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

Anonymous Referee #2

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GENERAL COMMENTS

The paper presents results of N2O flux measurements during a 5 week field experiment in a beech forest in Denmark. The main novel scientific aspect of this work is the below-canopy application of the eddy covariance (EC) technique to measure N2O efflux from the forest soil. Together with the simultaneous chamber measurements, a valuable dataset is presented that allows the study of several aspects of N2O emission from forest soil. The topics addressed are scientifically relevant and well within the scope of BIOGEOSCIENCES. 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 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. 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

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be necessary to quantify and discuss the uncertainty range within which a difference cannot be detected by the statistical test.

SPECIFIC COMMENTS

- 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.

- 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.

- 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.

- 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?).

- 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.

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- 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)?

- 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).

- 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.

- 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?

- 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.

- 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 (see also general comments).

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

- 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.

- 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.

- 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)

- 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.

- 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.

- 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.

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

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