Biogeosciences Discussions, 2, S839–S842, 2005
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European Geosciences Union

Biogeosciences Discussions

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

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Received and published: 8 January 2006

General comments:

This is a well written and very interesting paper focusing on long-term measurements of ozone fluxes to a Scots pine forest and thus absolutely being within the scope of Biogeosciences. A thourough understanding of processes governing ozone fluxes to plants is needed, as it has generally been recognized that ozone risk assessment of plants, usually quantified through an exposure index (AOT40) is inappropriate. Plants are sensitive to stomatal fluxes, not to concentrations. However, the interpretation of ozone flux measurements is often confunded by manifold possibilities of chemical, physicochemical and biological interactions in which ozone may take part.

The ozone flux measurements reported in this paper allow an analysis on the influences of many of the environmental factors which are assumed or known to determine

O3 fluxes. Based on the simultaneous measurements on two different scales (shoot and canopy), overall fluxes are divided into a stomatal and a non-stomatal part, and consistent with other studies the latter is found to contribute substantially. In the past, the non-stomatal part of ozone deposition (or destruction) has been suspected to be enhanced by either rising temperatures or rising humidities, and there have been studies supporting both (contradicting) views, respectively. The authors specifically address this question and, based on leaf wetness measurements and correlations with other environmental parameters, they come up with the conclusion of a strong humidity influence on non-stomatal fluxes.

It is the major advantage of this paper that it splits up different environmental situations and compares ozone fluxes with high time resolution, and that the subdata sets still contain a significant amount of data. This gives the results and the conclusions drawn high credability, which is further supported by different plausibility tests of the data (e.g. that the fluxes derived on the shoot and the canopy scale are matching if LAI is taken into account).

There are some positive aspects which are new or relatively new and important for further research: - Identification of conditions with a strong humidity influence on O3 fluxes - the identification of situations where the humidity approach does not work, which merits further research - The clear indication of nocturnal O3 fluxes and the conditions governing them

There are, however, some small criticisms: - It would have been interesting to know something about ozone concentrations, which are now only accessible via Fig. 8. - Some of the results and conclusions are difficult to reconfirm by the figures shown (see below)

Overall, this is a well structured paper, which is transparent in gathering and summing up results and conclusions. Especially Figs. 6 and 8 and the explanations given show a nice example of in-depth data analysis. Measurements of NO and BVOCs as possible

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other actors of ozone destruction are not included, which especially in case of the BVOCs might further explain some findings. This is discussed by the authors. NO influence can be ruled out from another study in the same area.

In my opinion, the interpretation of the data is not complete as it does not consider sufficiently the contribution of deposited aerosols on the water film composition (highly concentrated solutions) and its influence on subsequent ozone solubility and destruction. I agree that this would have been speculative and is not the main scope of this already long paper. But I think the amount and composition of deposited aerosols might e.g. help to explain the differences between spring 2002 and 2003, or between different age classes of the needles. Hygroscopic action of salts on the needle surfaces is also the reason why I think the BET isotherm, based on a homogenous surface, is not the most suitable approach to describe water adsorption in this case.

Specific comments: The general quality of the graphs and captions is good, with some small comments (technical comments see below):

- Fig. 4/page 1753: (in)existence of nocturnal O3 fluxes: I can not see the difference between 2002 and 2003 mentioned in the text.

- 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 CO2 model are missing.

Technical corrections

The two grey lines in Fig. 1 would have been better recognizable by different colours. The details given in the text to explain Figs. 2 and 3 (e.g. adressing night time values) can not be followed easily.

The diagonal line in Figs. 7A, C does should go through origin.

Equations are not fully explained (Fig. 1) q vs. Q, page 1748 V is not explained (eq. 1, eq.7) Some references missing (Hari et al., 1986)Ě. p. 1756: O3 conductances p.

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1756: eq. (4) should be eq. (3) p. 1741: Sanderman p. 1768: Snyden

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