

Interactive comment on “Field investigations of nitrogen dioxide (NO₂) exchange between plants and the atmosphere” by C. Breuninger et al.

Anonymous Referee #1

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GENERAL COMMENTS

This manuscript reports results of chamber measurements of NO₂ fluxes of Norway spruce branches, compares them with earlier observations, and seeks for reasons for discrepancies. Contrasting some previous studies, the authors did not find a significant compensation point of the NO₂ flux and their deposition rates were lower than in most earlier published data. When discussing the reasons for this, the authors emphasize the accuracy of their measurements compared with some earlier studies, because they used a highly specific NO₂ detection system, unlike many of the earlier ones.

Overall, this study seems careful and measurements and data analysis reliable. There are no novel revolutionary findings about the NO₂ fluxes between vegetation and the atmosphere, but it is good to get more data on this phenomenon, and the authors seem

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to have done their best to avoid known sources of error within this kind of chamber measurements. There are, however, some points that need to be clarified and I think the analysis requires more discussion on the implications of the findings, to give the paper more value.

About the language: the paper is mainly clear and the message easily understandable but there are some errors with grammar and phrasing. I recommend a native proofreader.

SPECIFIC COMMENTS

- I would like to see you explicitly stated what it means, in practice, if there is no compensation point. Does the NO₂ concentration inside the plant leaves equal zero? Is there an infinite NO₂ sink in the plants? You conclude that, based on your statistical tests, it is unlikely that there is a compensation point for the NO₂ flux. However, you also report “significant” emission of NO₂ (page 18179). How is that possible if there is no compensation point? Please clarify this.

- Photolytic conversion of NO₂. You very much stress the others have had insufficient measurement techniques and that is why their results differ from your highly specific ones. It would be nice if you discussed what this means. Did the others detect a real flux, of biological origin, of some other NO_y compound (HONO, PAN, ?) visible to their analyzer but not yours? Or do you think their extra fluxes were purely an artifact?

- What are the implications of your findings: do the lower deposition velocities and the non-existing compensation point affect something, for instance, in air chemistry models? Do you have an idea of how much?

- Page 18167: Aim of the paper is: “to investigate the stomatal NO₂ uptake comparing field and laboratory measurements of spruce..”. This sounds like you had done both field and lab measurements, which you haven’t. I suggest rephrasing the aim so that it will be clear you only have field data, but you compare it with existing published data

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from field and lab.

- In the Methods chapter, you do not explain how the nutrient concentrations of needles were analyzed. Please add it.

- Page 18168: It seems your chamber walls were not rigid. How accurate is the volume of the Teflon bag chamber?

- The reference chamber:

a) Page 18169: Please explain more clearly how the measurements were done. So you had an empty reference chamber, but it in fact was not used in the NO₂ measurements, only for the CO₂ and H₂O measurement? Did you monitor the inlet and outlet NO₂ concentrations of the reference chamber simultaneously with the branch measurements, or did you just trust the previous empty-chamber tests reported in your earlier paper (Breuninger et al. 2012)?

b) If you used the earlier results: You say deposition of NO₂ onto the walls of the chamber was not significant. Have you tested the chamber blank in near-zero concentration, or only within the range reported in Breuninger et al 2012 (6 ppb and higher)? If not in near-zero, is it possible the chamber acted as an NO₂ source when the ambient concentrations decreased close to zero?

- It would be helpful especially for a first-time reader if you had the definition of symbols ($m_{s,NO}$, b_{NO_2} , F_{ex,NO_2} etc) in a separate table, to make it easier to check what was what. Although some of the symbols are quite self explanatory, all are not.

- Page 18176: Why did you filter away the NO_x concentration peaks coming from the traffic? It would have been interesting to see the flux data.

- Chapter 3.2: This is a Results chapter. Please move to the Discussion the general background information (about how plant physiological processes and nutrient concentrations may vary) and speculation about whether the differences in potassium concentration were significant, and include only your results.

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- Significance of the compensation point

a) Page 18177: It is a bit confusing (although apparently correct) to call the same percentages first “significance probability for $m_{\text{comp},\text{NO}_2} \neq 0$ ”, then “unlikely probability for $m_{\text{comp},\text{NO}_2} \neq 0$ ”. I suggest you write it for dummies, e.g. ‘with a probability of 19.98% to 91.22%, the compensation point was zero’ or something like that.

b) Discussion: You say ‘Moreover, the significance probability of the compensation point concentrations in our study was always “unlikely”.’ What is the limit for an “unlikely” percentage? Greater than zero? Greater than 50%? If the latter, you cannot say it was always unlikely.

- In the abstract and conclusions you say the compensation point was 7.4+-6.4 to 29.0+-16.3 nmol m⁻³, but on page 18177 you write the range started from 2.4+-9.63.

- Page 18178: do you have any idea of why the potassium concentration was higher for the young enclosed needles?

- Page 18179: Mention the tree species used in Chaparro-Suarez et al. (2011)

- Page 18181: You could refer e.g. to the study by Rondón & Granat 1994. They conclude that “the relationship between NO₂ needle conductance and stomatal conductance was close to 1:1.” And they had a photolytic converter.

- Page 18183: You suggest one source of discrepancies between you results and others could be that the others did not use the bi-variate regression analysis. It is difficult for a reader to know how much this affects the results, since you do not show it. Would your deposition velocities and compensation points move closer to those observed by others, if you used the simple linear regression?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 18163, 2012.

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