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Comment on acp-2021-670

Anonymous Referee #1

Referee comment on "Ozone deposition impact assessments for forest canopies require accurate ozone flux partitioning on diurnal timescales" by Auke J. Visser et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-670-RC1, 2021

acp-2021-670
Ozone deposition impact assessments for forest canopies require accurate ozone flux partitioning on diurnal timescales
Auke J. Visser et al.

General comments

This paper evaluates the potential added value of a multi-layer representation of vegetation canopies with respect to the traditional big leaf approach (e.g. Wesely, 1989) for simulating ozone deposition and ozone impact metrics for forest canopies. In the Appendix, It also compares for corrected version (vapor pressure and soil moisture of dry deposition such as Zhang et al (2003)).

This is an relevant paper making an interesting comparison between the proposed MLC-CHEM scheme and W89. Although the authors mention that the use of W89 mechanism is not valid for flux-based assessments (e.g from model simulation or observations), they do not discuss how their MLC-CHEM scheme could be implemented in a practical way in chemical transport models. Since the proposed methodology is constrained by flux measurement **observations,** it cannot technically be implemented in regional transport models since it would require a spatial density of ozone flux measurements which does not exist over the domain of such models. As the authors point out: "sites with long-term ozone flux measurements are scarce especially these sites with long-term ozone flux measurements". Therefore, the authors should explain how MLC-CHEM is an added value for current air quality models providing near real-time simulations (and using modified versions of W89).

Similarly, the authors also wrote: "*Observational studies indicate that ozone deposition exhibits substantial temporal variability that is not reproduced by atmospheric chemistry models due to a simplified representation of 5 vegetation uptake processes in these models*". The authors should elaborate a bit on that and mention that the lack of observational routine data for dry deposition is an handicap to appropriately simulate processes described by MLC-CHEM. It would be nice if clear recommendations would be given to modelers in that respect (other than "W89 is invalid").

Note that W89 is rather obsolete nowadays as many authors have shown its weaknesses

through the years. For example, LAI scaling to canopy was omitted in W89 and should be included. VPD weakness has been corrected by using Jarvis (1976), etc.

Specific questions/issues

1) The authors wrote: "This invalidates the use of the W89 mechanism for flux-based assessments…". The authors would be careful with such general statement. Moreover, the comparison with Wesely (1989) could not be entirely appropriate since very few institutions and models use W89 as in its original form. Most of models use different corrected forms of W89 (exchange of ozone, e.g. accounting for stomatal closure based on the vapor pressure deficit (VPD) and soil moisture, LAI scaling correction in W89,

2) Line 115-166. W89 can be deemed representative for the representation of dry deposition in other atmospheric chemistry model. Please be careful since several models have upgraded W89 to correct for some of its weakness (include VPD deficit, wetness on foliage, corrected form LAI missing in W89, etc.). e.g. (Jarvis, 1976; Ref. Valmartin, M, Heald, CL and Arnold, SR (2014) Geophysical Research Letters, 41 (8). 2988 - 2996.etc.).

3) Conclusion: How can MLC-CHEM be included in a regional transport model. ?. Avoidance of Wesely type parametrization might not be possible unless CO2 is better evaluated by these models (which does not seems to be the case now) and routine flux and near-real time measurements are available.

4) In the Appendix it would be useful and informative to also show the diurnal profile of CO2 concentration (not only CO2 fluxes). It would be also interesting to provide values of CO2 compensation point and diurnal variations of CO2 concentrations for the benefit of the reader (in Supplementary material).

Technical corrections:

1) Line 77: detailed below --> which are detailed below.

2) Typo: Line 117 uptak --> uptake

3) Typo: Legend of Figure 2 shaed --> shade

4) Legend of Figure 1: "and their in- and output variables" --> and their input and output variables.

5) Line 131 and 175 is a repeat of the same information.

6) Please avoid starting a section or a paragraph with the word **Figure** (section 3.1.1 and elsewhere)

7) Figure 5. "The shaded area in panel d highlights the daytime period (defined as 8-20 h LT) over which the stomatal flux is calculated". It looks that the gray (shaded) area rather corresponds to overnight time not daytime. Please review and correct.

8) Figure 8. Please specify the meaning of the numbers just below the figure ("0.28 0.23 0.31", etc.). the meaning does not appear anywhere in the document, it seems.