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Biogeosciences Discussions

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.

Anonymous Referee #2

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General evaluation

This paper presents an automated dynamic chamber system for continuous surface exchange measurements of some trace gases on low vegetation. The presentation consists of the description of measurement theory and technical design principles, augmented by a series of field, laboratory and numerical tests. While similar measurement techniques have been previously reported in a number of papers, the present study introduces a comprehensive system combining many advantageous features (residence time, materials, automation, number of compounds and parallel chambers) and thus can be considered a novel approach. The performance of the system is thoroughly

analysed by various tests and comparisons. These constitute a significant part of this study and demonstrate the applicability of the chamber system.

The topic of this paper is very suitable for the scope of Biogeosciences. The methods are described precisely and appear sound. The paper is exceptionally detailed and, while there are some minor problems with organization, reads well. I am pleased to recommend its publication in Biogeosciences and only have a few comments that are not central to the overall quality of the paper.

Major comments

(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).

(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, l. 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, l. 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*.

(3) Throughout the paper, the authors emphasize the long-term applicability of the

BGD

5, S2214–S2217, 2008

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, 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.

Minor comments

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

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

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.

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.

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

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

BGD

5, S2214–S2217, 2008

H for a fixed u*).

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').

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

page 3174, equation 14: An asterisk missing.

page 3179, equation 16: A minus sign missing.

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

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.

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

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

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