

Biogeosciences Discuss., referee comment RC1
<https://doi.org/10.5194/bg-2021-247-RC1>, 2021
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Comment on bg-2021-247

Anonymous Referee #1

Referee comment on "Influence of plant ecophysiology on ozone dry deposition: comparing between multiplicative and photosynthesis-based dry deposition schemes and their responses to rising CO₂ level" by Shihan Sun et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-247-RC1>, 2021

In this paper, the authors use a standalone terrestrial biosphere model to evaluate both multiplicative and photosynthesis-based schemes of stomatal conductance of ozone. Observational datasets of the dry deposition velocity and the stomatal conductance of ozone are used to do the model evaluation. The authors suggested that the photosynthesis-based stomatal algorithms that captured the responses to water stress had a better agreement with the observations. The manuscript describes a straightforward modeling study exploring basic parameterizations and comparisons to observations, and fits into the scope of Biogeosciences. I have a few minor comments as listed below.

My major concern is that based on the model-observation comparison in this paper, I do not see a significant improvement by using photosynthesis-based stomatal conductance methods, compared to the traditional multiplicative methods. The default multiplicative W89 scheme without stomatal response to water stress fails to reproduce the diurnal variations in G_s, but the multiplicative Z03 method seems to agree well with the other photosynthesis-based methods and the observations. Furthermore, all schemes compare poorly with observations in rainforests and in the Blodgett forest site (which is often associated with higher temperatures and water stress). Can the authors comment a bit more on the advantages of using photosynthesis-based methods?

The numbers and names of the modeling schemes are sometimes confusing. For example, the words "multiplicative" and "photosynthesis-based" in the title refer to stomatal conductance schemes, not dry deposition schemes, right? In the abstract, the Medlyn scheme is also a photosynthesis-based method, so there are actually two multiplicative (W89, Z03), two photosynthesis-based (FBB, MED) stomatal conductance schemes. I

think it should be stated clearly in the abstract and introduction, or it will confuse the readers.

Also, the figures should be consistent to show all 6 schemes when comparing to observed dry deposition velocity V_d , and show all 4 schemes when comparing to observed stomatal conductance G_s . For example, why not compare the Z03 scheme in Figure 11?

L42: Does this "45%" refer to an annually averaged percentage? How does this compare to your results? As the stomatal conductance is the main focus of this paper, I would suggest moving Figure S3 (showing the fraction of stomatal conductance to total deposition) to the main text.

L257: Please briefly explain the P-M method here.

Table 3: This table contains a lot information and is not easy to read. How about using some background colors, e.g., red/blue to show overestimation/underestimation and dark/light colors to indicate large/small bias?

L327 Not sure what this sentence means. Do you mean ozone reacts "with" soil-emitted NO and BVOC here?

Figure 2 The models seem to predict an overall earlier peak than the observations. Can the authors comment on why it could be?

L401 which site is "ponderosa pine forest"? Include the site name here.

Finally, this manuscript includes many abbreviations and sometimes is hard to follow. I would suggest including a list of abbreviations and explanations if possible.