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Interactive comment on "A fertile peatland forest does not constitute a major greenhouse gas sink" *by* A. Meyer et al.

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We thank the reviewer for the thoughtful and constructive comments, which certainly helped to improve the manuscript. Following the comments and advice, we completely revised the manuscript with particular focus on the calculation and presentation of uncertainty estimates. In order to ensure clarity, uncertainty is now discussed in detail for all given fluxes in the new section 4.5 in the discussion. We extended the discussion on uncertainty of all and added new uncertainty estimates for tree biomass, chamber and EC fluxes. Also, we completed the text by explaining in more detail how uncertainty estimates were calculated and the respective errors propagated. We clarified the previously imprecise wording where necessary. Additionally we further analysed the EC flux footprint and added a new figure 1c where flux footprints are distinguished

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between summer and winter periods.

Below we address the general and specific comments. For clarification we attached the manuscript with marked changes as a pdf file and figure 1 which we complemened by a new figure 1c to demonstrate the footprint of the NEE flux for different time periods.

General comment 1 and 2 “Two different approaches are used to estimate the CO2 balance, which in principle can be considered a strength of this study. However, the way the estimates are presently interpreted actually constitutes a major weakness. While it is obviously useful if the magnitude of different component fluxes can be determined, the interpretation of the results appears problematic, as the overall balance obtained this way significantly differs from that measured with micrometeorological techniques. In this respect, the authors should formulate the discussion of the results and the final conclusions more carefully. The uncertainty estimates (and how they are defined) play a key role here. For example, in the beginning of Section of 4.2 it is stated that the approaches “gave different results”, but due to measurement uncertainties the results are “actually not different”. This sound very vague, as does the discussion on the nature of the error propagation scheme that follows. 2) Related to the previous comment, the uncertainty estimation procedures should be made more explicit. It is not clear how the standard errors are calculated for Eq. (13) and what “the respective components” (p.5121, l.14) are. Based on the text and the data presented in Table 4, I assume that the error propagation principle is applied with standard deviations and that these uncertainty estimates only represent the variation between the three stations. The statistical rationale/test adopted for comparing the mean +/- s.d. values obtained in this way should be presented.”

We now thoroughly revised all data, their uncertainty estimates, and completed these where necessary. For N2O fluxes, the uncertainty estimates were completed by including a procedure to account for the uncertainty induced by gap-filling (L 465). For tree growth, the measurement uncertainty was considered (L 356). Uncertainties of respective flux terms are discussed in more detail, e.g. for biomass (L 710), N2O fluxes

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(L 820) and the general difficulty in comparing biometric data with flux data (L769).

General comment 3 “The geographical context of the study should be defined more exactly. The introduction is very much focused on Sweden, while Finnish studies are used for comparison. In Section 4.2. some other studies are mentioned but not considered comparable because “site conditions differ considerably”. The authors should define the conditions that their results represent and outline the region within which comparable organic soils can be found.”

The main focus of our study is on nutrient-rich, drained, afforested (conifer-dominated) organic soils in the boreal and hemi-boreal zone. In order evaluate the impact of the nutrient level on the GHG flux we also take fluxes from forests on poor organic soils into consideration. For direct comparison of fluxes, we consider conifer-dominated forests on organic soil in the hemi-boreal to boreal zone. However, the impact of changes in environmental factors like drainage has been demonstrated to be comparable between sites in the boreal and cold-temperature zone like the UK, therefore we also take studies from UK into consideration. We modified the discussion to clarify the exact geographical context and restrictions with regard to soil/vegetation properties (L 588).

Specific comments

- 5109 1 Which drained organic soils, those in Sweden?"

This applies to drained organic soil in general, irrespective of the climate zone.

- 5109 24/25 These ranges are based on a limited set of studies, cf. Maljanen et al 2010)"

In accordance with the journal guidelines excessive citations should be avoided, therefore we choose the references which reflect the range of fluxes given in literature. For completion, we added the review paper by Alm et al. (2007), which summarizes the current stage of knowledge about CO2 emission from afforested peatland sites (L57).

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- 5111 4 Lohila et al. measured N2O and CH4 fluxes with chambers rather than using micrometeorological methods.

We refer to the measurement of the CO2 flux in Lohila et al (2011), which was determined by micrometeorological measurements. We clarify that in the text (L105).

- 5111 10 and elsewhere: Which GHG compounds?

In L102 we have defined GHG emissions as emissions from CO, N2O and CH4.

- 5111 10 Minkkinen et al. (1999) is probably not an appropriate reference here, "Klemedtsson et al. (2005) found ... (Klemedtsson et al., 2005...)" sounds awkward; please rephrase. What is the geographical coverage of these data?

We consider Minkkinen et al. (1999) an appropriate reference because they could show that the C balance of organic soils became positive with increasing nutrient levels. As mentioned above, this study was conducted at an organic soil in the hemiboreal to boreal zone. Naturally, we restrict the comparison to other studies to the ones conducted in the same principal climate zone, as well as vegetation zones and we discuss differences if relevant. Klemedtsson et al. (2005) is based on a study of Swedish sites is therefore directly comparable to the presented field site.

- Section 2.1, 5112 Please indicate the forest height.

Forest height and LAI were added to the site description: "The dominant and co-dominant Norway spruce trees were 22 – 25m in height and the average LAI was 5.1 m2 m-2 (Dewar et al. 2012)." (L 158)

- 5112 23 'Please explain 'BA' (defined on p.5118).

We are sorry for the missing explanation of BA (=basal area), the definition was included. (L156)

- 5116 8 Which fluxes?

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The whole paragraph refers to the measurement of the NEE (defined as CO2 flux in L28 as stated at the beginning, so the annual flux sums refer to CO2 (L 257).

- 5116 12 Lindroth et al. do not provide a full description of the data post-processing procedures. More details should be presented (e.g. WPL term, compensation for high frequency attenuation).

We added more details on data post-processing procedures from L259 on.

- 5116 16 Unclear what is meant by 'spikes' here.

We refer to the CO2 flux spikes (peak outliers)

- 5116 17 How large was the storage term as compared to the eddy flux?

The annual average CO2 storage term was on average 2 % of the EC flux. We added more information regarding the storage term from L274 on.

- 5116 18 Is this "biotic flux" the same thing as NEE?

Yes, but for clarity we changed it to NEE.

- 5116 20 Unclear what is meant by "eddy covariance criteria"

By eddy covariance criteria we refer to the threshold value of the friction velocity for which the mixing was well developed. We clarified this in L279.

- 5117 13 The Kljun et al. model (parameterisation fit) also works in stable conditions, at least up to z/L<1. The Kormann and Meixner model employs stability correction functions that in principle are not valid beyond this limit either.

In order to evaluate the footprint more thoroughly we applied a new footprint model by Schmid (L305). Thus, the information on the previously used approach was deleted.

- 5118 25 Please describe the chamber measurements.

We added general information on the site set-up in the site description (L178) and com-

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plemented the methods with more detailed information on the determination of ECO2 (L 384-395). Information on the instrumentation is given in table A1 in the appendix. Additional details of the chamber technology used for determining the N2O and CH4 fluxes were added in section 2.7.

- 5119-2 Why is root litter decomposition (R_LR) excluded from Eq. (10)?

As a consequence of the trenching experiment, all roots (including all coarse roots and fine roots) are cut and will be decomposed by microbes. The flux from the decomposition is quantified by the term Rdecay according to the function by Ngao et al. (2007). We cannot exclude that minor parts of older root tissue might still be decomposed in the soil matrix, however, the contribution from this will be minor compared to the huge flux from fresh root litter by the trenching. Furthermore, roots cut by the trenching will not response to a litter production model as determined by intact soil cores. Root Litter is therefore not explicitly included as an extra term in eq. 10 (RLR) since this would lead to double accounting. Please see also our response to the comment by P Ojanen.

- 5119-25 Eq. (11) is confusing. The constant 0.997 actually represents the decay time (in years) and corresponds to the measurement period of Ngao et al. Please clarify.

We are sorry for the confusion about the -0.997 constant which represents the relative time period (364 days : 365 days) for which the decay function of Ngao et al. 2007 was determined. We simply overtook this value since our measurement period also covered about one year. In our experiment fine root biomass was determined on December 7th 2007 and we quantified the decay for the whole of 2008, the value should actually read -1.065 (389 days:365 days). The difference though has only a negligible effect on the result.

- 5121-13 NEEcalc has not been defined.

We regret the missing definition of NEEcalc, which has now been added in section 2.3 after equation 6b (L239).

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- 5121-21 Please indicate the measurement height (1.5/22m?).

The instrumentation table A1 now includes this information.

- 5122-10 The discussion of source areas is too qualitative and vague ("in general", "occasionally", "prevailing wind direction"). The conclusion that the measured fluxes were "certainly" dominated by the forest stand needs more justification.

The analysis of the footprint has been repeated in detail and is now presented in Figure 1c and described in the text (L304, L514 and further discussed in L747. Since the fraction of fluxes with footprint within a distance of more than 350m from the tower was only minor (as can be seen in the new Figure 1c) the impact from areas of different vegetation and soils can be neglected.

- 5126-9 A value of 8.2 is not within a range of -0.8 to 6.7.

The sentence was rephrased (L619)

- 5126 17 The estimate of random error requires a reference.

The reference was added (L740)

- 5127 1 The uncertainty estimate depends on the number and length of gaps, as well as on the gap-filling technique adopted. While most estimates reported by Moffat et al. are within the range cited, a more thorough discussion should be presented, as no proper uncertainty calculation is presented in the paper for the EC data. Moreover, the value of 15% adopted may not represent a conservative estimate, as only a few error sources are considered.

We have now added details about the gap-filling method used (L275) and also about the error estimated induced by the gap filling (L299): "Gap filling error estimates were done by randomly removing a certain number of measured values, filling them according to the gap filling method used, and then comparing the estimated gap-filled values with the measured values. This procedure was repeated 10 times, and the mean gap

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filling error was estimated for day and night values". Furthermore we highlighted areas in Figure 2 to demonstrate for which time periods the data needed to be gap-filled.

We have estimated the error caused by gap filling (Methods L299, Results: L507): "The average gap filling error was estimated to 6 % and 14% for daytime and nighttime values respectively, corresponding to an uncertainty of ± 13 g C m-2 for all gap-filled data. This corresponds to an uncertainty of ± 6.5% of the total annual flux". These values are further discussed in the discussion section (L731).

- 5127 5 This kind of discussion is too general to be really useful. A quantitative estimate could be calculated for the source area effect by using the footprint model described previously.

We thoroughly revised this part and included some new numbers on the flux footprints to support our findings (L737-746)

- 5127-14 It would be trivial to check the relationship between NEE and wind direction from the existing data.

We rephrased this part since the footprint model indicates that for more than 90% of the time fluxes derive from a distance within 100m around the tower (L750). He spatial overlap between biometric and EC data was therefore very good.

- 5131 1 It is illogical to state that "the site is either a GHG sink or source, depending on the approach". Whether a site is a sink or a source obviously does not depend on measurement methods.

The sentence was rephrased (L847)

- Table 3 The tree growth rate at Kalevansuo is incorrect. The NEE value for Alkkia is from Lohila et al. (2007), not Lohila et al. (2004). The first SOM respiration value (-0.8)for Alkkia should be removed. It seems to be taken from Lohila et al. (2004), but that paper deals with an agricultural peat soil growing barley and grass.

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We regret the typing error in Table 1 column 1, which cited Lohila et al. 2004 instead of 2007. We corrected values where necessary.

- Table 4 If the CH4 flux is -4.4 kg CH4 ha-1 yr-1, then wouldn't this correspond -0.1 tCO2eq ha-1 yr-1?

Following up on the comment from A.Freiberger, we changed all units of Table 4 into t C ha-1a-1 to make the overall data consistent. The CH4 flux then becomes -0.03 t Ceq ha-1a-1 (in Table 4 rounded to 0.0)

- Table A1 What is 'flux integration'?

We rephrased flux integration to flux calculation

- Fig. 2 It would be useful to indicate if longer periods of NEE data are based on gap filling. Do these NEE data include the storage term?

In the revised Figure 2c we indicate the time periods for which data have been gap-filled. It can be seen that time periods usually covered few days except for few periods of about 2 weeks. The data include the storage term (L270).

Technical corrections - 5110-5113 all corrected

- 5116 16 A wrong unit for friction velocity Corrected

- 5117 12 A wrong Kljun et al. paper in the reference list

We applied a new footprint model so that this part was changed

- 5122, 5132-Fig 3 all corrected

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C4662/2013/bgd-10-C4662-2013-supplement.pdf

Interactive comment on Biogeosciences Discuss., 10, 5107, 2013.

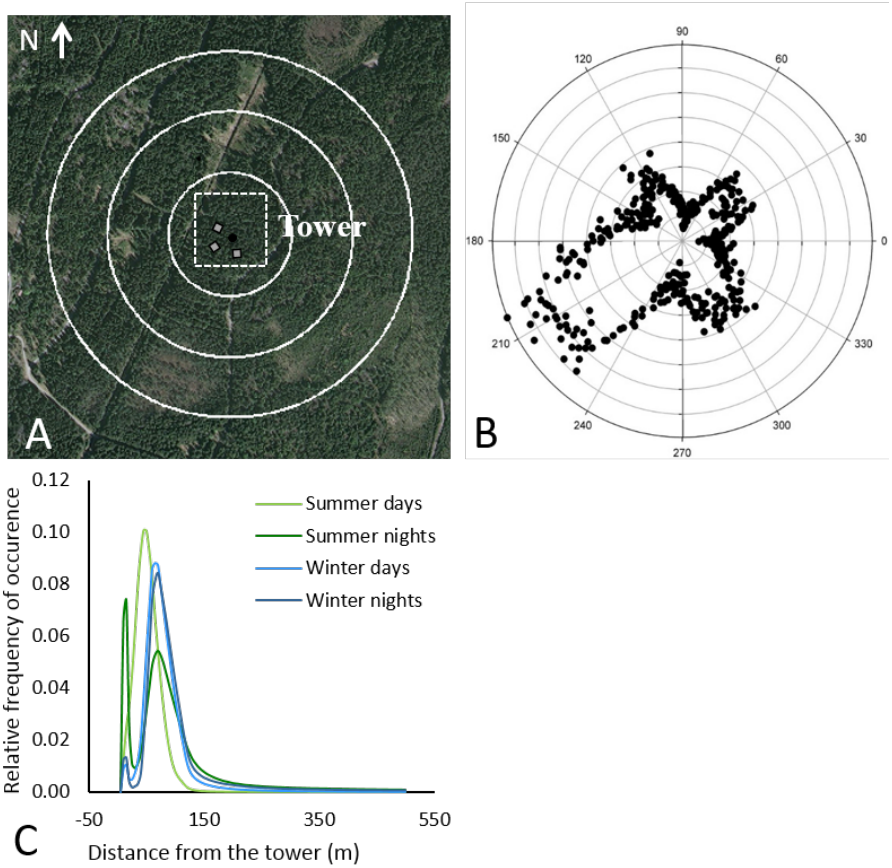

Fig. 1.

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