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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "A case study of eddy covariance flux of N_2O measured within forest ecosystems: quality control and flux error analysis" *by* I. Mammarella et al.

Anonymous Referee #1

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MS No.: bg-2009-139 Special Issue: Greenhouse gas exchanges, carbon balances and processes of northern ecosystems

Title: A case study of eddy covariance flux of N2O measured within forest ecosystems: Quality control and flux error analysis

General Comments: The manuscript is aimed to assess performances and limits of eddy covariance (EC) technique in measuring N2O fluxes. The topic is of great interest because the correct quantification of nitrous oxide balance is important for its impact

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on global climate change. EC technique has been largely tested for CO2 and H2O flux measurements; on the other hand, applications to nitrous oxide flux are still poor and highlight large uncertainties and variability in the measurements due to the nature highly intermittent of the signal and to instrumental drift, typical of TDL and QCL spectrometers. In the manuscript two data sets have been analysed, both collected within the sub-canopy space of two different forests. Authors performed a careful evaluation of the main error sources of EC N2O fluxes by using already known methodologies (like the Allan variance concept to filter instrumental drift, or the co-spectral correction method to estimate the high frequency flux loss). The obtained results are interesting and can be useful to tune a standard methodology to correct routinely evaluation of EC N2O fluxes. On the whole, the paper is well written and well structured and the flow of text is clear and logical. In my opinion the obtained results are sufficient for a publication on BioGeosciences and I recommend to accept the paper after the following revisions:

Specific Comments:

1) Authors should revise the organization of section 3 that describes the used methodologies. In particular, some parts of the section are too poor and should be broaden with major details by briefly explaining the instrumental or physical effects that produce an error in the flux measurement and the procedures used to estimates or eliminate those errors. For example, I suggest to briefly explain Allan variance method (also in a separated appendix). Moreover supplementary explanations given in section 4 (that should contain only the obtained results!) should be moved in section 3; i.e., the dependence of the optical interference fringes on temperature (pag 11), or the explanation of the effect that produce the flux underestimation (peg. 14 "Systematic flux underestimation . . .. By using co-spectral methods).

2) DETRENDING OF DATA: At the begin of section 3 (from line 16 of pag. 7, to line 11 of pag. 8) authors wrote that they applied a linear detrending (LDT) to the signals to remove average values and trends. Moreover they applied an autoregressive running

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mean filter (RMF) to N2O signal to suppress TDL instrumental drift that produce low frequency noise (artificial effect). But at page 8 (lines 15-18) they also wrote: “For further corrections and validation of the fluxes co-spectra of sensible heat, CO2 (only for Kalevansuo) and N2O were calculated using fast Fourier transform (FFT) on linearly de-trended segments of 215 data points”; and at page 14 (lines13-15) “The N2O co-spectra show more random variability especially in the low frequency range, where contributions with opposite direction to the total covariance are measured and the effect of N2O signal drift is clearly evident”. This is a little bit confusing: Why the authors do not compute other statistics directly on N2O signal de-trended with an RMF? In my opinion the correct procedure is to firstly de-trend the data from those artificial effects that can alter the flux estimates (RMF for N2O signal and LDT for other signals); then to apply other flux corrections or compute other statistics on the de-trended data (co-spectra, flux-random uncertainty, etc).

3) (Pag. 11- line 20) Has been the relationship (alfa=-beta-1) already observed or it is a new result? Authors should specify and/or discuss that point in the text.

4) Authors used the spectral model given by equ. (1) to fit the sensible heat cospectra and to quantify the high frequency spectral loss of other scalar cospectra. In that model they used the reduced frequency n=fz/U. Then they scaled the frequency of cospectral maxima with n=fhc/U (hc:canopy height, U at z(?)) imputing the different maxima position in the two experimental sites to the different length scales of coherent structures that dominate the transport inside the vegetal canopy. I agree with that discussion, but not with the used scaling. In fact the phenomenology of the onset of coherent structures just above dense plant canopies can be explained through an analogy to Kelvin-Helmholtz instabilities observed in plane mixing-layers (Raupach et al., Boundary Layer Meteorol., 78, pp. 351-382, 1996). Dominant eddies result from a continuous hydrodynamic instability process produced by the inflected velocity profile in the upper canopy. These eddies have integral length scale of order hc, are advected downwind at speed U_hc (U at the canopy top) and their energy corresponds to the

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main spectral peak (Finnigan, Annu. Rev. Fluid Mech., 32, pp. 519-571, 2000). For this reason the correct scaling for reduced frequency is : n=fhc/U_hc. In fact using this scaling the position of the spectral peaks usually do not vary through the roughness sublayer.

5) The optimal high-pass filter time constant of 50 sec found for the two analysed data-sets can be related to some instrumental characteristics? Or to what?

6) (Figure 6) Has been plotted in those figures the absolute value of cospectra? In fact in figure 4 (are the same cospectra?) N2O cospectra exhibit variation of sign in the resolved frequency range. Negative values should be eliminated or differentiated also in figure 6. Has these negative values been considered in the application of the methodology used to estimate the high-frequency N2O flux reduction? How do they influence the obtained results?

Technical Corrections:

7) (pag. 4 –lines 8-12) The sentence: "Chamber flux data. . .. errors." should be moved at the end on the section: "Finally for validation purposes we compare the EC fluxes with those obtained by soil chamber technique. Recommendations how to treat data for post- processing are derived from the assumption that below-canopy eddy covariance flux measurements should match the temporal pattern and magnitude of chamber flux measurements, although also chambers are prone to systematic errors.

8) (pag. 6 – lines 6-7) "More details on chamber setup and data processing are given in Pihlatie et al., 2009." Has the cited paper been already accepted for publication? If not, probably could be better to give some brief information about chamber setup and data processing of the second measurement campaign.

9) (Figure 2) For an easily comparability of the two methods I suggest to uniform the scale of the horizontal axis for Allan variance and spectral density; for example authors could change frequency in time in figures 2c,d.

10) (Pag. 16, lines 13-15) In how many sub-records (N) has been divided the averaging period T for the calculation of the relative flux error? Has been the N2O signal correctly de-trended with an RMF before the computation of that error (see also point (2))?

11) (Figure 4) "The wind velocity was 0.8 m/s and o.6 m/s . . .." Are those U measured at z? What is the standard deviation? However, authors should change the scaling (see point (4))

12) About references authors have made a small 'mess'. In fact A LOT of papers cited in the text are missing in the list:

- Pag. 2, line 25 - (IPCC, 2001);

- Pag. 3, line 16 - (Ambus and Christensen, 2005; Silver et al., 2005);

- Pag. 3 line 19 – (Nelson et al., 2002);

- Pag. 3 line 23 – (Scanlon and Kiely, 2003);

- Pag. 7 line 14 – (Vickers and Mahrt, 1997);

- Pag. 7 line 20 – (Hernandez, 1986; Brodeur et al., 2008);

- Pag. 9 line 6 – (Lee et al., 2004);

- Pag. 9 line 12 – (Webb et al., 1980);

- Pag. 10 line 1 – (Lumley and Panofsky, 1964; Lenschow et al., 1994);

- Pag. 10 line 4 – (Vickers and Mahrt, 1997) already cited at pag. 7;

- Pag. 10 line 25 – (Hernandez, 1986) already cited at pag. 7;

- Pag. 13 line 24 – (Amiro et al., 1990);

- Pag. 14 line 3 – (Kaimal and Finnigan., 1994);

- Pag. 14 line 12 – (Cava et al., 2005) – more, I think the authors refer to Cava et

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al. 2004: “Organised Motion and Radiative Perturbations in the Nocturnal Canopy Sublayer above an Even-Aged Pine Forest”,Boundary Layer Meteorology, 112, 129-157, 2004.

- Pag. 15 line 3 – (Horst., 1997);

some references are incorrect:

- Pag. 3 line 33 – (Laville et al., 1999) is (Laville et al., 1997);

- Pag. 15 line 3 – (Moore et al., 1996) is (Moore et al., 1986);

and some references contained in the list are missing in the text (Conen and Smith, 2000; Fowler et al., 2007; Kroon et al., 2008; Rochette et al., 2008). Note that the missing references have made more tricky the evaluation of some part of the paper. Next time authors should pay more attention.

Interactive comment on Biogeosciences Discuss., 6, 6949, 2009.