

Comment on the manuscript:

The dynamic chamber method: trace gas exchange fluxes (NO, NO<sub>2</sub>, O<sub>3</sub>) between plants and the atmosphere in the laboratory and in the field.

by: C. Breuninger et al.

MS No.: AMT-2011-92

In this manuscript the authors describe the dynamic chamber method used to measure bi-directional flux densities of NO, NO<sub>2</sub> and O<sub>3</sub>. To demonstrate the performance of the equipment the authors show data obtained for spruce. For NO<sub>2</sub> they report deposition velocities in the range of 0.07 to 0.42 mm s<sup>-1</sup> and very low compensation points. Therewith they confirm their own findings obtained with other plant species and published elsewhere.

Focus of this manuscript is a description of the dynamic chamber used in these experiments, the performance of the chamber as well as the analytic and mathematical methods used to determine flux densities, deposition velocities and compensation points for the triad NO, NO<sub>2</sub> and O<sub>3</sub>. There were many attempts to measure such data. But determination of these quantities is complicated because NO, NO<sub>2</sub> and O<sub>3</sub> undergo conversion in the gas phase within minutes. This conversion is well known since more than 50 years now. Nevertheless, even in newer literature these conversion processes were often neglected.

Such unjustified negligence possibly has caused defective results. I therefore see the need for a thorough description of the basic concept of flux measurements with respect to the NO, NO<sub>2</sub>, O<sub>3</sub> triad. The present manuscript is a successful approach. Although the basic concept is not novel, the accurate consideration of conversion processes as well as the description of consequences for flux measurements justifies publication. As AMT deals with development, intercomparison and validation of measurement instruments and techniques of data processing, AMT is the correct address where to publish this manuscript.

The paper address relevant scientific questions within the scope of AMT, the experiments have been carefully conducted and the results have been evaluated with due diligence. Overall, I find this paper interesting and worthwhile to be published. However, there are some points that should be revised.

Here my general concerns:

1) The manuscript is not easy understandable because of some unnecessary lacks.

In several cases I cannot understand equations as the abbreviations used therein are not explained directly. Examples therefore are the equations 1.1 to 1.3. Reading the text near to these equations I cannot find a remark wherefore  $m_a$ ,  $m_s$ ,  $k$  or  $j(\text{NO}_2)$  stand. Other examples are equations 4.1 to 4.3. Here crossbars emerge without any explanation. Not until two pages later averaging is mentioned giving a reader an advice regarding the meaning of the crossbars. Nevertheless, the meaning of the crossbars is still not mentioned.

The reader of this manuscript is forced to first read the appendix before understanding several equations written short after the introduction. This should be avoided and I therefore ask the authors to check their text for such unnecessary flaws. These make it hard to read the manuscript and redirect from the basic message.

2) The authors should give some comments on their quality criterion of significant concentration differences ( $m_a$ ,  $m_s$ ).

The authors' requirement of significant concentration differences ( $m_a$ ,  $m_s$ ) is certainly justified if concentrations in the chamber are well above a hypothetical compensation point. It allows obtaining reliable flux densities because artificially low concentration differences are not taken into consideration. But, if  $m_s$  would be near to a hypothetical compensation point, this criterion is not clear to me: At concentrations near to the compensation point these concentration differences should be low.

The authors should state how they distinguished between concentration differences being insignificant due to an artifact or insignificant due to trace gas concentrations near to the compensation point.

There are two questions connected to this item. These also should be commented.

Does a strict requirement of significant concentration differences introduce a bias to the results because it may rule out data points obtained near to a hypothetical compensation point where small differences are correct?

If the requirement is strict, compensation points will only be obtainable by extrapolations. Is that true or can compensation points also be measured if concentration differences are zero?

3) The data from the laboratory experiments do not give any important information and the whole section can be deleted without abating the quality of the manuscript.

The only justification for a detailed description of the laboratory results would be some important information on top of those from field experiments. But due to the sub-optimal performance of the analytic device the results from the laboratory experiments do not give such information. It is therefore unnecessary to describe the laboratory experiments. Focus of the manuscript is a description of analytical and mathematical procedures. These procedures are described in detail at hand of the field experiments. The authors should therefore consider deleting the whole section dealing with the laboratory experiments.

Some minor points:

Chapter 2.1 refers to basic equations explained in the appendix (A7.1 – A7.3). Please give these equations directly at the beginning of this chapter (justification can be left in the appendix A). This will also help for easier understanding of the equations 1.1 to 1.3. as also the abbreviations can directly be explained.

P. 5193 lines 4 ff.: it is written: “purging rate  $Q$  and leaf area  $A_{\text{leaf}}$  are unchanged”. Equations 8.1.1 to 8.2.3 on the same page contain averaged quantities. If both quantities are unchanged why are averaged values used? Please explain.

P. 5196 line 11: typo: “99u4”

P. 5210 line 7: check grammar “depends from”

P. 5215 line 8: sample flow ( $1.42 - 1.67 \text{ m}^3 \text{ s}^{-1}$  or  $8.5 - 10 \text{ L/min}$ ). These data do not fit together. Check for the typo.

P. 5232 lines 1 to 3: The  $m_{\text{comp,NO}_2}$  values reported in previous studies .. would be overestimated between 3 and 17% if the photolysis of  $\text{NO}_2$  was not considered. “ Give more information on the  $j(\text{NO}_2)$  values that are the base for this estimation.

P. 5235 Conclusion No. 6 can be drawn without any experimental results. Conclusion no. 6 is therefore trivial and should be deleted.

P. 5235 Conclusion no. 8 is not clear to me. I can read it as request to adjust a reference cell properly (which is trivial) or as a hint that reference cells are anyhow useless. What is the conclusion no. 8? Please clarify.

Page 5237, lines 13 ff: Here seems to be a misunderstanding regarding the conception of a steady state condition. Steady state condition just means that the respective quantity does not change with time. Equilibrium is another matter. Please correct.

Page 5238 line 19 typo "Atkinson"

Page 5242 standard error listed twice, delete once