

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

**N. Altimir et al.**

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We thank J. Burkhardt for his understanding of our paper and the appreciative comments about our work and apologise for this late response. This reply answers the 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. Quotes indicate text from the reviewer’s letter:

We agree fully with J. Burkhardt that the interpretation of the data would be completed with better consideration of the contribution of deposited aerosols on the water film and its influence on subsequent ozone solubility and destruction. We realise that knowledge (of type and/or amount) of the compounds present at the surface is a requirement for a proper prediction of the magnitude of water film formation, quality of the solution, and potential to ozone destruction. Our discussion includes the different pollution load as a reason for differences in ozone deposition between sites. It would be indeed in-

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teresting to compute the aerosol load on the foliage as e.g. in Burkhardt and Eiden (1994) for the Hyytiälä site. In particular, it would be interesting to see if aerosol load alone -and the ensuing chemistry in the water film solution- would explain the ozone sink strength at the foliage surface without having to invoke more reactive compounds as we suggest in the discussion on the possible role of BVOCs.

J. Burkhardt comments that the BET isotherm might not be the most suitable formulation to describe water adsorption on a foliage surface. We agree that the case is clearly not a homogeneous multilayered adsorption but one where adsorption is facilitated by deliquescence -due to presence of salts- and capillary condensation -due to presence of structural inhomogeneity. The intricate composition and structure of the foliage surface might demand another adsorption formulation and we welcome suggestions. Furthermore, the behaviour of the adsorption may yield information about the surface structure and components. Perhaps a more detailed analysis of the surface wetness sensors data would be in place. We considered that, as a first approximation, the BET isotherm provided a reasonable match to the observed phenomena.

"Fig. 4/page 1753: (in)existence of nocturnal O<sub>3</sub> fluxes: I can not see the difference between 2002 and 2003 mentioned in the text."

The reference to Fig 4 is only to confirm that spring is the driest time of the year. The difference in the ozone deposition between springs 2002 and 2003 should be seen from the conductance values in Fig 2. The almost inexistence of nocturnal ozone deposition can be seen from the fact that the lowest values (which happen during night) are close to the x-axis, thus around 0. For comparison, during summer and autumn (and less clearly winter) the minima are quite clearly above the x-axis. We consider this can be seen clearly during 2002 because the daily patterns are more clearly marked than during 2003.

"Fig. 7 C and D/page 1756: I can not follow the description and the explanation given. In addition, the dashed line should go through origin. Details of calibration of the CO<sub>2</sub>

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model are missing."

We have rephrased the paragraph. For details on the model we refer the reader to previous works that describe the model and explain its calibration. We have corrected the position of the dashed line.

"Technical corrections"

Ozone concentration has been added to Fig 2 and 3. Other suggested technical corrections have been incorporated.

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

**BGD**

2, S1014–S1016, 2005

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