

Interactive comment on “The dynamic chamber method: trace gas exchange fluxes (NO, NO₂, O₃) between plants and the atmosphere in the laboratory and in the field” by C. Breuninger et al.

Anonymous Referee #1

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General comments:

The paper presents a very detailed account on applying dynamic chambers for studying the exchange of NO₂, NO and O₃ between plants and the atmosphere. The main achievement of the paper is the systematic account of possible errors in estimating fluxes, deposition velocities and compensation points. Thus both the optimal chamber design, the effects of accuracy, precision and limits of detection of the sensors as well as the effect of the statistical treatment is considered. The paper includes an extensive discussion also attempting to point out possible errors in previous publications on

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exchange studies of the NO-NO₂-O₃ triad between atmosphere and plants.

In general, I find that the paper is well written and certainly merits publication. The paper can be considered a reference paper for the state-of-the art of such studies. However, there are some points, as specified below, that in my opinion can improve the paper further.

Specific comments:

p. 5201, l. 20: Why is the efficiency of the BLC so low, and how does that effect the LOD? Please state why the efficiency is higher under field conditions. Is it due to higher concentrations?

p. 5204, l. 10: This is a rather low flow rate (and much lower than under field conditions) resulting in a rather long exchange time of the air volume in the chamber. How does this affect the results? And does it affect the comparison to field data?

p. 5206, Sect. 3.3.4: I wonder for how long time the branches were enclosed. It seems that the lid is not opened during the whole experiment?

p.5208, l. 13. I would suppose that a temperature increase might occur in the chambers. Was this measured?

p. 5214, l. 20. I suppose the improvement was due to higher concentrations under field conditions. Please add this information.

p. 5526, Sect. 5.3.2: I understand the “statistical” reason to select for higher delta m,NO₂, but I also think that this will bias the resulting average v_{dep,NO₂}. Could you comment on this?

p. 5230, l. 14 to p. 5231, l. 1: I find this too speculative and not fair to the original authors because you actually assume some errors in their experiments that are not documented. Therefore, I highly suggest that you delete these lines and leave in the discussion the other parts which are justifiable.

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p. 5233, l. 19-25. This is not documented in the present paper. It might be a conclusion of the companion paper mentioned somewhere else in the text.

Figure 12: It is clear that the concentration of NO and NO₂ in the measurement interval is not always very stable: in the presented data-set e.g. intervals 16-20 min. and 28-32. What would be the fate of such data after your data quality check procedure?

Technical corrections:

p. 5184, l. 9. I suggest: "detection of the photolysis product".

p. 5193, l. 5. I suggest to delete "which"

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 5183, 2011.