

***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 #4

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General comments

This manuscript deals with two different issues: the development of a chamber method to measure NO-NO₂-O₃ fluxes at the plant level and the compensation point of NO₂ in plants, and how this latter is perturbed by the measurements themselves.

These two subjects are highly relevant to the journal and make a very substantial import to the field of measurement techniques and on the knowledge of NO₂ fluxes themselves which are until now poorly quantified.

These researches are of very high scientific standards and the manuscript is well writ-

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ten and quite clear, although it may be improved by making less reference to equations and summarising more results in Tables. I think that this work should be published and is likely to become a reference in terms of fluxes measurements of reactive gases with chambers.

I however have a few comments which I would like the authors to consider prior to publication:

- Overall it is a bit disappointing that the authors do not discuss more the physiological reason for NO₂ compensation point. They should also discuss the reason for the negative NO₂ compensation point found in the field experiment.

- The introduction very nicely details the potential drawbacks of chamber flux measurements of O₃-NO-NO₂. However they do not consider at all other compounds like VOCs. Does it mean that the only significant interaction in such chambers is between O₃ and NO_x?

- There is a great number of equations which may be given as one equation for all the three gases using index notations (e.g. 1, 3, 8, ...). This may get the paper smaller and may be easier to read. The reference to the large number of equations makes also the paper hard to read. Please consider whether this may be useful.

- Regarding the argument that highly sensitive instruments are needed to determine the NO₂ compensation point: Could one not argue that the compensation point may be determined by extrapolation of the “ms” versus “ma” line (Fig 1b) down to low concentration?

- Please consider simplifying the results presented in section 2.2 “constraints of precision”, by synthesising the results in tables. The text is very hard to read at the moment with many symbols and numbers.

- In the constraints of design, I would suggest to consider the need to have the smallest possible aerodynamic resistance in order to be able to retrieve stomatal exchange

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parameters. The leaf temperature should also be characterised in order to estimate water concentrations in the stomatal cavity. I would like to know the authors opinion about these points.

- The authors have used a Model 49C (Thermo Electron Corporation, USA) for O₃ concentration measurements. They also have used electro-valves to switch between chambers and between inlet and outlet. However these analysers have been shown to have large negative readings if switching between different humidity regime, which is expected between inlets and outlets. Did the authors had to face such an issue and if so, how did they get round this problem? (see ref: Wilson and Birks, Environ. Sci. Technol. 2006, 40, 6361-6367).

- The authors speak of deposition velocity measured from the chamber. Since the mixing is very efficient, should they not rather talk of leaf conductance?

- I sometimes feel the Figures hard to read. The legends, especially, could probably be improved. Maybe legends on the graphs or beside the graph may be easier to read.

Detailed comments

P5185L25: Should it not be “leaf area indexes” rather than “leaf are indices” ?

P5188L19-L25: see comments on Appendix A.

P5188L24: Please specify if Aleaf is a one or a two-sided leaf area index?

P1589L5-L7: This statement is only true if NO/NO₂ is not emitted from the leaves. . .

P1589L12-13: There may be a flux without “consumption” and “destruction”, as for instance in the case of a compensation point which is a concentration in equilibrium with a liquid phase. Please consider rephrasing.

P5190L1-L2: Wouldn't “inward” and “outward” fluxes be more appropriated here?

P5190L4 & L16: Consider using italics for Latin “sensu stricto” and “a priori”. P5190,

eq4.1 and onwards: Overbars notations have been introduced without definitions.

P5198L7: “stomatal” instead of “stomata”.

P5201L6: “Air liquide”

P5201L20-21: How do the authors explain this increase in conversion efficiency of the BLC?

Fig. 3: The color line are difficult to see: I could not identify the “dark purple line” . . . Try to use other colors.

Fig. 7: How do you explain the negative values observed just after the step change?

Appendix A: To me “molar mass” is the “mass per unit of moles”. I think there is a confusion here with “number of moles”: The “molar mass flux” is rather a “molar flux” i.e. the total number of moles leaving or entering the chamber. I suggest the authors to clarify the nomenclature.

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

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