Biogeosciences Discussions, 2, S571–S579, 2005
www.biogeosciences.net/bgd/2/S571/
European Geosciences Union

Biogeosciences Discussions

Interactive comment on "Nitrous oxide emissions from a beech forest floor measured by eddy covariance and soil enclosure techniques" *by* M. Pihlatie et al.

M. Pihlatie et al.

Received and published: 21 September 2005

Comments to Referee 2

The authors wish to thank the anonymous referee 2 for very valuable comments to improve the manuscript. We have carefully considered each of them and corrected the manuscript when possible. Response to the referee comments is listed below (Whenever the referee is cited, the text is written inside quotation marks).

—

"As declared in the manuscript, the aims of the study were (1) to evaluate whether the EC technique can be used below a forest canopy to measure soil emissions of N2O, and (2) to compare the magnitude and variability of N2O fluxes measured by EC and chamber techniques. The first aim was not satisfactorily achieved in the present form of

the manuscript. As it was formulated independent of the second aim, the applicability of the EC technique under the specific circumstances should be demonstrated (as far as possible) independent of the chamber results. A useful test in this respect is e.g. the analysis of the peak in the covariance function as described by Wienhold et al. (1994) that also provides a check of the delay time between the vertical wind speed and the N2O concentration caused by the air sampling system (see specific comment below). The specific characteristics of below-canopy EC should be discussed in more detail considering published results e.g. by Wilson and Meyers (2001) about the variability of fluxes, the effect of longer averaging intervals, and about the influence of mean vertical wind velocity. Also the stationarity of the wind-flow and turbulence in the trunk space should be addressed since it was mentioned to be a major requirement for below canopy EC measurements (p.584 line 9)."

The authors agree with the anonymous referee that the first aim in the original manuscript was not satisfactorily achieved. We used the analysis of the peak in the covariance function as described by Wienhold et al. (1994) but his did not provide with more information on the behaviour of the measurement system. As the N2O fluxes were close to the detection limit of the TDL system, the noise in the data prohibited us to do further analysis. Such checks for the ability for below canopy measurements should be conducted in a forest with higher fluxes than in our study. Our data was insufficient to answer to that question.

"The discussion of the results and the formulation of the conclusions (including the abstract) should be more focussed and consistent. A major issue is the "hot spot" chamber among the six manual chamber locations. On one hand it is included to prove that "...the spatial variability in N2O emissions is greater than the temporal variability...". On the other hand it is omitted to demonstrate the good agreement between chamber and EC results (especially in the abstract!). The authors have to decide and explain whether an omission of the "hot spot" chamber for comparison with the EC method is meaningful or not."

BGD
2, S571–S579, 2005

The referee is correct in that omitting the “hot spot” chamber is not meaningful. We have rewritten the discussion and conclusions more concise and focused to the main results of the manuscript. The “hot spot” chamber is mentioned in the results, however, it is not excluded from the comparison between the chamber and the EC techniques.

“In a similar way, it should be clearly decided and explained in the text whether it is meaningful to use wind sector selected EC data (as presented in the abstract) for comparison with chamber results at the expense of temporal concurrence. The large variability of measured fluxes often lead to insignificant differences of mean results according to the applied tests, despite large relative differences of factor 4 and more (p.591 line 4-6, Figure 3). In addition at least some of the mean fluxes were not significantly different from zero (considering a 2-sigma detection limit). Under such conditions the statistical insignificance does not mean that the two techniques give comparable results, but that a meaningful comparison is not possible. Thus it might be necessary to quantify and discuss the uncertainty range within which a difference cannot be detected by the statistical test.”

In general, the comparison between the EC and the chamber techniques is uncertain due to the following reasons: 1) there was only very few, in total 11 half-hourly EC flux values, EC measurements from the direction where the chambers located, 2) the EC was measuring close to the detection limit, which created unwanted random uncertainty to the data, 3) the chambers were located only in one wind direction which was after the study found to be one of the least presented wind direction at the site. Despite these drawbacks, it is informative to sort the EC data to wind direction sectors and test whether such trends exist that could explain the differences in the fluxes. The referee is right in that the tests do not give statistical significant results in cases where scatter in the data is large. The discussion on the comparison between the two techniques and the use of statistical tests is rewritten to better reflect the above mentioned critics.

SPECIFIC COMMENTS

BGD

2, S571–S579, 2005

“p.583 line 11ff. The authors give an explanation for spatial N2O flux variability in agricultural ecosystems. But more important in the present context are possible reasons for spatial variability in forest soils.”

The factors causing spatial variability in N2O production in different soil ecosystems are mainly the same: soil available nitrate and ammonium, soil organic matter, pH and changes in these variables. In forest soils one additional reason for spatial variability can be fine root dynamics reported recently by Silver et. al. (2005).

“- Sections 2.2/2.3/2.4 Important information about the EC instrumental setup and data processing is lacking: (1) the total dead volume and residence time of the TDL inlet system including the dryer unit and the analyser cell, (2) how was the delay time between the vertical wind speed and the N2O concentration determined, (3) how large were short term variability/drift effects of the TDL sensitivity apart from white noise.”

1) The volume of the inlet system can be calculated using the length of the tubing and inner diameter to be 0.15 litters. The volume of the sample cell is about 0.45 litters. A reference to a study with the same TDL system, in which frequency response of the system was studied is now given.

2) A more detailed description of the calculation of the lag-time is now given in the manuscript.

3) The short term variability / drift were not considered to decrease the sensitivity of the instrument. In eddy covariance measurement the absolute concentration of the measured gas is not crucial but the ability of the instrument to measure short term fluctuations in the concentration. The drift in the TDL used in this study was very small.

“- p.586 line 1-10 This paragraph has to be clarified concerning the geometry of the analyser. Is the "sample tube of 1.5 m" the same as the "absorption tube" with length Ls? Alternatively the whole paragraph can be omitted, just keeping the literature reference.”

BGD

2, S571–S579, 2005

The detailed information on the instrumentation was removed from the text since detailed description is given in the paper of Edwards et al. (2003).

"- p.587 line 1ff. Since the applied rejection criterion reduced the EC data coverage tremendously, the scientific basis of the chosen threshold should be explained in more detail (especially because it obviously relies on unpublished data of a different site?)."

A more detailed description and justification of the data rejection criteria are now given. A reference to the manuscript in which the data rejection criteria is tested is given. This criterion is also tested using the CO2 flux data recorded during the measurements described in this paper, as is now stated in the revised manuscript.

"- p.588 line 16ff. The measurement protocol of the automated chamber is not totally clear from the given information "..three gas samples were taken at 40min intervals". Does it mean that the chamber was closed for 80min or 120min within the 3h cycle? In either case, the long closure time fraction over a one week period could have significantly modified the soil conditions (moisture, temperature) within the chamber. In this context it is also interesting (but not discussed in the text) that the variability of the automated chamber measurements significantly increased after changing from a 3h to a 12h cycle."

The measurement system of the automatic chamber was clarified in the text.

"- Section 2.5: What was the precision of the GC detector and thus the detection limit of the chamber fluxes (compared to the EC detection limit estimated in 2.4)?"

Detection limit for the GC measurements of 2 μg N2O-N m-2 h-1 was included in the text.

"- p.589 line 9ff. It has to be explained why the comparison of the methods was confined to one week (or two out of three days with manual chamber measurements)."

During this one week period, the automatic chamber was measured more frequently than later on. The intensive measurements provided with enough data points for daily

BGD
2, S571–S579, 2005

EGU

mean calculations for the automatic chamber. This was why the comparison of the two methods, the EC and the automatic chamber, was confined to one week only. The EC and the chamber techniques were also compared during two other days when both the automatic chamber and the manual chambers were operated.

"- Section 3.1 first paragraph: the results of the single exemplary cospectrum can be omitted since it does not give useful information about the quality of the EC measurements. Alternatively, exemplary results of covariance function analyses (see general comments) could be shown."

The figure of the spectra and the discussion is omitted in the revised manuscript.

"- p.590 line 6f. If the high frequencies are contributing more to the flux than in "normal" EC measurements, is there a considerable problem due to high frequency damping with the applied system?"

Even though the high frequencies contribute relatively more to the measured flux, the difference is not huge. As there is currently no theoretical basis to correct the damping for no correction is applied.

"- p.591 line 24ff. Despite the scientific importance of the topic, the discussion of the relation between N2O emission and soil NO3, NH4 and moisture contents is purely descriptive and qualitative. More clear and quantitative information, especially the correlation coefficient values with information about their statistical significance should be given for the dependences plotted in Figs. 5 and 6."

Correlation coefficients and statistical significances (p-values) are given in the Figs 5 and 6, and also in the text.

"- p.592 line 7 The statement should be omitted or modified, because the insignificant dependence on wind direction was mainly a result of the high variability/uncertainty of the flux values (as stated in the subsequent sentences). With the available EC data even a systematic variation of the flux by a factor of 3 could not be significantly detected

BGD
2, S571–S579, 2005

(see also general comments)."

The statement "Nitrous oxide emissions measured by the EC were independent of wind direction" was removed as suggested.

"- p.592 line 22f. This statement already occurs in Section 2.3 and can be omitted here."

The statement regarding data filtering due to low turbulence was removed as suggested.

"- p.592 line 24f. The relatively low coefficient of variation of 26% is said to be valid for the period 7 to 14 May. However, in Section 3.1 (p.590 lines 8-14) the same value in Table 1 is referred to as result for the entire measurement campaign. Which one is true? When discussing the temporal variability of the automatic chamber, a confinement to the one week, when variability was lowest, seems somewhat arbitrary."

This part of the text was modified so that when talking about coefficient of variation of the automatic chamber, a number for the whole measurement period was given. This changed the text very little since the CV% for the whole period for AC was only 45% as compared to 150% of the manual chambers and 257% of the EC.

"- p.593 line 28f. The statement that part of the high variability of the EC flux can be explained by the spatial variability of the soil emission needs further support and discussion. It seems to contradict other statements in the manuscript saying that the EC flux does not depend on wind direction and that the spatial variability is rather small-scaled."

The discussion on the spatial and temporal variability in the EC fluxes and causes for this variability is discussed in more details in the revised manuscript.

"- p.594 line 2-7 What is the conclusion here for the present study? What averaging time should be used in practice for the EC measurements? (cf. Wilson and Meyers, 2001)"

BGD

2, S571–S579, 2005

No general rule on the length of the averaging time can be given, as the conditions at different sites vary. Here we used 30 minute integration time for a single flux value, which is in the range of typical integration times (30-60 min). This captures well most of low frequencies contributing to the flux. The daily averaging of flux values was done in order to reduce the effect of instrumental noise on the fluxes. The length of this averaging period depends on the signal to noise ratio of the measurements and on the desired accuracy of the flux values. As the former depends on the flux rate as well as on the system characteristics, no general recommended averaging time can be given and the more complex studies on the subject are beyond the scope of this paper.

"- p. 595 line 7-12. This results of the footprint analysis with a reference to Fig. 1 should be given earlier in the text (Section 2.3 or 3) and not only at the end of the discussion."

The results of the footprint analysis were moved to the section 2.3 as suggested.

"- p.595 line 15ff. The statement that temporal resolution is less important than spatial resolution might be valid for forests but not for all ecosystems. In arid or fertilised ecosystems, a large part of the N2O emissions may occur in short pulses after fertilisation or rain events."

This was further discussed in the discussion.

"- Section 4 and 5. The negative characteristic of the EC method, that most nighttime data had to be rejected due to insufficient turbulence, should also be mentioned in the final discussion of the methods."

This remark was added to the discussion

TECHNICAL CORRECTIONS

- p.589 line 11f. rephrase to "Daily mean fluxes of the automatic chamber and the EC technique were compared using a T-test ..." - p.590 line 12 rephrase to "Variation of daily mean N2O fluxes, ..." - p.590 line 17 change to "...N2O emission to 20 ug N m-2 h-1 was measured ..." - p.590 line 19 change to "...peaked again on ..." - Figure 3

BGD
2, S571–S579, 2005

EGU

should be larger (same size as Fig. 4), different symbols for EC and manual chambers should be used

The above technical corrections were done.

Interactive comment on Biogeosciences Discussions, 2, 581, 2005.