Review of Park et al., An evaluation of O₃ dry deposition simulations in East Asia
This study evaluates and compares two dry deposition schemes used for model simulations of atmospheric ozone. Considering that dry deposition is a very important process affecting ozone levels and also the fact that relatively little studies in the literature have focused on ozone dry deposition, I think this is a topic suitable for publication at ACP. I would recommend publication of this MS after the authors address some relatively minor issues – 1. L194-196, “Herein, we only considered the measured dry deposition velocities in the range 0 to 2.0 cm s⁻¹, which is a typical O₃ dry deposition velocity range in the literature (Padro, 1996).” So do you mean that the measurement

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data for dry deposition velocities outside of this range is unrealistic? I think better clarification/justification is needed here.

→ We adopt the new criteria to select the observed values based on the previous literature as follows:

Following the previous observation studies (Matsuda et al., 2005; Tsai et al., 2010), we used values only for a case in which 1) the ozone concentration was greater than 1 ppbv, 2) the surface wind speed was greater than 1 m s⁻¹, and 3) a computed value was less than the maximum ozone dry deposition velocity defined as $1.5 \times (R_a + R_b) - 1$.

(2) L275-282 on the discussion of the feather that the largest differences in O₃ dry deposition velocity over the continents do not result in the largest differences in ozone concentration there but rather over the downwind ocean, the authors attribute this to the efficient export of O₃ from the polluted continent. I think it would be more clear if the authors can show the ratio plots for O₃ concentrations as well as the dry deposition velocities (i.e. similar to Fig. 1c and 3c, but shown as ratios instead of differences). In addition, I'm thinking perhaps the changes in O₃ precursors (I assume the two dry dep. schemes were applied to not only O₃ but other species in the model) also contribute to this?

→ Following the suggestion, we made ratio plots as shown below, but decided to keep the difference plot in the manuscript because the ratio plot shows too much saturation over the ocean owing to the low values in the denominator. In addition, the absolute change of ozone can provide quantitative information for the ozone air quality criteria and thus be more useful we believe.

→ As to the export of ozone precursors, their contributions are relatively minor compared with the direct ozone export and we added this sentence in the revised manuscript.

(3) L37-38, “the model was considerably sensitive to the input parameters, which indi-

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icates a high uncertainty for such O3 dry deposition simulations.” –It is unclear to me how can we reach this conclusion, please clarify here.

→ We rewrote the sentence for clarity as follows: However, we find significant changes in simulated ozone concentrations using the Wesely scheme but with different surface type datasets, indicating the high sensitivity of ozone deposition calculations to the input data.

(4) Some edits/proofreading is needed for this MS; I only tried on the 1st few pages as shown below for examples, but I think the authors need to check throughout their MS – 1). L45 O3 tropospheric => tropospheric O3 2). L65 air quality sources => air pollution sources 3). L76 in the most widely => in two of the most widely 4). L79 as well as two => as well as the two 5). L85 – assessing the spatial and temporal distribution of O3 and the contribution from a specific source 6). L233-235, “The dry deposition velocity domain mean difference between the two methods is 0.14 cms-1, which is 1.4 greater than the M3DRY method domain mean dry deposition velocity (0.10 cm s-1)” This sentence is very confusing and need rewriting. 7). L261. 4. O3 concentration spatial and diurnal patterns in East Asia => spatial and diurnal patterns of ozone concentrations in East Asia 8). L557 Table 1 lists not only “Physics” parameters but also Chemical mechanisms, so I would suggest changing to “Model set up for the WRF-Chem simulations”

→ We rewrote all the misused words and had a native speaker check out our manuscript to improve the use of words and to correct grammatical errors.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 919, 2014.

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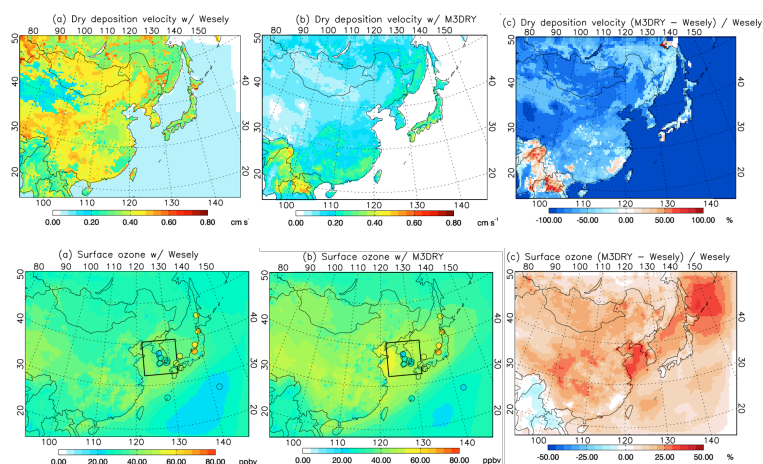


Fig. 1.

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