

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

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This manuscript has some serious problems that lead to very misleading conclusions. The manuscript reports that the dry deposition model in WRF-Chem based on Wesely (1989) produces more accurate ozone dry deposition velocities than the M3Dry model that is used in the CMAQ model. This conclusion is based solely on Figure 2 that shows hourly measured ozone dry deposition velocities from two field studies in Colorado and model results from Wesely and M3Dry. There is extremely little explanation of this comparison. Are these results averaged over many days or weeks? For what time periods? Are the time periods the same for both sites? How were the model results produced? The only modeling discussed is for WRF-Chem applications in Asia. Are

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the results shown in Fig 2 from WRF-Chem in Colorado? Are they box-model results using measured inputs, if so, from which site? These sites are about 100 km apart and are presented as if they are directly comparable. All these questions need to be answered if this Figure is to be taken as evidence of model performance.

Another major problem is the lack of explanation about how the M3Dry is implemented in WRF-Chem. The article cited for M3Dry (Pleim et al 2001) presents the dry deposition model as part of a coupled land surface model (LSM) for meteorology and chemistry. This article, and a more recent article by Pleim and Ran (2011), both explain that a key advantage of M3Dry over stand-alone dry deposition models such as the Wesely model is that the stomatal conductance and several other parameters are used directly from the LSM in the meteorology model. In this way, the stomatal pathway for dry deposition is proportional (scaled by the ratio of the chemical diffusivity to the diffusivity of water) to the stomatal conductance used to compute transpiration for surface moisture flux in the meteorology model. In the most recent model versions, dry deposition velocities are computed in CMAQ (M3Dry was removed from MCIP in 2011) using stomatal conductance that is output from the meteorology model. Thus the stomatal pathway for dry deposition in M3Dry is as good or bad as the stomatal pathway for evapotranspiration. If the stomatal conductance is not realistic, the meteorology simulation will not be accurate.

Prior to 2012, MCIP did include an option for a standalone stomatal conductance calculation for use if this parameter was not available from the meteorology model output. This option was never used at the USEPA (the developers of CMAQ) since stomatal conductance is a standard output from WRF when using the Pleim-Xiu LSM. The stomatal conductance from other LSM options in WRF can also be used. Since this manuscript does not explain how the stomatal conductance was computed for the WRF-Chem modeling presented, I guess that the alternate standalone stomatal calculation was probably used. This is not the preferred way to apply M3Dry and is in fact no longer available in recent CMAQ versions. I strongly suggest that the M3Dry

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be applied as intended using the stomatal conductance and other parameters such as aerodynamic conductance from the LSM in WRF. This should not be difficult since WRF-Chem is an online met-chem model. The results shown here provide no valid basis for concluding that either M3Dry or Wesely are better for calculating ozone dry deposition velocity. If, as I suspect, the stomatal conductance for the M3Dry model runs did not use the stomatal conductance from the WRF LSM (in this case Noah), the results are not representative of the model's typical performance since it used an option that is not recommended, never used in EPA applications of CMAQ, and is no longer supported or available.

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