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Biogeosciences Discussions

Interactive comment on "Foliage surface ozone deposition: a role for surface moisture?" *by* N. Altimir et al.

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The paper contains a long time series of interesting data on ozone deposition to a northern European forest. Both reviewers supported the first step of the publication in BGD and suggested minor revisions for the final publication as indicated in their comments. The authors are focusing their analysis on the possibly enhanced or facilitated ozone deposition on wet surfaces. As an interested reader - more a generalist than a specialist - I got lost in the presented information. The paper would get more attention if the messages are more clearly presented and the dense information jungle is thinned out. Already figure 1 is confusing me. Why is the ozone uptake in the canopy order of magnitudes smaller than the ozone uptake of the shoot with comparable CO2 exchange rates? In my understanding, the descriptions of solid and dotted vertical lines have to be interchanged. The units for the shoot scale flux should probably be ng/m2/s instead of micro-g/m2/s?

At the end of the introduction the aims of the paper are described as follows: "We analyse the dependence of ozone flux to foliage on environmental and biological factors, with special reference to the role of stomatal uptake and surface wetness." This aim has not been fully achieved in the paper, because the evaluation of the deposition pathways is limited to conductance values. As a further step, the evaluation of stomatal and non-stomatal deposition fluxes should be added! The presentation of average conductance values (and relative contributions) in Table 1 is not very meaningful, because they do not necessarily represent the actual relevance of the removal processes that depends also on the ambient concentrations (which are not adequately presented in the manuscript).

As the authors point out themselves these models have their limitation for high humidity or low water vapor deficit. The most pronounced effect of the increase of the non-stomatal conductance occurs exactly for humidity ranges where the used algorithm for the stomatal conductance looses their validity. This potential conflict should be better sorted out. From the plant perspective the ozone flux into the stomata is important.

For the final publication the paper I suggest some clarifications.

The text in the methods sections is partly too circumstantial and could be shortened considerably. For example equation (3) is not needed because the eddy covariance method is not discussed in detail here and the reader is referred to the corresponding literature. In addition, almost the entire paragraph p.1750 line 20 to p.1751 line 10 can be omitted since the content is quite trivial. Instead I propose to move equation (A.1) to the main text.

In my understanding, there is a discrepancy between the results presented in Fig.7 and Table 1 for the partitioning of stomatal and non-stomatal conductance under dry conditions. In Fig. 7 the regression slopes close to 1 lead to the conclusion that the deposition under dry conditions is almost fully explained by stomatal uptake. However the average values for stomatal and non-stomatal conductance for dry conditions in

BGD

2, S930–S932, 2005

Table 1 are almost equal.

Figures 4 and 5 can be omitted. Figure 7 is too busy, why not showing an example and add a table with the regressions of the different experiments

Fig.8: The quality of the plots is limited by the relatively large data symbols. Therefore the visual impression is dominated by few extreme data points. As an alternative, box-plots (e.g. with data grouped for rel.humidity classes) would provide more quantitative information (also about statistical significance).

Fig.8: On p.1772,line 13 it is said that a reliable calculation of the stomatal conductance with the photosynthesis model is only possible for VPD $>$ 2g/m3. For a maximum temperature of 25°C this corresponds to a relative humidity of $<$90%. However the data points plotted in Figure 8 (right panel) include values up to 100%. How come?

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