Reply to 'Comment on gmd-2021-157'

August 24, 2021

The comments from referees are colored black.
The authors' replies are colored blue.

RC1: 'Comment on gmd-2021-157', Anonymous Referee #1, 05 Jul 2021

Improvement of stomatal resistance and photosynthesis mechanism of Noah-MP-WDDM (v1.42) in simulation of NO_2 dry deposition velocity in forests presents results of different model mechanisms for representing stomatal deposition of NO_2. 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.

The authors would like to thank anonymous referee RC1 for the valuable insight. We very much agree with the referee's summary of the work's achievements and noteworthy findings. The authors are very supportive of the referee's opinion that there are substantial knowledge gaps in our current understanding of in-canopy processes. The authors also think that the present deposition modules coupled in the CTMs are lacking in simulating the processes and characteristics of the canopy surface.

In previous work, the authors noticed that the first version of the Noah-MP-WDDM model could not consider and express more precisely the stomatal resistance and leaf nitrogen physiology. Therefore, in this model description paper, the authors tried to combine several classical mechanisms with the previous version of the Noah-MP-WDDM model and carry out an elementary comparison of their performance.

As the referee mentioned, these mechanisms can not effectively solve the problem of inaccurate deposition velocity simulation. The authors are aware of this and agree with the referee. The current work is just the beginning in comparing different mechanisms in the Noah-MP-WDDM model, and the authors expact to identify areas where more research is needed based on the current attempt. We have reworked the scope and conclusions of the manuscript following the referee's suggestion. The authors have also included a the work plan for the next phase of the performance comparison with other deposition models and on other observation samples or land surfaces, as explained in the conclusion.

In addition, because we are not native speakers of English, the authors overstated language such as "emphasize importance," "best," and "better" in the first manuscript. Based on the suggestions of the referee, we have modified all such expressions in the hope of describing the results more accurately.

The specific responses to the reviewers' comments are as follows:

Specific comments:

Additional proof reading of the manuscript is needed.

L49. what is meant by this?

Thank you for pointing this out. What the authors mean here is that the calculation of these two resistances in different deposition mechanisms follows similar principles. In order to avoid ambiguity, the authors changed this sentence to the following: The calculation of these two resistances in different deposition mechanisms follows similar principles (Finnigan, 2000).

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.).

Thank you for your advice. We carefully studied these references, cited them, and expanded the original description.

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.

Thank you for your advice. Yes, the findings of Delaria and Cohen 2020 have important enlightening value for all existing land surface models. We tried to quote and tentatively comment on their findings; please see lines 75 to 79.

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 NO_2 uptake (Delaria et al., 2018 and 2020). Place et al, 2020 may also be interesting for you to look at.

Thank you for your suggestion. It is true that we had not read these two articles you mentioned when we started to improve the Noah-MP-WDDM model. Based on your suggestion, we have carefully read them. Their work is very in-depth and instructive for the discussion of nitrogen compensation points, and it is worthy of our consideration in further Noah-MP-WDDM model coupling work. Unfortunately, when we tried to read the code corresponding to the articles, we found that it was written using the Matlab language, and our model is based on the Fortran language, which makes it difficult for us to join and couple in our current work. It is expected that the code will be reconfigured in future versions so that it can be incorporated.

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

Thank you for pointing this out. The authors agree that this expression is inappropriate, and it has been modified in the revised manuscript. The previous expression was based on the fact that the Ball-Berry scheme is based on experiments with net photosynthetic rate and stomatal conductance, while the Jarvis scheme is an empirical model of continuous multiplication that does not consider the physiological significance of parameters (Wang and Wen., 2010). In addition, although the Ball-Berry type stomatal resistance scheme behaves very similarly to the Jarvis type in modeling transpiration, the former scheme allows a direct coupling of terrestrial water and carbon fluxes and improves the simulation of vegetationatmosphere interactions (Niyogi et al., 2009, Yang et al., 2011). We tried to modify the entire subsection with more accurate expressions, avoiding irregular expressions such as "better" and "key." Please refer to lines 108 to 115 of the revised manuscript.

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?

Thank you for your advice. Indeed, as you said, over-parameterization is prone to occur in the process of model characterization, especially on the regional scale. However, the problem with the original Noah-MP-WDDM model we used and improved is its insufficient ability to characterize the physiological processes of vegetation, rather than over-parameterization. At present, the parameters we use are still the default parameters in Noah-MP's lookup table, and no experiment has been performed on its uncertainty. This suggestion is worthy of further work. There is indeed a risk to model performance due to parameter uncertainty. In line 112, the authors added a statement that these parameters may cause uncertainty, according to your suggestion. In addition, the authors believe that it is necessary to conduct a systematic assessment of the uncertainties caused by these parameters when we couple the current single-point Noah-MP-WDDM model into the regional model in the next step.

L115. Rns is not defined. Please define.

Thank you for pointing this out. The Rns is non-stomatal resistance. The definition was add to line 122 of the manuscript.

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

Thank you for pointing this out. As described in the subsequent line 137, although the default version of the NoahMP model sets a nitrogen-limiting factor, its calculation comes from two constants that make it impossible to characterize the changes in the nitrogen concentration on the leaf surface. Similarly, other land surface process schemes coupled in climate models and chemical transport models are basically the same, such as Noah, SiB, BATs, P-X, etc.

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

Thank you for pointing this out. According to the results of Zheng S. and Shangguan Z. (2007), for different types of vegetation, the effects of leaf nitrogen content have obvious differences in vegetation saturated photosynthetic rate (P_{max}), photosynthetic nitrogen use efficiency (PNUE), and quantum PS II electron transport (ϕ_{PSII}). We quoted this in the original text to illustrate the possible impact of over-simplification by setting the leaf nitrogen content of different vegetation to a constant parameter in the Noah-MP land surface model. At the same time, the authors also recognize the results discussed in your suggested article, which point out that the relative importance of chemical sinks and deposition will vary with NOx concentration. The authors greatly appreciate the reviewer's recommendation of Delaria et al. (2020), which is worthy of repeated study and quotation. We added a reference to it in the revised manuscript; please see line 160.

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

Yes, the authors agree very much with your point of view; it is very difficult to carry out deposition observation. And there are different uncer-

tainties based on observation method. However, in this paper, the authors main purpose is to use observations as the basis to explore differences in the simulation of deposition velocity by coupling different stomatal and photosynthetic mechanisms in the Noah-MP-WDDM model. The observed deposition velocity data used in this work came from Zhang et al., 2017, who also discuss on the sources of uncertainty in the observation itself. Vd was measured using the aerodynamic gradient method, which uses the concentration difference between the canopy interior and canopy exterior (Wu et al., 2015). The NO_2 concentration was measured using the NO_2 analyzer (Model T200, Teledyne-API, USA). The inspection instrument Model T200 used for observation had to run in a ventilated environment, so that the gas that had been assayed in the chamber could be discharged in time to ensure the accuracy of the next sample test results. But the instrument was limited by the conditions surrounding the flux tower; the assayed gas had accumulated (especially at night) in the reaction chamber, resulting in a partial (nocturnal) high observed value. In order to improve the reliability of the data, Zhang et al. excluded the abnormally high nighttime observation. Besides, the wind speed inside and outside the canopy is different and shows significant diurnal variation. During stable nighttime conditions, the uncertainty of the observation comes from the lack of turbulence. In order to ensure the reliability of the data, the observation data with small u* (smaller than 0.15 m s^{-1}) were also excluded in Zhang's study.

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?

Thank you for your advice. For the model, the characterization parameters of forests are indeed very important. In this paper, the authors use the default lookup table of vegetation parameters from the Noah-MP land surface model, and do not specifically modify the parameters with the localized values of the observation area, which may also be a source of poor simulation performance. Thank you very much for this inspiration. We included this point of view in the corresponding position in the revised version of the paper. Please review it.

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 Vd 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).

Thank you for pointing this out. First, as you said, based on the results in Figure 2, if you look at the simulation and observation results at all times together, all the mechanisms of the current version of the Noah-MP-WDDM model perform very poorly. However, from the characterization of the simulated diurnal variation in Figure 4, part of the simulation mechanism can show the relative day-night variation of the dry deposition velocity. It is worth emphasizing that in this manuscript, the authors are concerned about improving the function of the previous version of Noah-MP-WDDM and do not expect to solve all problems in one fell swoop. Second, the authors very much agree with the reviewer's suggestion of separating day and night. We tried to extract the results just during the day and the results just at noon. The results are shown in the figures below. As a supplement to Figure 2, it can be seen that for the deposition velocity during the daytime, the results do not show an obvious difference with the all-time results. But the just-noon results are seemingly captured better, and the differential performance of different test results can be displayed more clearly.

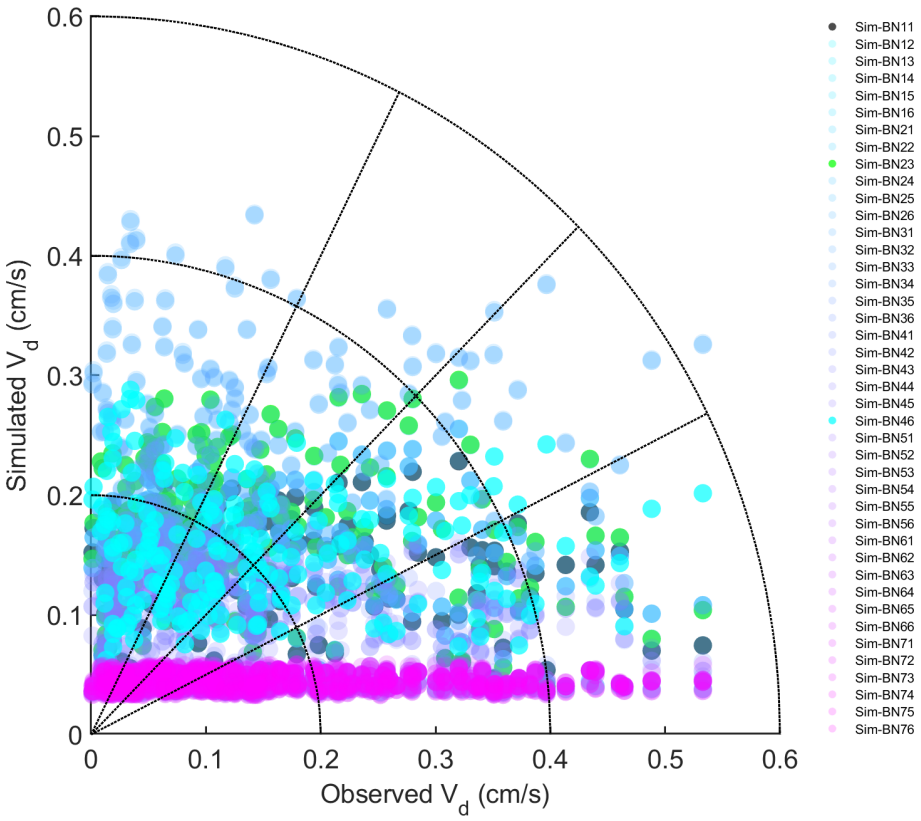

Figure 1: Comparison of observed and simulated V_d of NO_2 during the daytime

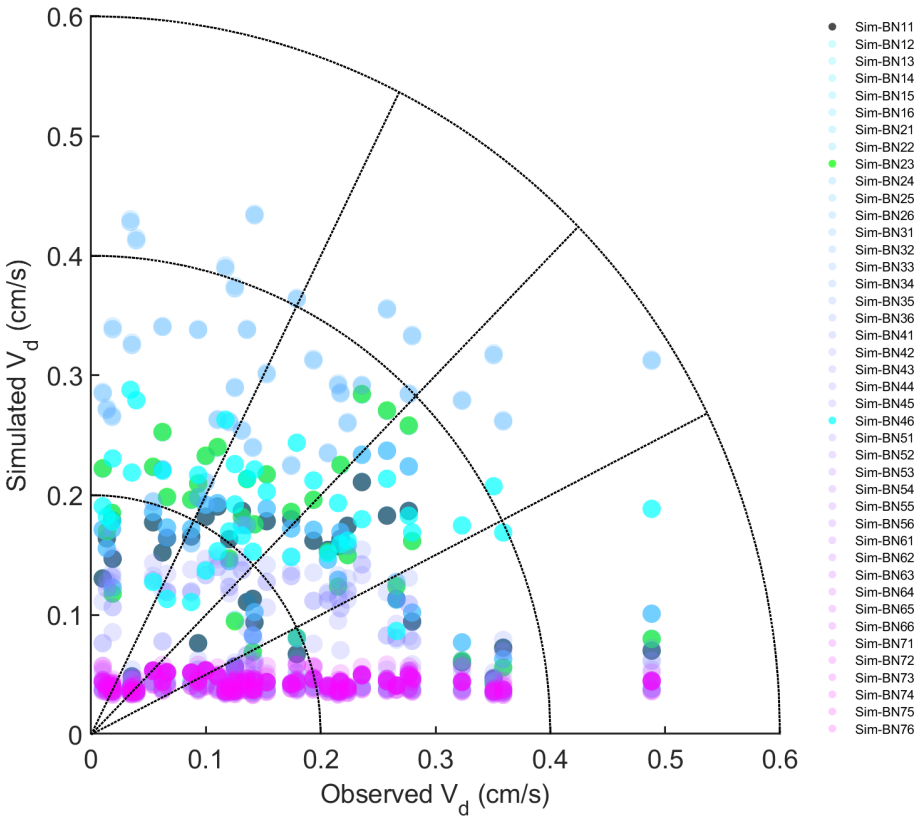

Figure 2: Comparison of observed and simulated V_d of NO_2 at noon

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

The time series of observation data and the three sets of simulation results of BN-11, BN-23, and BN-46 are shown in the figure below. It can be seen that part of the model results can characterize the effect of turbulent exchange. At the same time, it can be seen that, as mentioned in Zhang's article, there are many missing observation data. This lack of observation data also restricts our further evaluation of the improved performance of the model, so we can discuss and explain it only tentatively in this part of the article. We look forward to working with other observation teams to test and evaluate the simulation performance of our improved model.

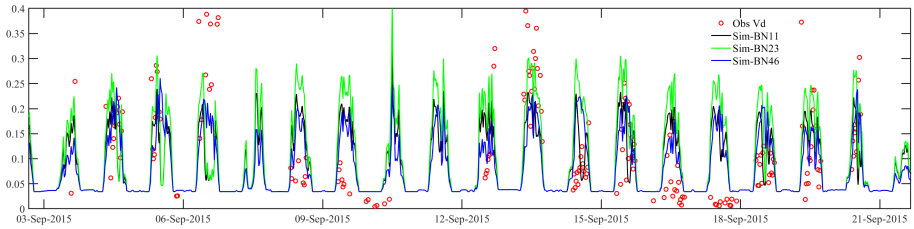

Figure 3: Time series of observed and simulated V_d of NO_2

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.

Thank you for your advice. The authors very much agree with the reviewer's view that lines 287-291 are important conclusions. For the work in the current manuscript, our main contribution is to improve the previous version of the Noah-MP-WDDM model and to tentatively compare the performance of various combinations of stomatal conductance schemes and nitrogen-limiting photosynthetic schemes and explain the simulation differences of the different schemes. Subject to the lack of observation data and uncertainty, we really cannot directly say whether a certain mechanism is the best. Therefore, the authors revised the conclusion of this paper after profound consideration and discussion based on the suggestions of the reviewer.

Figure comments:

Figures 4—7: How are errorbars calculated?

The errorbars are the standard deviation of the data. The authors use the "$varfun(@nanstd, time_series_data, 'GroupingVariables', 'Hours')$" function of Matlab to create a vector of diurnal results and compute their standard deviation, excluding NaN values.