

Reply to 'Comment on gmd-2021-157'

August 24, 2021

The comments from referees are colored black.

The authors' replies are colored blue.

RC2: 'Comment on gmd-2021-157', Anonymous Referee #1, 07 Jul 2021

General comments

As a whole, the paper is interesting as it compares the performance of various combinations of stomatal conductance models and nitrogen-limited photosynthetic schemes. I think the paper should be published as it brings new and useful information to the scientific literature. However, the presentation has to be improved before publication. The authors should pay attention to details (see below). On the other hand, I am concerned about the fact that the authors did not show results of the Aphalo and Jarvis (1993) scheme (simulation BN-31-BN-33) on Figures 4-7 despite the fact that this scheme present among the lowest bias (MBM-3 in Figure 3). It would be interesting to include results from Aphalo & Jarvis model in Figs. 4-7 as well. Finally, the discussion section is absent and more interpretation of the results should be done including uncertainties and knwoledge gaps.

From the point of view of the English language, the authors should ask the help of a native English speaker to review and correct the manuscript because at times we see poor English. Moreover, proof editing is needed since there are a lot of typographical errors in the submitted manuscript.

The authors would like to thank anonymous referee RC2 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 the paper should be published as it brings new and useful information to the scientific literature.

At the same time, the authors noticed that for the two combinations BN-13 and BN-33 with lower bias (-0.0187) mentioned by the reviewer, the results are very close to the lowest bias result (BN-23, -0.0185), and their daily change is basically similar, with only a small difference in Rb and the standard deviation (as shown in the figure below). So it was not highlighted in the discussion of the previous version. The authors added an explanation of the results of BN-13 and BN-33 in line 215 of the main text; please see the revised version.

In addition, because we are not native speakers of English, the authors asked a native English speaker to review and correct the manuscript. Based on the suggestions of the referee, we have modified all such expressions, hoping to describe the results more accurately.

Specific comments

- In the methodology section it is necessary to make clear that model output interpolated at one point (at the measurement tower site) is not necessarily comparable with a measurement point. - A discussion section should be added to discuss several weakness and uncertainties of inputs and results. For example, tower measurement is made at one location not necessary spatially representative of a whole model grid tile (whereas model output are average over a bigger area). The authors should also mention the resolution (grid spacing) of the model used and should discuss the validity and uncertainties of measurement versus model. What about scale dependency of dry deposition ?. A discussion section should be added to review the results and provide further interpretation and describe uncertainties and knowledge gaps in a better way.

Thank you for your advice. The authors fully understand what the reviewer mentioned. For the land surface model, in general, the grid point center subject to it does not necessarily match the location of the observation site, and therefore the comparison with the observation site results does not need to be very accurate. The authors very much agrees with the reviewer that this scale conversion will bring a certain uncertainty, especially when a single-point test model is coupled with a regional or global model.

This is very important and worthy of discussion. However, in this paper, we use the single-point Noah-MP-WDDM model to improve and couple the mechanisms that affect dry deposition simulation. The driving data come from regular meteorological observation data at the observation station, and the grid and station location mismatch or the spatial interpolation step does not exist in the current paper, so the authors did not discuss it in the original text. Based on the reviewer's suggestions, the authors revised the methods and conclusions of the paper, and further clarified the model setting. The authors expect to couple the current improved mechanisms to a regional and global land surface model in the next step, and it is hoped that an in-depth discussion will be conducted on the impact of this scale-up effect.

-Lines 93-97: not clear to what refers option 1. For those not very familiar with this model, it is hard to follow. More details are needed concerning options description and model characteristics.

Thank you for pointing this out. Yes, this is the authors' negligence. All the default parameterization options are used for these schemes, and the description has been revised in the paper. On this basis, the authors also revised the description of the other schemes in lines 96-97. Please see the revised version.

-Section 2.2 Coupling of stomatal resistance scheme. Not clear how the coupling is done. Please explain.

Thank you for pointing this out. The integration of these schemes is done by writing the equations from Table 1 as subroutines in the Noah-MP-WDDM model. The authors first commented about the original default equation, found the variables needed in the coupled equations from the original source code, used the variables as input to the corresponding equation, and then used the output variable result in the subsequent calculation. Some variables that are not in the original source code are calculated indirectly through other related variables. At the same time, in the process of this equation coupling, it is also necessary to declare the data type, calling listcalling tree, and to set the input and output defining declaration of the corresponding variable.

-Stomatal resistance or stoma resistance ? Throughout the document you should use stomatal resistance or stomatal conductance. E.g. line 13 please replace stoma resistance stomatal resistance

Thank you for pointing this out. All instances of "stoma" have been

replaced with "stomatal" in the manuscript.

-In models of stomatal conductance, $G_s = k A \cdot RH / [CO_2]$, inputs for RH and A are available but how did you obtain $[CO_2]$?. By measurement ? CO_2 is usually not available from numerical models. Please clarify.

Thank you for this question. The concentration of CO_2 is usually a variable in climate or atmospheric models, but as you mentioned, it is generally not available in quite a lot of land surface or biosphere models. In the current version of the single-point Noah-MP-WDDM model, the concentration of CO_2 is just a parameter input. After discussion, the authors believe that this is also one of the reasons for the small difference in the simulation results of the different schemes. We hope to be able to test the CO_2 monitoring data as different parameter input for the model from observation networks such as Fluxnet or ChinaFlux in the future. Thank you for your reminder.

- Line 233-234 smallest simulation deviation. This is poor English.

Thank you for pointing this out. The original intention of the authors here was to express it as the "minimum bias." Based on the suggestions of the referee, we have modified all such expressions, hoping to describe the results more accurately.

-The reference to simulation BN1-BN76 should also be better explained there in reference to Table 3. I suggest putting the name of the simulation (BNx) in Figure 3 (just below the bias values). It would become clearer for the reader and allow a direct comparison and consistency with the following figures 4-7 where simulation name BN are used.

Thank you for your suggestion. However, it is difficult to add references to each mechanism in Table 3. We tried to modify the form of Figure 3 with your next suggestion, as shown in the figure below. And as you mentioned, it becomes clearer for the reader and allows a direct comparison and consistency with the following figures 4-7 where the simulation name BN is used.

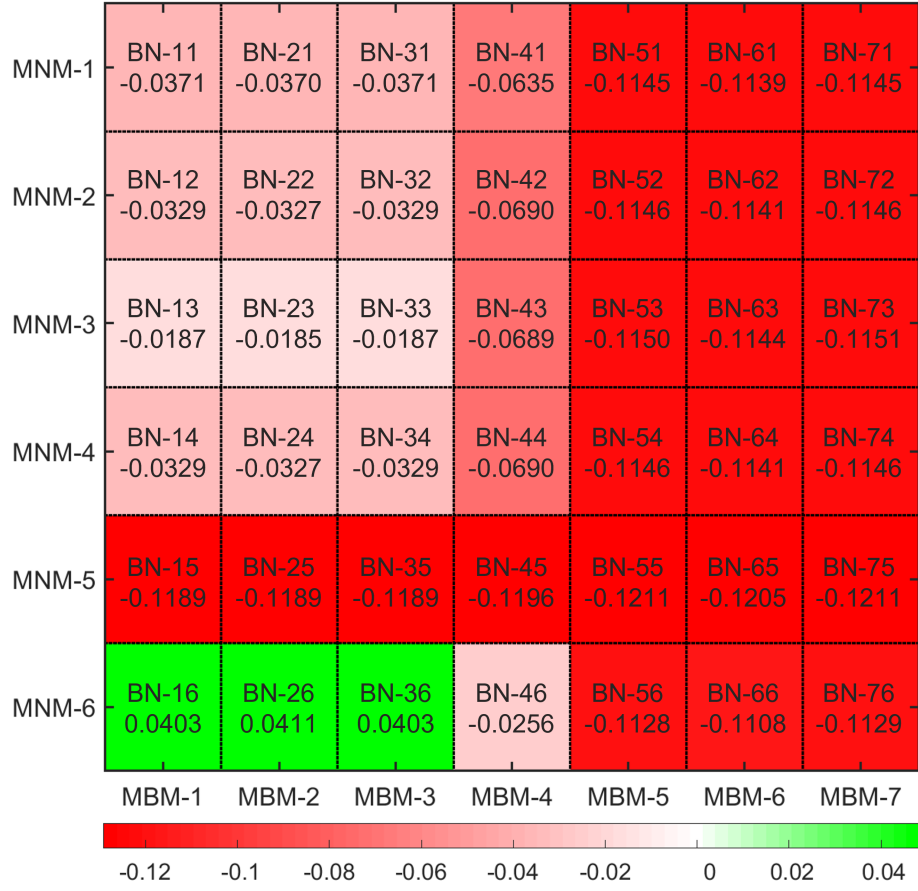


Figure 1: Mean bias of observed and simulated V_d

-Table 3 and Figure 3 are presented in different order (columns and rows are inverted). Please transpose the matrix in Table 3 to be consistent with Figure 3.

Thank you for your suggestion. As shown in the figure above, the content of Table 3 has been fully expressed in the new Figure 3, so we deleted the original Table 3 to avoid confusion.

-Figures 4-7. Why not putting results of simulation from Aphalo and Jarvis (1993) model in the figures (Sim BN31-BN36) ? It would have been interesting to present also simulation of BN31-BN33 in your figure since they show the lowest bias in Figure 2.

Thank you for pointing this out. First of all, the different presented order

of the authors' presentation of the tables and figures made the reviewers mistakenly believe that the low bias simulated combination in Figures 2 and 3 was BN-31 \sim BN-33. In fact, it should be BN-13 \sim BN-33, where BN-23 has the lowest bias. Second, as shown in the figure below, we display the simulated diurnal variation results of these cases in the form of Figures 4~7. It can be seen that the results of using the BN-13 and BN-33 mechanisms are basically the same as the results of BN-23. The discrimination causes only a slight disturbance to Rb. This may be due to the uses of CO₂ concentration as a parameter input to constrain the performance of the model. On the other hand, the differences of MBM1 to MBM3 are masked by the MNM process.

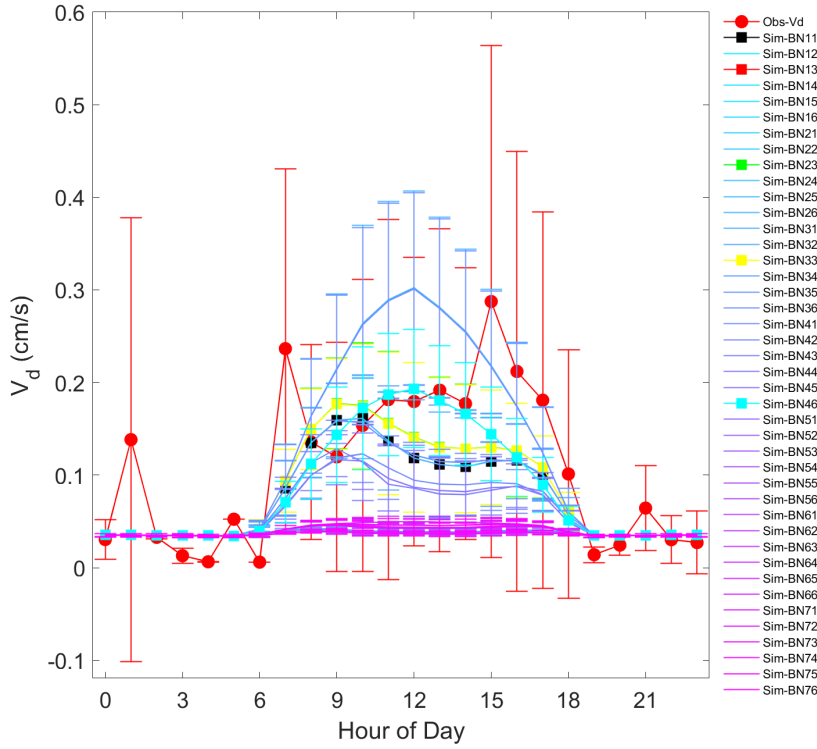


Figure 2: Diurnal variation of observed and simulated V_d with BN-13 and BN-33 highlighted

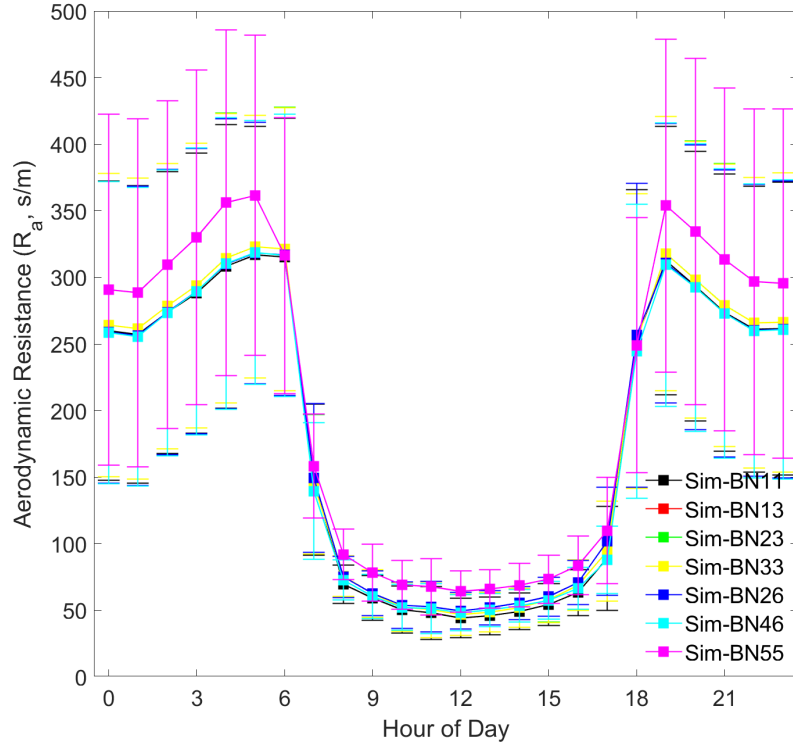


Figure 3: Diurnal variation of observed and simulated R_a with BN-13 and BN-33 highlighted

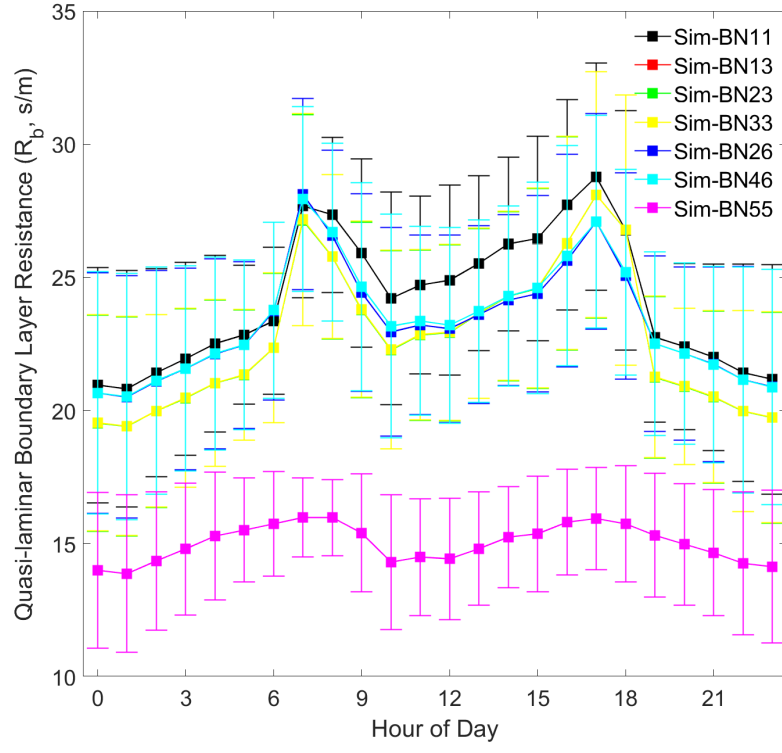


Figure 4: Diurnal variation of observed and simulated R_b with BN-13 and BN-33 highlighted

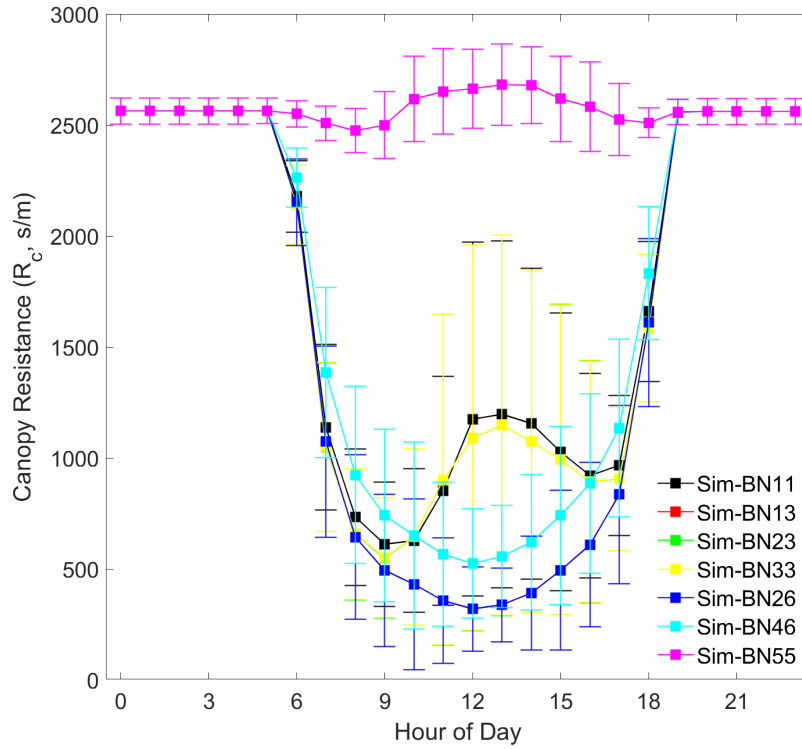


Figure 5: Diurnal variation of observed and simulated R_c with BN-13 and BN-33 highlighted

Minor

Line 9 and 11 Nitrogen-limitings -> nitrogen-limiting

Thank you for pointing this out. It has been modified.

Line 34 deviation - ??? standard deviation ?

Thank you for pointing this out. It has been modified. The deviation here means "uncertainty." When we tried to use Google Translate to English, its priority translation became "deviation."

Lines 42 and 44 typo. Please correct.

Thank you for pointing this out. It has been corrected.

Lines 45 Equ. - Eq.

Thank you for pointing this out. It has been corrected.

Figure 5,6 and 7. Units needed in the legend (s/cm ? or s/m?). Please specify.

Thank you for pointing this out. The units are s/m and they are added to the Y-labels.

Line 169, 170. 212 Typos. Please correct.

Thank you for pointing this out. It has been corrected.

Line 205 typo. 0.05 cms 0.05 cm/s

Thank you for pointing this out. It has been corrected.

Line 215, 234, etc.: deviation ? you mean standard deviation of what ? please clarify.

Thank you for pointing this out. The "deviation" in lines 215 and 234 should be "bias." Again, the Google Translate priority translation was "deviation." We have asked a native English speaker to help us polish the manuscript during the revision process.

Line 216 need space between number and units (typo)

Thank you for pointing this out. It has been corrected.

Lin 226 English could be improved, e.g. The black line in Fig 4 is BN-11 and the green line is BN-23 The black line in Fig. 4 corresponds to experiment BN-11 and the green line to BN-23, respectively.

Thank you for giving an example. It has been improved.

Line 227 This sentence does not make sense. Vd simulation upward simulated Vd values are increased.

Thank you for pointing this out. It has been improved.

Line 227-230 badly worded. Please re-write.

Thank you for pointing this out. It has been rewritten.

Line 233 smallest deviation (standard deviation ?)

Thank you for pointing this out. It should be "minimum bias."

Line 234 bias 0.001 cm/s ? where does it come from ?

This bias comes from all BN-46 midday simulated results and all the observed values at noon.

Line 245 s/m or s/cm ? in other parts of the manuscript s/cm units are used. Why here use s/m ? Please be consistent with units used elsewhere in the document. Do not mix units it is confusing for the reader. Either s/m in the whole document or s/cm but not both.

Thank you for pointing this out. The units of resistances output by Noah-MP-WDDM are "s/m," while the unit of deposition velocity is "cm/s." We have checked and unified all the units in the whole manuscript.

Section 3.4.1 badly written

Thank you for pointing this out. It has been rewritten.

Line 126. Later, the method of Lohammar et al (1980). This does not make sense. The authors just previously talk about a reference Leuning (1990). Year 1980 comes before 1990 not later. This poor English. Please reword.

Thank you for pointing this out. The situation here is that the method of Lohammar et al (1980) was applied by Leuning (1995)'s work. It is subject to poor English proficiency, which we have not stated clearly before. This part has been rewritten.