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Biogeosciences
Discussions

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.

I. Mammarella et al.

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The authors wish to thank the anonymous Reviewers for valuable comments to improve the manuscript. We have addressed below each of the comments point by point. Whenever the referee is cited, the text is written inside quotation marks.

Reviewer 1

"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 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)."

1) In the final paper we will revise the Section 3 as suggested by the Referee. In particular we will move few sentences from the Section 4 to Section 3, and we will add a short Appendix about the Allan variance method (Appendix A).

"2) DETRENDING OF DATA: At 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 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 cospectra 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 (cospectra, flux-random uncertainty, etc).”

2) We will clarify this point in the final version of the manuscript. RMF detrending method was used before calculate the N2O flux and its random uncertainty. The detrending method is irrelevant when we want to estimate the high frequency flux loss. Moreover the co-spectra shown in Figure 4 are not high pass filtered, in order to highlight how the N2O drift gives a substantial low frequency contribution to the fluxes.

“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.”

3) The relationship (α = -β-1) was already reported by Werle et al.(1993), as already mentioned in the paper.

“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 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.”

4) We agree with the Referee that the appropriate reduced frequency in the roughness sublayer should be n =fhc/U_hc and this has been reported in several studies especially for spectral density of wind components. Unfortunately there are not available measurements of Uhc for these sites. Although the normalised frequencies estimated in our study (nm, Table 2) lack generality for the purpose of comparison with other sites/studies, however, they have been evaluated and can be applied for the purpose of making corrections in current study.

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

5) The RMF time constant of 50 sec refers to the average value we found, which was suitable for reducing the N2O signal drift effect on the fluxes. In terms of Allan variance analysis, such value can be seen also as the maximum integration time, for which the N2O noise level not exceed the one estimated at 10 Hz.

"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?"

6) Figure 6 shows also few negative values of co-spectra (closed down triangles). In the final version of the paper we improve the resolution of this figure. The negative values are probably due to the fact that the measured N2O fluxes are small and some spectral modes are more affected by random noise. In fact for this reason a common practise (assumption) in the co-spectral transfer function method is to use the co-spectral model (and not the measured ones) in order to estimate the high-frequency flux attenuation.

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."

7) We will modify the sentence according to the Referee's comment.

"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."

8) Pihlatie et al. (2009) reports details on chamber setup and data processing. The paper is still under revision, but it is accessible through BG Open Discussion. We made clearer in the Reference list.

"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."

9) We would like to keep the actual scale in the horizontal axis. The conversion from frequency to time is straightforward for the reader.

"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))?"

10) We used N=6. N2O signal was de-trended with an RMF.

"11) (Figure 4) "The wind velocity was 0.8 m/s and 0.6 m/s . . .." Are those U measured at z? What is the standard deviation? However, authors should change the scaling (see point (4))"

11) Yes, U was measured at the measurement height z. About the scaling, see the comment 4) above.

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

12) We apologize for this oversight. We will complete and correct the reference list.

Reviewer 2

"The article submitted by Mammarella et al. discusses the methodological issues involved in making accurate measurements of N2O exchange employing a tunable diode laser absorption spectrometer and eddy covariance technique. This is an important contribution as such measurements are rare. There is an urgent need to compile experiences from well organized campaigns of such measurements from different ecosystems. Currently, biogeochemical model validation for N2O exchange is being done primarily with the data measured using chambers. Chamber methods although very useful are limited by how often the exchange is measured. Most of the data presently available is gathered through manual chamber measurements and as such, the time resolution associated with the data available for model testing is too coarse to allow a proper validation of the model performance. To overcome this disparity in the time resolutions of model validation and observations available for validation, continuous eddy covariance measurements are indeed needed. In this context, the present submission is relevant. The paper is well written. I recommend that this paper be accepted for publication after the following specific comments are addressed."

"1. The authors mention in second section that the instrument was calibrated once during the set up time. Was this the only calibration done? Could the authors elaborate on why they thought that one time calibration is sufficient?"

1. Yes, this was the only "two point" calibration done. By design, the TGAS software (supplied with the TGA100 by Campbell Scientific) used the know concentration of the reference gas as a calibration factor when calculating the concentrations of unknown samples. According to TGA Reference Manual, the system should be very stable, and it has only an offset error caused by optical interference (fringes effect). In theory such offset error changes slowly in time (TGA Reference Manual). In this case it would not be a critical issue for EC method, and it can be easily removed by standard linear detrending procedure (LDT). However in our case we experienced offset drift changes faster than the typical EC averaging time (30 min). The reason of this is not yet fully understood. See also comment 3 below.

"2. Simultaneous transfer of the entities is an important consideration in the EC data processing. The authors mention that WPL corrections were not done as a dryer was used to dry the incoming sample. How effective was this drying process? Can the authors quantify this from their own data? This is important because N2O fluxes are of small magnitude."

2. A test was done in laboratory conditions by sampling ambient air of varying moisture conditions (outside air into the laboratory). The dryer was able to remove 95% of the moisture from the air samples.

"3. The authors observe at the end of the section 3 that the fringe effect was less frequent for the SORO site. Can the authors investigate more on this issue as to why the effect was less frequent at this site compared to the other site? What part of the set up at the two sites was different so that the SORO site showed less effect?"

3. The set-up of two TDL gas analyzers was very similar. In our opinion the fact that the fringe effect was observed more frequently in Kalevansuo than in Sorø could be related to different environmental conditions rather than differences in the setup. Optical interference fringes are due to small variations of the TDL optical properties caused by small changes of the instrument temperature. During the campaigns, both TDL systems were collocated inside the TDL box and the insulated enclosure cover, recommended by the manufacturer, was used in order to dampen diurnal temperature variations. However in Kalevansuo the forest stand was much more open than in Sorø and the TDL box was exposed to the direct sun radiation. For this reason, although we are unable to proof it, we hypothesize that the rate of related temperature change of the optical element inside the TDL was somewhat more serious in Kalevansuo.

"4. In the section on co-spectra, lines 23-25 are not clear ('with opposite direction'). Please clarify."

4. The co-spectral densities are normalized by the respective covariance values, and then one would expect all co-spectral modes having a positive sign. However, few points in the Figure 4b and 4d show a negative contribution. These values are due to the fact that the measured N2O fluxes are small and some spectral modes are more affected by random noise.

"5. The authors indicate that N2O uptake was evident at their site. Please provide magnitudes of uptake rates. Were the site averages shown in the tables inclusive of these uptakes? If yes, the magnitude and deviation from the mean of uptake rates should be discussed."

5. Recent publications indicate that N_2O uptake may be a real phenomenon in N poor ecosystems, such as in forests (e.g. Rosenkranz et al., 2006; Pihlatie et al., 2007). In this case study we observed occasional N_2O uptake values in 30 min runs in both the measurement sites. These negative values were included in the site mean calculations given in the tables 3 and 4, which represent net exchange estimations of N_2O over the whole measurement periods. As the EC measurements introduce large random error to the measurements, it remains unclear whether these data from Sorö and Kalevansuo give proof to a real "biological" N_2O uptake at the sites. We will modify the text to include discussion on the reasons behind the negative fluxes and processes possibly responsible for the biological N_2O uptake.

"6. Editorial correction – line 25 page 6950- change 'the one' to 'that'." 6. Done.

"7. Make sure that all abbreviations used in the paper are properly assigned at the first instance they occur in the paper." 7. Done.

"8. Page 6960, line 17 – change 'become equal to' to 'occur at'." 8. Done.

"9. Not all references referred to in the text are listed in the references section and some of those mentioned therein are not referred to in the text."

9. We will complete and correct the reference list.

References cited in the author answers:

Pihlatie, M., Pumpanen, J., Rinne, J., Ilvesniemi, H., Simojoki, A., Hari, P., and Vesala, T.: Gas concentration driven fluxes of nitrous oxide and carbon dioxide in boreal forest soil, Tellus 59B, 458–469, 2007.

Rosenkranz, P., Bruggemann, N., Papen, H., Xu, Z., Seufert, G. and Butterbach-Bahl, K. 2006. N_2O, NO and CH_4 exchange, and microbial N turnover over a Mediterranean

pine forest soil. Biogeosciences, 3, 121–133.

Reviewer 3

General comments: "This article aimed to address the required quality control aspects when measuring N_2O EC fluxes by tunable diode spectrometry. In addition, the possible errors are discussed in these EC flux measurements. This topic is important since only a few papers have been published in which the quality control is partly addressed of EC flux measurements of N_2O (e.g., Eugster et al., 2007; Kroon et al., 2007). These published articles noted that laser drift could possible cause an over- or underestimation of the fluxes. However, no thoroughly investigation has been done on the effect of drift on flux values. This manuscript addressed the possibility of filtering laser drift by a running mean filter. In addition, the drift is evaluated using several techniques, e.g. Allan variance and fast Fourier transforms. It is relevant to discuss the required filtering technique in the community before a standard methodology could be developed for EC flux measurements of N_2O. I recommend that this paper will be published after revisions. The manuscript should be written more clearly. In addition, some additional information should be included. Major/minor comments and technical comments will be listed below."

1) Introduction

"Page 6950, line 23: The greatest warming potential. The GWP of N_2O is indeed larger than the GWP of CH_4 and CO_2. However, there are some species with a larger GWP (see Table 2.14 of 2007 IPCC Fourth Assessment Report (AR4)).The sentence should be rewritten."

Page 6950, line 23: We will modify the sentence as "Nitrous oxide (N_2O) is the greenhouse gas having the greatest greenhouse warming potential".

"Page 6951 (line 28) – Page 6952 (line 18): This part should be rewritten. The following aspects should be written more clearly. The gap of knowledge, the objectives, the way in which the research is performed. For example: there have been published already some other papers in which the performance/suitability of spectroscopic techniques is evaluated (e.g., Eugster et al., 2007, Kroon et al., 2007). Euster et al., 2007 and Kroon et al., 2007 focus both on quantum cascade laser spectrometer N2O EC flux measurements. The author should indicate this in the introduction. In addition, the author should check if there are some articles available about the performance of TDL EC flux measurements of N2O. Then, the author should describe better the gap of knowledge and the related objectives of this paper (For example: 1. Detailed evaluation of the main error sources and uncertainties 2. Derive recommendations how to treat data for post-processing) Next, the author could tell how they will reach this goals. For example: Using the datasets ... over a period ... Some more small points which could be improved: For example: some parts are now written twice; that EC fluxes are compared with chamber measurements (at line 4-6 and 16-17). After the second objective, recommendation how to treat data for post-processing, the post processing elements which are discussed in this paper could be listed. Which parts are new in comparison with CO2 EC flux measurements?"

Page 6951 (line 28) – Page 6952 (line 18): We will rewrite this part in the final version of the paper following the referee's suggestions.

2) Site description and measurements

"Page 6952 – 6952. The same characteristics of both measurements campaigns should be given. Some examples: Coordinates, precipitation rates and mean temperature are not given for the first campaign and are given for the second campaign. LAI is given for the first campaign and is not given for the second campaign."

Page 6952-6952. We will add in the final paper these details for both measurement campaigns.

"Page 6954, line 12: Both TDL's were calibrated once during the measurement period? Did the author check if the calibration factors were constant in time?"

Page 6954. The "two point" calibration was done in the field only for the TDL system used during the Sorø campaign. Unfortunately the other TDL was not calibrated in the field, since it was shipped from the manufacturer factory directly to the measurement site. There, it was set-up by two technicians from the Campbell Scientific Inc. By design, the TGAS software (supplied with the TGA100 by Campbell Scientific) used the know concentration of the reference gas as a calibration factor when calculating the concentrations of unknown samples. According to TGA Reference Manual, the system should be very stable, and it has only an offset error caused by optical interference (fringes effect). In theory such offset error changes slowly in time (TGA Reference Manual). In this case it would not be a critical issue for EC method, and it can be easily removed by standard linear detrending procedure (LDT). However in our case we experienced offset drift changes faster than the typical EC averaging time (30 min). The reason of this is not yet fully understood. See also the comment 3 (and related answer) of the Reviewer 2.

3) Methods

3.1 EC measurement: data processing and corrections

"General: In the introduction, the author noted that a detailed description will be given of the main error sources and uncertainties. That's why; it is recommended pointing out the possible errors and uncertainties more clearly. For example: the author could start this section with how the fluxes are calculated (using LD and RM), then how the lag time is calculated and then they could list very shortly all possible errors, e.g. calibration error, low and high frequency response losses error, density fluctuations error. Then the author could describe the errors involved in this study and how they quantify theses errors/correct for these systematic errors."

General: We will add the referee's suggestions in the final version of the paper.

"Page 6954, line 23: Include the time period over which the linear detrending is performed." Page 6954, line 23: LDT was performed on 30 min periods. We will include it in the text.

3.2 Random error of flux estimates

"Maybe, the author could couple section 3.1 and 3.2 better with adding one sentence at the beginning of section 3.2. For example: after applying corrections for all systematic errors, there are still some random errors/uncertainties left. It will be interesting to include also the absolute uncertainty in 30 min EC flux values. This uncertainty can be derived using Businger et al., 1986.

Businger, J.A., 1986. Evaluation of the accuracy with which dry deposition can be measured with current micrometeorological techniques. Journal of climate and applied meteorology, 25, 1100-1124. In the present manuscript, only the uncertainties of sensible heat and N2O EC fluxes can be compared. The absolute magnitude as function of EC flux magnitude will be very interesting to add to this manuscript."

$$u_{ran} = \sqrt{(20Z/TU)}\sigma_{w'c'}$$

The Businger formula indicated by the Referee is based on the error analysis made by Lumley and Panofsky (1964) and Wyngaard (1973),

$$\delta_{\varphi} = \sqrt{(2\tau_{\varphi}/T)}\sigma_{w'c'}$$

where the integral time scale of instantaneous flux $\varphi = w'c'$ is simply parameterized as $\tau_{\varphi} \approx 10z/U$. Such parameterization, derived from surface layer spectra (Kaimal et al., 1972), definitely does not hold above and/or inside a forest canopy. The method we used in our manuscript to estimate the flux random error δ_{SE} (Eq. 2 in the manuscript) is straightforward and it does not make any assumption on turbulence field characteristics. Moreover in a recent study on random uncertainty of EC aerosol particle flux measurements, Rannik et al. (2009) shown that δ_{SE} and δ_{φ} are two different methods to estimate the same flux random error, and their values are expected to be approxi-

mately the same. In the figure 7 (upper panels) we have chosen to show the relative flux error ΔF (eg δ_{SE} divided by the EC flux magnitude). Moreover Figure 7 (bottom panels) clearly shows that most of the 30 min runs having a random error δ_{SE} larger than the EC flux magnitude (|ΔF|>1) are those having small flux values. However, in the final version of the paper we will report values of absolute uncertainty as suggested by the Referee.

4. Results

"General comments: In my opinion, the structure should be changed to improve the manuscript. Section 4.3 should be included in section 4.1. Both sections are devoted to how researchers should deal with instrumental drift. Section 4.2 should be included in section 4.6. "

The footprint section (4.2) will be moved to Appendix B. However we will keep separated the sections 4.1 and 4.3 (which will be 4.2).

4.1 TDL system stability and performance

"The author noted very well which requirements are needed before drift could be contaminate to the flux. Then it should be noted more clearly that first point 1 is addressed and than point 2. Point 1 is addressed partly by the Allan variance versus FFT comparisons. Point 2 is addressed using co-spectral analysis (section 4.3 which could be better included in section 4.1). As noted at line 7 of page 6962, at the Soro site there is a smaller laser drift effect. Therefore, it is recommended including also a Figure of the Soro site in Figure 1 and in Figure 2."

During Soro campaign the laser drift effect was observed less frequently than in Kalevansuo. This does not mean that it was "smaller", as the Reviewer said. At line 7 of page 6962, we just said that the high frequency end of N2O spectra in Soro show an apparent inertial sub-range, due to EC digital filter of TDL system used during the Sorø campaign. But the low frequency part (which is normally affected by laser drift) is very similar to that one found in Kalevansuo. In the final manuscript, we will include also an example from Soro site.

“Allan variance stability time is 50 s, however, it will be good to note some examples given in other studies (For example given in Nelson et al., 2004 and Kroon et al., 2007) Nelson et al., 2004. High precision measurements of atmospheric nitrous oxide and methane using thermoelectrically cooled midinfrared quantum cascade lasers and detectors, Spectrochimica Acta Part A, 60, 3325-3335.”

We will add these references in the final manuscript.

4.2 Co-spectra

“Page 6960, line 23-25. At the low frequencies there are negative and positive contributions? So what’s the net effect of the laser drift? Does it give a flux under- or overestimation? It will be important to note the effect on the flux values when using a 50 s RM, 100 s RM etc? If we use a 50 s RM do we miss real contributions which should be added to the flux? How could we correct for these missing contributions? What will be the correct data processing method? (Just a guess: Should we first check the Allan variance stability time, then performing a RM filter with that time and next correcting the missing contributions using sensible heat or other spectra??)”

Page 6960, line 23-25. Determination of a high-pass filter time constant is in practice a trade-off between removing unwanted trends in signal and minimizing low-frequency flux underestimation. The low frequency drift is due to artificial instrumental contribution as well as non-stationarity of atmospheric signal, and it can cause an under- or overestimation of the fluxes, depending on how the related N2O fluctuations are correlated with the vertical velocity fluctuations. On average the net effect of low frequency drift on the flux is to strongly enhance the run-to-run variability of the flux values. Such large random variability shows up even in the daily average fluxes (see Figure 8 Kalevansuo site EC-LDT), which are randomly distributed around zero. We agree with the Reviewer that after performing a high pass filter to the data (RMF), the correct data

processing method would include also the flux correction for low frequency loss. However, in practical, we did not include such correction in the final flux values, because a correction would require a priori knowledge about the instrumental interference structure and therefore is not recommended. As a summary, a general prerequisite for any measurement are high quality time series data unaffected by instrumental influence.

4.3 Flux systematic error

“State here that only the high frequency losses systematic error is discussed in this section. It will be of added value when the low frequency losses systematic error is also included to this section.”

We will specify this in the final manuscript.

“It should be stated if the separation distances are horizontal and/or vertical.”

We refer to horizontal separation distance between the sensors. We will specify this in the final manuscript.

4.4 Flux random uncertainty

“It will be a great added value when the absolute uncertainty of 30 min EC fluxes is also evaluated in this section. (See comment above with reference of Businger et al., 1986)”

See the comment above on Sec 3.2 Random error of flux estimates.

4.5 Comparison with chamber flux

“In my opinion, the footprint Figure could be skipped and a short summary of the footprints of both EC flux towers could be included in this section.”

The Sec. 4.2 (footprint) and the Fig.3 will be moved to Appendix B.

“Before comparing EC fluxes with chamber data, it will be important to note which corrections are made on the EC flux values. (For example: the high frequency response corrections as discussed in section 4.5 are these corrections performed on the used data in the comparison?)"

Yes, the final flux values used for the comparison were corrected for high frequency loss. We will make this clearer in the text.

"Is it fair to remove the fluxes with a relatively large uncertainty?"

In general such selection criteria based on flux random uncertainty should be used with caution, if our interest is to understand potential relationship between the error estimates and observation conditions. The aim of this analysis in the manuscript was just to show that the N2O flux random error is larger than the one estimated for other EC fluxes (e.g., sensible heat flux). We observed that the relative flux error distribution is asymmetric, and the uptake fluxes of N2O have larger random uncertainty. Then the use of such selection criteria has a notable effect on ensemble average flux values, as it can be seen from the Table 3.

"Could you explain why the EC fluxes and chamber measurements are much more comparable at the Kalevansuo site than at the Sorø site? Is this possible due to missing contributions of eddies with time scales larger than 50 s? The EC values are smaller than the chamber flux values. Could this be explained by the missing eddies with time scales larger than 50 s?"

The EC values, calculated by using LDT method, are also smaller than the chamber flux values (Figure 8). Then difference between EC(RMF) and chamber fluxes cannot be fully explained by the missing low frequency contribution. We believe that such difference is more related to the fact that in Sorø there was only one big automatic chamber, and the comparison between the two methods is uncertain due to the high spatial variability in N2O emissions at the measurement site (Pihlatie et al., 2005).

Conclusions

"Page 6966, line 6-9. They are not really in good agreement. It will be better to give a

percentage of the difference between both emission estimates."

Page 6966, line 6-9. We will modify this sentence.

Table 1 "Could the author indicate if the spatial separation was vertical/horizontal or both?"

Table 1. This information is given in the revised paper. See also the comment above.

Figure 1 "Both Figures should be made in the same lay-out. In addition, the unity of T should be included."

Figure 1. Done.

Figure 2 "Include unities (For example Allan Variance [ppb2]). Include a -5/3 line in the middle range of Figure 2c. in addition, the measurement site could be noted in the caption."

Figure 2. Done.

Figure 3 "The average used z/L value could be indicated in the caption."

Figure 3.Done.

Figure 8 "AC should be explained in the caption."

Figure 8. Done.

Further technical comments and missing references will be included in the final version of the manuscript.

References cited in the author answers:

Kaimal, J.C., Wyngaard, J.C., Izumi, Y., Cote, O.R.: Spectral characteristics of surface-layer turbulence. Quart. J. Roy. Meteorol. Soc. 98, 563–589, 1972.

Lumley, J.L. and H.A., Panofsky: The structure of atmospheric turbulence. Interscience, 239 pp.

Rannik, U, Mammarella, I., Aalto, P., Keronen, P., Vesala, T., Kulmala, M.: Long-term aerosol particle flux observations Part I: Uncertainties and time-averaged statistics. Atmospheric Environment, 43, 3431-3439, 2009

Wyngaard, J.C.: On the surface layer turbulence, Workshop on micrometeorology, D.A. Haugen, Ed. Boston, AMS, 101-149

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