

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2021-157

Anonymous Referee #1

Referee comment on "Improvement of stomatal resistance and photosynthesis mechanism of Noah-MP-WDDM (v1.42) in simulation of NO₂ dry deposition velocity in forests" by Ming Chang et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-157-RC1>, 2021

Improvement of stomatal resistance and photosynthesis mechanism of Noah-MP-WDDM (v1.42) in simulation of NO₂ dry deposition velocity in forests presents results of different model mechanisms for representing stomatal deposition of NO₂. The authors conclude that they substantially improved upon the earlier Noah-MP-WDDM version and also assert that canopy stomata and leaf nitrogen-limiting mechanisms from various classic models cannot well express the diurnal changes in stomatal deposition.

This work is interesting and I recommend publication only following a reworking of the scope and conclusions of the manuscript. In my opinion the conclusions that the authors have substantially improved upon the earlier Noah-MP-WDDM version and that the results "emphasize importance of the canopy stomatal carbon dioxide compensation mechanism and the GPP-controlled leaf nitrogen-limiting factor for the simulation of nitrogen deposition" are overstated. The more interesting finding is that all classic model mechanisms do a fairly bad job at capturing the stomatal deposition of reactive nitrogen at the chosen site in China, indicating there are substantial gaps in our current understanding of in-canopy processes. This paper would be substantially improved with an expanded discussion of what those knowledge gaps are, and how this current study is able to identify areas where more research is needed. Although I think certain findings of Chang et al., are significant, they should also be better placed in the context of recent publications. I would like to see more discussion of the results of this paper in comparison with other models, observations, and laboratory findings. The lack of discussion as written limits the value of this current study to the wider scientific community.

Specific comments:

Additional proof reading of the manuscript is needed.

L49. what is meant by this?

L64. I would like to see some additional citations here of modelling papers that represent Rc. (e.g. Wolfe et al., 2011, Delaria and Cohen 2020, Simpson et al., 2012, Ganzeveld et al., 2002, etc.).

L 70. Would be good to discuss findings of the paper cited here, as the finding of Delaria and Cohen 2020 seem to tie in well with the purpose of this paper. This sentence as written just summarizes the introduction section of Delaria and Cohen 2020.

L 72. It seems you have missed a few recent papers that also support this finding and further discuss the compensation points and the roll of nitrogen availability on NO₂ uptake (Delaria et al., 2018 and 2020). Place et al, 2020 may also be interesting for you to look at.

L101: why is it a better mechanism? Need citation for this? In what way is it "better"? Is this backed up by observational data?

L106: One might argue that these "key plant physiological parameters" result in model overparameterizations are too species- and possibly individual- specific to be useful at a regional scale. How may spatial and species deviations in these parameters introduce uncertainties into your conclusions? What advantage or disadvantage does this have compared to the Jarvis model?

L115. Rns is not defined. Please define.

L 146: How is it simplified? Why is this simplification not ideal?

L150: I am confused. Are you stating that the nitrogen leaf content changes just the photosynthesis rate, or changes the relationship between G_s and nitrogen V_d ? I would recommend looking at Delaria et al. and Place et al. 2020.

L198: Observations of deposition velocity are difficult and subject to different uncertainties based on observation method. How is V_d calculated in your observation data? What uncertainties may be present in the observation data? Citation to data?

L208: Discussion of species present in the site considered is needed much earlier in the manuscript. Are you using species-specific parameters? If so, where are these parameters from and are there experimental data to support?

L214: Based on figure 2, even this scheme seems to have high stability of parameterization. It looks that it may even be anti-correlated with observed deposition velocity. From Figure 2 I would conclude that all mechanisms are bad at representing deposition velocity. It seems the goal of representing changes in V_d with different environmental conditions has failed, although diurnal cycles are captured seemingly better. I am curious what this data look like if you separate into different times of day (eg daylight hours only).

L225: I would like to see a time series of data over multiple days. Does your model not capture this turbulent exchange effect?

L281. I don't think you can conclude from the results presented that the new v 1.42 is much improved. I don't see much evidence in the diurnal cycles that the agreement with observations is significantly (statistically) different, and there doesn't seem to be much evidence that would allow you to assert which model scheme is "best". The important conclusions from this work are in lines 287-291.

Figure comments:

Figures 4---7: How are errorbars calculated?