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

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We thank editor A. Neftel for adding his comments to the revision of our work. This reply answers the editor's general and specific comments about the paper. Mention of changes refers to the version of the paper that will be considered for the final stage of publication in Biogeosciences. Point-by-point changes in the new version are addressed in a separate letter to the editor. Quotes indicate text from the editor letter:

The editor considers that "the paper would get more attention if the messages are more clearly presented and the dense information jungle is thinned out". For a clear message, the reader is invited to read the abstract and conclusions. The logical steps along the paper are specified at the end of the intro. The text is certainly extensive but we felt details of the methods and a thorough interpretation and discussion was in place. We were satisfied to read that both reviewers considered the paper well-written, well-structured, and clear.

"The text in the methods sections is partly too circumstantial and could be shortened considerably."

We agree that the methods section could be shortened in the new version .

"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)."

The paper is focused on analysing what are the mechanism producing the measured fluxes, more than on reporting the existing level of fluxes. The aim has been rephrased to make this idea more clear.

There could be the impression that the use of conductance is somehow exaggerating the non-stomatal component. Fig 1 shows that the night time values of fluxes (mainly non-stomatal) have magnitudes comparable to day time fluxes. Section 2.4 deals with the relation between fluxes and conductances. This relation is basically a first order relation and the factor of proportionality is the ambient O3 concentration. In simple terms, flux=conductance x concentration. At any given time, the ratio of stomatal versus non-stomatal components of the flux is the same regardless whether it is computed from the conductance values or from the flux values. That is the case because we use the same ambient ozone concentration for both. It would be interesting to find out whether using the same concentration is a reasonable simplification or whether the stomatal and non-stomatal sinks experience a truly different concentration, but this is

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not treated in this paper.

Ozone concentrations are now added in Figure 2 and 3.

"As the authors point out themselves these models have their limitation for high humidity or low water vapour deficit. The most pronounced effect of the increase of the nonstomatal 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. "

We discussed the potential limitation of stomatal estimations in section 4.3.2. We agree there is overlap between the uncertainty of stomatal estimations and the humidity ranges were the wetness enhancement of ozone deposition would be potentially larger; we particularly highlight the nocturnal case in section 4.4.1. We would welcome a formulation of stomatal conductance that would be reliable under all conditions. Perhaps a mechanistically based model of stomatal behaviour is the direction to look for a future replacement. About the validity of the photosynthesis model used in the paper, see next answer.

"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 25C this corresponds to a relative humidity of $<$90%. However the data points plotted in Figure 8 (right panel) include values up to 100%. "

For Fig 8 there are several explanations. One of them is that most of the high RH values belong to night time data when lack of light makes the photosynthesis model predicts stomatal conductance=0. That makes it possible that there are data points all the way till 100%RH. The VPD filter to detect unreliable conductance estimation is activated only in presence of light. In practice, the filter applied was more sophisticated than VPD $>$2g/m3. The cut-off VPD value varies with the light and other parameter, the combination of which describes the conditions where the model is very sensitive

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to VPD. This happens at very low VPD and irradiance above the light compensation point, typically at sunrise. The cut-off reported represented the lower limit of all possible situations and is applied often to the model when including data at low VPD is not crucial. In our case we obviously wanted to add as many data as possible and thus allowed for the maximum VPD possible under the particular conditions of every measurement. We could say we had thus under-explained the filter. On the other hand its relevance appears overstated. The actual amount of data filtered out is very small, in the category of removing few outliers. This filter thus should not gather too much attention and we have now removed the mention.

"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 Table 1 are almost equal."

The only difference between the data summarised in Table 1 and the data displayed in Fig 7 is that Table 1 contains May to August and Fig 7 contains April to September. That could have been confusing and thus we have now generated a new table that contains the same data as Fig 7. The values of deposition during April are very low, thus the inclusion of April in Table 1 lowers the average values, as the removal of April from Fig 7 would rise the offset.

A slope close to 1 tells the behaviour of the two variables is similar. The magnitude of the non-stomatal component can be seen in the value of the offset, which is visually more clear in the new version of Fig7. Taken the missing month into account, the offsets in Fig 7B agree with the values of dry gnonsto,O3.

"Fig 1: 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,

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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?"

The units of the shoot should be in ng and have been corrected.

The lines were dashed (- - -) for the start and end of thermal growing season and dotted (···) for the start and end of thermal winter. The dots were certainly small and could be interpreted as a continuous line leading to the misunderstanding. We have changed it to a real continuous line and updated the figure caption.

"Figures 4 and 5 can be omitted."

It would be interesting to know the reasons for omitting these figures. We are of the opinion this figures should stay. Together they make a strong point about the conditions of the site and how they manifest on the foliage surface. That is, the site is humid (Fig 4) and the humidity piles up on the foliage surface (Fig 5) and that happens a good proportion of the time (Fig 4). We want to show these facts have been measured, not extrapolated from visual observations neither deduced from other results. Fig 5 in particular wants to show the consistency of the phenomena inside and outside the chamber.

"Figure 7 is too busy, why not showing an example and add a table with the regressions of the different experiments "

A new version of Fig 7 shows data from only two shoots. Caption did not need updating.

"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)."

We agree the figure is dense. A new version of Fig 8 has smaller symbols and the grey has been changed by red for better contrast.

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Interactive comment on Biogeosciences Discussions, 2, 1739, 2005.