

Interactive comment on “Simulating ozone dry deposition at a boreal forest with a multi-layer canopy deposition model” by Putian Zhou et al.

Anonymous Referee #3

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The paper represents an effort to model ozone sinks into a boreal forest canopy. I appreciate the attempt to go beyond the traditional big leaf models in order to explain different O₃ sinks driven by turbulence and energy balance at different levels. The article reads well and confirms previous finding that relevant stomatal sinks occur during the day while chemical reactions with VOC are important mainly during the night time. However, I would have appreciated a more extended parameterization and a better description of the model in order to clearly understand the formalism adopted to predict energy balance terms. There are some arbitrary choices of parameters, and not a convincing analysis of sensitivity or results from a model calibration. A table showing results from a sensitivity analysis should be provided. Basic questions like: what could be the effect of an increase in air temperature and precipitation regimes on ozone deposition? Are not resolved, although it would have been nice triggering the

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model for some predictions of Ozone deposition under future environmental changes. In general the paper lacks of more mechanistic explanations of the results, with more discussion on the possible drivers of dry and wet ozone deposition.

Pag 2 line 25: You mention again that dew on leaves can increase deposition, but could you spend two lines mentioning the reasons or hypothesis why a hydrofobic molecule reacts so fast on wet surfaces? Pag 3 line 10. What about NO_x emitted from soils? Couldn't fast reactions between O₃ and NO lead to high O₃ fluxes in the sub-canopy region? Pag 3 line 34: Only one month to test the model? The relative contributions of O₃ sinks changes a lot during the seasons in response to air temperature and plant phenology. It is a pity that such an important modelling effort is limited to one month, I would extend to the all vegetative season. Pag 5 line 5: Extensive research has been conducted in Yttiala to refine turbulence limitation to flux measurements. Why should we expect an ustar threshold different from other scalars measured at the site? Pag 6 line 20: do you have experience of subcanopy O₃ fluxes so that you can better parameterize soil resistances? It seems here that usage of one value rather than another is arbitrary and not properly calibrated. Pag 7 line 15. So you mean that K_t has been estimated from measured fluxes? Or in which other way? Reading through the manuscript I feel like the description of the model is not accurate, and more informations should be provided. Pag 19 line 15: Can you say that NO_x are also not relevant in the boreal forest? Pag 20 line 11: Since the Stomatal resistance is calculated based on evapotranspiration, are you sure that relevant nocturnal soil evaporation does not contribute significantly to R_c? Have you tried to separate canopy transpiration from soil evaporation in the model?

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