

Biogeosciences Discuss., referee comment RC2  
<https://doi.org/10.5194/bg-2021-247-RC2>, 2021  
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## Comment on bg-2021-247

Anonymous Referee #2

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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 CO<sub>2</sub> level" by Shihan Sun et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-247-RC2>, 2021

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The authors examine the impact of carrying the stomatal conductance parameterization on the predicted deposition velocity. Comparisons are made between the well-used Wesely (1989) and Zhang et al. (2003) approaches as well as derivative parameterizations developed by substituting two different photosynthesis-based approaches into both the Wesely and Zhang approaches. Results are compared against observational data from several flux studies as well as the SynFlux dataset. Overall, the paper is sound but could benefit from additional editing. The text in Table 3 is very small as is the text in Figure 1. I am surprised that the photosynthetic models did not offer greater improvements in modeled values and would be interested in your thoughts on that. I would also be interested in seeing values of the correlation coefficient.

### Technical comments:

Line 111: It would be helpful to include examples of the biosphere-atmosphere interactions that are lacking in CTMs

Lines 140-143: There are multiple implementations of the W89 and the Z03 schemes in CTMs. Please be more specific here as to which implementation you used for both frameworks and not any specific differences that would have implications to the results here.

Line 170: Not specific to this line of text per se, but little is said about the stomatal blocking factor that is in the Z03 scheme and the potential effect of it. While it doesn't affect the actual value of the stomatal resistance, it definitely has implications for the contribution of that pathway to the overall canopy resistance and the deposition velocity. It merits a bit of attention in the paper.

Lines 220-226: It has previously been stated that the W89 version is the one in GEOS-Chem and it doesn't need to be restated here. The focus here is clearly on the stomatal parameterization. Have other papers already addressed the non-stomatal resistances? Perhaps cite any studies that have here and maybe make recommendations about whether that work should or could be part of future work.

Lines 227-228, it would be helpful to add "in TEMIR" after mode for clarity or you could add text to the model description to indicate that it runs both in single-site mode and as a gridded model.

Lines 239-240: It would be helpful here to indicate how the model handles multiple land use types within the grid cell. Again, this could be added here or in the model description. It is stated a bit later but would be better addressed in Section 2.1. The grid cell size is pretty large for ecosystem specific studies. What impacts might that have?

Line 265: It would be helpful to provide the Penman-Monteith method in the Supplemental.

Line 292-292: I don't agree with the statement that the schemes can generally capture the magnitude for the major PFTs. The models do not capture the range of values observed for the coniferous forest or the rainforest and predict a range of values for grasses that is larger than the observations.

Line 305: How did you determine that the modeled  $v_d$  for grasses is largely determined by the minimum stomatal resistance?

Line 469: It might be helpful here to add a few sentences comparing these results to the ones for the field study sites.

Line 535-536: It isn't clear to me where in Figure 9a, the non-stomatal vs stomatal deposition rates are provided.

Line 385: Define RuBP; also is equation 8 also from Franks et al.?

Figure 11: Why is W89 only used in this figure rather than including Z03?

Line 622-623: The results are not very convincing for switching to a photosynthesis-based model

Line 637: I am a curious about the use of "guaranteed" here.

Line 648: While I fundamentally agree that photosynthesis-based models offer opportunities for improving our estimates of air-surface exchange, I think there is still a lot to be considered when coupling in grid models with subgrid variability in vegetation types.

### **Editing notes:**

Lines 27 – 29: Sentence could benefit from editing

Line 89: Consider rewording to "tree and crop species of concern"

Line 90: Add "the" before DOSE

Line 127-128: Change "discussed" to "discuss"; add "the" before stomatal; parameterization should be plural

Line 157: add "the" before dry

Line 179: change its to their

Line 194: change "is" to are

Equation 4: the concentration should be denoted by C not c

Line 213: add the before photosynthetic

Line 277: Insert "the" before different and dry

Figure 1: in the caption, add "where" before different. I would also suggest using different colors as the current ones may not be optimal for users with color blindness issues.

Line 352: add "mean:" after monthly

Line 360: delete "in this"

Line 377: Perhaps the second vd in the sentence should be Gs?

Line 604: I have seen those experiments referred to as FACE not FREE