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5, S2754-S2760, 2009

Interactive Comment

## *Interactive comment on* "An automated dynamic chamber system for surface exchange measurement of non-reactive and reactive trace gases of grassland ecosystems" *by* L. Pape et al.

L. Pape et al.

Received and published: 16 January 2009

Anonymous Referee #1

The authors have used a NOx monitor which uses non-specific converter for NO2 concentrations. How does this affect NO2 fluxes?

>> A comparison of the molybdenum converter derived NO2 concentration to a specific photolytic converter at the field site showed that usually less than 10% of the molybdenum converter signal have to be attributed to other oxygenated nitrogen compounds. The (overestimation) effect in the derived surface resistances for NO2 is supposed to be of similar magnitude or even smaller. We will add a comment on this problem in Section 2.3.



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Interactive Discussion



p. 3177 Meteorology Consult Inc. should be Meteorologie Consult GmbH

>> Will be corrected.

Bassin et al., should be 2007 in the reference list.

>> The correct citation in the text (p.3159, line 6) is "Bassin et al. (2007)", and the reference in the list has to be changed to: "Bassin, S., Volk, M., and Fuhrer, J.: Factors affecting the ozone sensitivity of temperate European grasslands: An overview, Environmental Pollution, 146, 678-691, 2007."

Davidson et al., 2007 is missing from the reference list.

>> This is a mistake in the text. The citation has to be changed to "Davison et al. (2008)"

Kesselmeier et al., 1997: Muller->Müller, Schafer->Schäfer ; add information that this article is in the Supplement.

>> Will be corrected.

Maljanen et al., 2004, which is in the reference list, is not in the text.

>> This reference occurs in Table 4 (new: Table 1).

Anonymous Referee #2

(1) There are quite a few forward references in the text. While this is probably intended to help the reader, in practice it may make the presentation more difficult to follow. In Section 2.1.2, which is part of the Materials and methods, the authors present simplified equations (Eqs. 4 and 5) before a key simplification (Rmix = 0) is justified by experiments (in Section 3.2.1)

>> The criticism of the referee is justified. We will remove the mentioned forward reference and introduce the simplification (Rmix = 0) not before the Result section. Eqs. 4 and 5 and the related text (Sections 2.1.2 and 3.2) will be changed accordingly.

BGD

5, S2754-S2760, 2009

Interactive Comment



Printer-friendly Version

Interactive Discussion



(2) The determination of the surface resistance (Rc) requires an estimation of the boundary-layer resistance (Rb\*), which is approximated by measuring ozone deposition to a liquid sink and assuming a simple LAI dependence for vegetated surfaces. This section (3.2.2) requires some further consideration. Firstly, I doubt if the Rb determined for a smooth liquid surface equals that of bare soil (p. 3175, I. 21-22). Secondly, Galbally and Roy (1980) did not separate the mixing and boundary-layer resistances, so it is not clear if the leaf area effect can be deduced from their results (p. 3175, I. 24-25). Finally, the authors do not justify the Rb\* parameterization (Eq. 14) by any data. For a small LAI, Eq. 14 will result in an Rb\* that is comparable to Rpurge and thus constitutes a significant term when calculating Rc. Therefore I would like to see an estimate of the uncertainty of Rc due to the uncertainty related to Rb\*. In this respect, the comparison based on Eq. 15 is not very useful, since it depends on the rough estimate assumed for Rc and represents a high LAI with a small Rb\*.

>> We declared in the text, that the Rb\* can only be approximated by constraining observations. In order to make this more clear we will rephrase the respective paragraph in text. We agree with the referee that an estimate of the uncertainty for Rb\* would be useful and we will add that in the revised manuscript. Yet we think that the LAI effect observed in our study is indeed (with some restrictions) comparable to the results presented by Galbally and Roy (1980). First, they report similar mixing air velocities within the chamber and therefore their mixing resistance is supposed to be small, too. Second, they found similar chamber deposition resistances for a liquid surface (KI solution) and a smooth bare soil surface (sprayed with KI solution).

(3) Throughout the paper, the authors emphasize the long-term applicability of the measurement system. However, in the section specifically dedicated to this topic (Section 4.3), there is rather little material that would actually demonstrate the applicability. The authors refer to measurements over a full growing season and visual observations (p. 3183, l. 23-25), but do not present any data. Would it be possible to include some kind of operational statistics related to the long-term performance (e.g. data coverage,

## BGD

5, S2754-S2760, 2009

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion



maintenance due to instrument failure and wall contamination)? It would also be useful to comment if there are any major differences in this respect between the automated chambers listed in Table 4.

>> We will include exemplary operation statistics in the text. A more detailed evaluation of the long-term performance of the system will be addressed in another paper where the results of full season measurements are presented. Unfortunately, there is usually little information about the long-term operation characteristics of chamber systems in the literature.

page 3159, line 6: "Bassin, 2007" missing from the reference list (but "Bassin et al., 2004" included).

>> Will be corrected (see above ref.#1)

page 3160, line 19: Tables should be numbered consecutively in accordance with their appearance in the text.

>> We will remove this reference to Table 4 from the text (see comment below).

page 3160, lines 16-19: It is not obvious from Table 4 how the chamber design and operational characteristics are adjusted to NO emission measurements as there is variation between the systems in all the reported parameters. Please elaborate.

>> We will remove the reference to Table 4 in the introduction. The chamber characteristics given in Table 4 are elaborated later in the Discussion section.

page 3160, lines 23-: This paragraph explains that the chamber system described here is based on previous systems for NO, NO2, O3 and reactive organic compounds. As the new system is also intended for CO2 measurements, it would be useful to briefly comment on previous chamber studies on CO2 fluxes. For example, Pumpanen et al. (2004, Agric. For. Meteor. 123, 159-186) present an extensive comparison of different chamber techniques for measuring soil CO2 effluxes.

BGD

5, S2754-S2760, 2009

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion



>> In this paragraph we give references only to specific chamber systems developed previously in the groups of the authors, on which the present system is technically based. The mentioned study by Pumpanen et al. (2004) predominantly focuses on non-steady-state chambers. We will include the reference in this context in the manuscript.

page 3164, equation 5: There is an error (most likely a typo) in this equation. "+ Rb\* + Rc" should be moved to the denominator.

>> The reviewer is right. This typo has to be corrected.

page 3164, lines 4-6: According to the Monin-Obukhov similarity theory, Ra depends on both the thermal stratification (sensible heat flux, H) and u\* (i.e. in theory varies with H for a fixed u\*).

>> The text will be rephrased accordingly.

page 3167, lines 3-4: Please explain why the transmissivity of the whole chamber body is calculated as "FEP film + 50% acrylic glass" (rather than "FEP film + acrylic glass").

>> The formulation in the text was not optimal and will be improved. Since the chamber walls (incl. lid) partly consists of FEP film only and partly of FEP film plus acrylic glass frame, the average radiation transmissivity experienced within the chamber volume was estimated as average of transmissivities of 50% FEP film only and 50% FEP film plus acrylic glass.

page 3173, line 4: "2005" should perhaps read "2006".

>> Correct. Will be changed.

page 3174, equation 14: An asterisk missing.

>> Will be corrected.

page 3179, equation 16: A minus sign missing.

5, S2754-S2760, 2009

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion



>> Will be corrected.

page 3185, line 13: "Ludwig et al.," should perhaps read "Ludwig,".

>> Will be corrected.

page 3186, lines 2-9: The pressure difference between outside and inside of the chamber is discussed and reported to be "generally less than 2 Pa". A comment should be added if the authors consider this insignificant. Pumpanen et al. (2004, ref. above) list various studies showing that pressure differences of as low as 1 Pa may cause errors in CO2 efflux measurements.

>> We cannot really judge whether the observed pressure difference is insignificant or not. The mentioned value is an upper limit of observed pressure differences at various positions in our chamber. The variations can be attributed to the fast air movement inside the chamber due to purging and mixing fans (because of the constraints concerning the effective mixing of the chamber volumes and the short residence time in the chamber, mainly for reactive trace gas species). Quantitative studies of this issue reported in the literature often used a laboratory setup with special (artificial) soil as described e.g. by Pumpanen et al. (2004). We did not have the opportunity to perform such experiments. However, the pressure effect is likely to depend on various complex factors such as soil type and texture, soil humidity distribution, the depth and tightness of chamber collars, and the depth distribution of the trace gas source (or sink). Such conditions are difficult to reproduce in the lab. In our opinion, a truly representative assessment of pressure effects is only possible under field conditions in comparison to independent measurement methods. The few results of nighttime CO2 efflux presented in Fig. 14 can only give a limited answer to this problem. Yet they indicate that no dramatic errors are to be expected in this application.

page 3189, line 14: "Davidson et al., 2007" missing from the reference list.

>> This is a mistake in the text. The reference has to be changed to "Davison et al.

5, S2754-S2760, 2009

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion



## (2008)"

page 3203, Table 3: Incorrect 10th percentiles are reported for CO2 and NO2.

>> Will be corrected.

Interactive comment on Biogeosciences Discuss., 5, 3157, 2008.

## BGD

5, S2754–S2760, 2009

Interactive Comment

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Interactive Discussion

