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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 critical but very constructive and helpful comments. We thoroughly revised the manuscript and addressed all comments accurately. The structure of the manuscript was modified and a new section 4.5 in inserted, which comprises the discussion on uncertainty of the different flux components. This allows for a more focussed discussion on the qualitative and quantitative aspects of the fluxes. Generally, we paid major attention to the methods and discussion section. The description of applied methods was completed so to make clear for the reader how data were derived. Furthermore, we improved the calculation and description of uncertainty estimates. We are certain that the manuscript improved considerably and hope that the revised manuscript meets the reviewers' demands. In the following, we address all general and

specific comments. The manuscript is attached as a pdf file with marked changes in order to better visualize the changes made. Also, Figure 1 is added to demonstrate the new footprint analysis.

General comment 1 " By reporting only highly synthesized and integrated data (only annual values of fluxes) with abbreviated methodology section, it is not possible for the reader to evaluate the rigor of different data processing steps. While most of the methodology may have been described in earlier publications, the key points of all methods should be mentioned here, too."

We regret that the description of applied methods was incomplete. We added details on the general site set-up, on the measurement design and processing steps of the dark and automatic chamber technique, and the EC technique in the text (end of section 2.1, section 2.2, section 2.7). Details on the EC method have already been listed in the appendix in Table A1, which was clarified in the text (L 248). Following comments from other reviewers, we investigated the sensitivity of CO2 and N2O fluxes to soil temperature and water table depth. CO2 fluxes showed a strong response to soil temperature, which is why we applied a model to extrapolate CO2 fluxes to the whole year. The description of the regression model and flux extrapolation is given in L 444.

General comment 2 " The use of different techniques to cross-validate one another is potentially a great strength of this study. However, with limited methodological detail and overly sweeping generalizations, the mismatch between the two methodological approaches is currently not adequately analyzed and explained. The potential for mismatch between biometric, chamber and eddy covariance measurements is well documented. For example, Barford et al. (2001, Science) and Gough et al. (2008,AFM) are two classic studies discussing the reasons behind the phenomenon. The current study did not mention the main source of such differences – interannual variability in belowground allocation. It should perhaps be added to the list of potential causes of the mismatch. I recommend that a section in Discussion be explicitly Dedicate to evaluating the potential sources of error and uncertainty. Right now this is cursorily done in sections 4.2.1. and 4.2.2., but not in sufficient rigor and in a systematic manner. Furthermore, some assumptions are treated as a fact, with no uncertainties or errors considered. Other components are assigned an arbitrary uncertainty level. At the very least, these values should be justified. As a preferred option, all assumptions should be critically evaluated, the performance of all gapfilling and interpolation models should be evaluated and presented. In the end, the authors probably want to be able to say that both approaches are consistent, given the uncertainty of measurements and gapfilling. Right now, this cannot be done, because the errors are not evaluated in a systematic manner. Additional and editorial comments are included in the amended PDF document."

The manuscript was carefully revised with regard to the calculation of uncertainty estimates and the presentation of these in order to improve the comparability of the two approaches. For N2O fluxes a procedure to quantify gap-filling uncertainty was included, for tree growth the uncertainty induced by potential measurement errors was added. We took the studies of Barford et al. (2001) and Gough et al. (2008) into consideration (L768). Generally, we paid special attention to the wording of interpretations to justify our conclusions.

Specific comments: - 5108 – 14/ 25: what is 'optimum growth phase'?

The C assimilation by trees has been shown to be dependent on tree age. Gower et al. (1996) estimated that boreal spruce forests have their maximum annual NPP at an age between 50 – 70 years. By the term "optimum growth phase" we referred to the age of the Skogaryd research forest of 60 years which indicates that the forest is likely to be at its maximum C assimilation. We agree that the term "optimum" is imprecise and rephrased the respective sentences to "the forest was probably in its phase of maximum C assimilation or shortly after " (L32). - 5108-25 what is meant by this? Please reword. The site has been shown to emit high GHG fluxes, i.e. soil C and N losses, which are compensated by the high C assimilation in the trees. However, the forest of about 60 years is likely to be in the phase of maximum C assimilation, which

implies that the compensation of the soil effluxes by the tree C uptake will probably decrease. Consequently, it is only certain for the period of maximum C assimilation that the site acts as a C sink. We reworded the sentence for clarification (L40)

- 5110-25 citation missing

The meaning of GPG was clarified (L 87 and 97)

- 5112-4 this is not the biggest problem of eddy covariance

We agree and have added the sentences below: (L135) "The largest problem with eddy covariance measurements is that they are conducted from usually only one tower, thus there is no replication. The source area (foot print) of the flux varies dynamically in relation to turbulent conditions; consequently foot print areas are generally smaller during days than during nights."

- 5112 please give LAI or canopy height

Canopy height and LAI was added (L 158)

- 5113-25 in each of the 6 plots?

No, only at two plots per station. The number of sensors and some more details on the measurement of soil parameters were added (L181)

- 5113-26 how was this derived?

Since TDR data were not used in the final manuscript, this part was deleted and replaced by a description of how water table depth was measured (L190). Furthermore the daily variation of water table level was included in Figure 2.

- 5115 9/10 Not necessarily. In any given year it may well differ.

We agree with the reviewer that assuming steady state for one particular year might not be applicable and potentially introduces a bias. Not assuming steady-state for the calculation of the soil C balance would change equation 6 to:

NEE = AGBinc + BGBinc + LL + LR – (RSOM + RLL + RLR)

so that the respective litter production from roots, LR, needs to be quantified by a model which potentially introduces a bias due to uncertainties in root turnover rates. Furthermore, RSOM was estimated by a trenching experiment so that the measured total efflux ECO2 comprises the flux from decomposing trenched roots (RSOM = ECO2 - Rdecay - RLL). By that a major problem arises: the measured CO2 efflux ECO2, and consequently the calculated flux Rdecay derive from the artificial experimental conditions from the trenching. A model assumption of LR though would derive from an assumed undisturbed ecosystem since applied root turnover rates derive from actively growing roots. Thus, when both fluxes, Rdecay and LR are considered for the overall soil balance, we combine two fluxes deriving from “different systems” which will exclude each other: Roots that are trenched will not produce litter according to the modelled LR and vice versa. Of course, we cannot exclude that root litter from previous years is still present in the soil matrix and adds to the measured efflux. We are convinced though that calculating the flux from root litter by a model will lead to a higher bias due to the above-mentioned reasons. Since we accounted for the effect of Rdeacy, the inclusion of LR will likely lead to an overestimation of the total litter input to the soil. In order to elucidate the uncertainties induced by the assumption of steady state we alternatively calculated the soil flux by considering root litter but neglecting the influence from trenched roots (as we are convinced that both fluxes cannot be considered together). This procedure is presented in the response to the comment by P.Ojanen and demonstrates, that this alternative calculation not assuming steady-state is very sensible to chosen root turnover rates.

- 5115-25 key features should be listed here, even if identical

The instrumentation used for the micrometeorological methods is listed in detail the Table A1 in the appendix. We rephrased the sentence so that this becomes clear. (L248)

- 5116-24 grouping data into discrete bins like this opens the possibility of gapfilling artifacts. From the step changes in NEE, this seems to have happened here. Maybe not, but a rigorous analysis of the goodness of fit and model bias need to be presented. Without it, the results presented here are not defensible.

We added a new gap filling analysis in which we quantify the error induced by the gap-filling (L301) to be in the order of 6-14% (L507). We further elaborated details on the total uncertainty of EC measurements as discussed in the discussion section (L732-756) The step change in NEE was most likely caused by rain events at the time of the “step change” which reversed the water limitations of the trees. This is demonstrated by the inclusion of the water table level in figure 2 (especially July and August 2008), in which we also indicate the time periods of the NEE flux which have been gap-filled.

- 5118 on determination of tree growth, plot design and fine root growth: how many plots, spread over how large an area? unclear, please explain the location and size of different plots better before getting to the corrections

We are sorry for the imprecise description and rephrased the text in the manuscript (L 327). Furthermore, we amended Figure 1 to indicate the area which was sampled for tree growth indicators. In 2007 and 2008 dbh was measured at 16 circular plots of 0.01 ha each which were evenly distributed over the area indicated in figure 1. The total area sampled was 0.16 ha.

In 2010 all trees within the rectangular area indicated in figure 1 (90x90m) were sampled. The basal area of these trees was on average 23% higher compared to the trees sampled within the small plots of 0.16 ha which is probably due to the small sampling area of the small plots. Regression analysis showed that basal area explained 50% of the variation of tree growth so that a correction as described in the manuscript was applied.

- 5118 reword, the implied causality is confusing. Was Minkkinen’s work done at the same site?

The biomass functions presented in Minkkinen et al. 2001 are based on the equations by Marklund (1988) which have been developed for spruce, pine, and birch trees on mineral soils based on an extensive field study in Sweden and which have also been validated for sites in Finland (Finer 1989, Minkkinen et al. 2001). Thus, tree species and climatic conditions of the model equations apply to the study site Skogaryd. Minkkinen et al. (2001) further developed the biomass equations for forests on peat soils which have been shown to allocate more C to belowground plant parts (Lahio and Finer 1996). (L347) The equations by Minkkinen et al. (2001) though only consider fine roots bigger 1 cm, which will lead to an underestimation of the total tree biomass if not considered otherwise. To correct for this, we assumed fine root biomass increment (<1cm) to correspond to the annual increment in needle biomass. We based this assumption on field observations, which found a proportional relationship between the biomass of fine roots and needles. He et al. (2012) and White et al. (2000) showed that the ratio between the annual C increment of fine roots and of needles ranges between 0.66 and 3.3 kg C kg C-1 for conifers. The majority of given values for spruce-fir forests ranged between 1 and 1.5 kg C kg C-1. Due to the wide range of values, we assigned an error estimate of 300% to the fine root growth

- 5119-23 how does this functionally quarantee more detailed understanding? could it just overparameterize the model?

L 397: In the study of Ngao et al. 2007 root decay functions were established from root biomass which was determined from soil cores. The sample size was only small - the cores had a diameter of 8 cm and sampled the soil to a depth of 15cm. Ngao et al. 2007 state that their soil core samples consisted mainly of fine roots which probably derives from the low sampling depth. Thus, their decay model widely neglects the impact of coarse roots. In our study the CO2 flux from decaying roots has to be determined for an area of 0.19m2 (=collar area) up to a depth of 60cm (= depth of the trenching frames) so that the impact of coarse roots becomes highly important. Since the Ngao et al. function widely neglects coarse roots, a modification of the equation towards a

parameterization for coarse roots is therefore necessary in order not to underestimate the overall CO2 flux.

- 5120-15 The assumption of linear accumulation since afforestation 60 years ago? this is a rather broad simplification, and potentially a source of error.

We agree that the assumption of a linear C accumulation in the litter layer since afforestation induces a potential uncertainty since year-to-year variation might differ; however, precise quantification is hampered due to limitations of methods currently available. Yet, the approach applied requires the quantification of litter layer C. A linear accumulation since canopy closure is more likely, which we therefore calculated. In order to test the sensitivity of the calculated efflux to this assumption, we changed the assumed canopy closure year between 10 and 20 years, which changed the total soil efflux at most by 0.9 t Cha-1a-1 (L 783).

- 5122-22 the annual sum is probably heavily influenced by the high uptake in June and July. Did you check that the step changes in NEE are not the artifact of gapfilling routine? The level of detail about data coverage and quality assurance protocols of gapfilling technique is insufficient and does not allow the reader to follow

As discussed in response to the question regarding 5116-24, the step change was not induced by the gap filling as can be seen from Figure 2. In between gap-filled time periods there are always non-gap filled data which demonstrate that the NEE was rather influenced by the drought stress and its reversion.

- 5123-10 Then why mention it in Methods and Discussion (p 5119, l 24 and after)?

We included mycorrhizal mycelia in the methods and result section in order to demonstrate that we considered them in our balance calculation and that their impact on the CO2 flux at this site is negligible. We deleted them from the discussion.

- 5123-19 what is the gapfilling uncertainty for the plot-based estimates? With large variance like that for N2O, additional description of the sources of variability is necessary. please elaborate.

In order to quantify the uncertainty induced by gap-filling we calculated the uncertainty of each gap-filled day by taking the standard deviation of the data used to fill the gap into account . The annual uncertainty was calculated by summing up the uncertainties for all gap-filled days. Total flux uncertainty was derived by taking the uncertainty of measured fluxes (=measured days) and gap-filled days into account.

Generally, we averaged fluxes and calculated errors for both plots of each station and then summed them up to annual fluxes for the site. A detailed description of how the uncertainty estimates were derived was added in L 464.

- 5124-2 We added an extra section on uncertainty of biomass equations for clarification (L 708)

- Discussion "Discussion should include an explicit section for uncertainty and error assessment. Can you say that "the estimates are consistent with one another given the measurement uncertainty"? How much do your assumptions play a role in the magnitude of errors? Would different assumptions result in different conclusions? Without explicit treatment of these questions the findings remain on uncertain ground."

An explicit section for uncertainty was included (L 707). As a result, we can say that the results are consistent given the high measurement uncertainty of approach 2. Nevertheless, we consider approach 2 as not appropriate to get precise estimates of the NEE due to major related uncertainties. These uncertainties are based on limitations of existing methods to derive flux components, e.g. the CO2 flux from the litter layer or the heterotrophic respiration from SOM.

For approach 2, two fundamental assumption play a key role: i) the assumption of steady state between litter layer inputs and outputs and 2) the assumption that the CO2 flux from old root litter remaining in the soil matrix is negligible compared to the large CO2 flux from fresh root litter deriving from the trenching. We revised the discussion

in order to specify, which uncertainty these assumptions put on the total calculated NEEcalc. Furthermore, we can demonstrate, that assuming non steady-state does not lead to a more precise or less uncertain flux estimate (please see response to P Ojanen).

- 5126-8 could this be an indication of sample mismatch between the source area of eddy covariance measurements and the biometric measurements? It was not clear where and how many vegetation survey plots were measured.

We complemented the manuscript by a more detailed footprint analysis (L305, Figure 1) which demonstrates that the spatial overlap between biometric and EC measurement was very high (L750). Moreover we added details on the location of biometric measurements (L329). In Figure 1c we indicate the footprint for summer and winter day and night- times. It can be seen that especially during summer, the footprint was within a distance of 100m of the tower with the main wind direction from Sw (Figure 1). Since the forest extends homogeneously about 200m in all directions from the tower, the source areas agree very well to each other. However, it cannot be excluded that a time lag exists between biometric and flux data, i.e. that photosynthates from the previous autumn are stored in the trees and used for growth during the next spring growth. Consequently, a time mismatch between measurements might exist. As discussed in detail in Barford et al. 2001 and Gough et al. 2008 (thank you very much for mentioning these references) this uncertainty can only be excluded when long-term data are considered, thus for short term periods like in this study, this uncertainty remains.

- 5126 14 please add citations. this is not entirely your own work, is it?

We added the missing references in this section and further discussed the uncertainty related to random merror, gap filling and measurement uncertainty. (L735-745)

- Please be more critical, and try to identify the more uncertain terms in your current study. These may not necessarily be in EC, but overall, I think a rigorous error and uncertainty assessment is a required component for a study like this.

The structure of the manuscript was modified and a new section 4.5 in inserted, which comprises the discussion on uncertainty of the different flux components (from L710). Besides a more detailed discussion on uncertainties related to the EC method, as mentioned above, we further discussed uncertainties of biometric measurements, chambers measurements and the correction terms of our approach 2. Regarding N2O fluxes, a gap-filling routine was tested so that the uncertainty induced by gap filling could be quantified (L460 and section 4.5.4).

- 5127 1 This is the 'minimum gapfilling uncertainty' below which models usually cannot get. However, poorly fitting models can perform a lot worse than this. Assuming that your gapfilling model performs as the best models out there is not justified. This needs to be tested.

We have now 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 $\pm$ 13 g C m-2 for all gap-filled data This corresponds to an uncertainty of $\pm$ 6.5% of the total annual flux". These values are further discussed in the discussion (L731).

- 5127 3 where does this number come from? It should be explained in Methods along with other uncertainty and error analysis.

After analysing the error induced by the gap filling method and considering the new the results of the footprint analyses we were able to better constrain all major errors (discussed from L755-759). We can conclude that our estimate of errors being within $\pm$15% holds.

- 5127 10 you did evaluate the footprint of fluxes, why not trust those estimates? We revised the wording of this paragraph and conclude that the spatial agreement between biometric and EC data is very well since more than 90% of the fluxes derive from an area within 200m around the tower (L751).

- 5127 22 was Eco2 measured in untrenched plots at all? this correction backward can be a big source of error in closing the budget

We agree that the use of a trenching experiment to determine the heterotrophic soil respiration and the soil C budget constitutes a potential big source of error. We further analysed the potential bias to elucidate the impact on the overall budget. (section 4.5.3). However, other methods to determine the heterotrophic soil respiration are scarce or laborious and cost-intensive (isotopic methods). Two methods were e.g. tested by e.g. Ojanen et al. 2012 from which one ("Rh method") is based on soil heterotrophic respiration measurements and model estimates of plant litter production, the other one ("Rfloor method") is based on forest floor respiration measurements. They show that both methods have strong limitations: the Rfloor method clearly underestimated respiration. The Rh method was highly sensitive to the choice of root turnover rates and, thus, had a high risk of bias.

Generally, if the compartmental fluxes (autotrophic, heterotrophic respiration) should be considered as well one needs to apply the methods available. One conclusion of our study is, that the approach 2 is related to too many uncertainties and is therefore not appropriate to get precise estimates of the budget. These uncertainties are to a major degree induced by the analysis of compartmental fluxes related to soil respiration. Nevertheless, our study gives incentives for future research and improvement of methods and provides estimates (even if uncertain to some degree) of compartmental fluxes.

- 5127/28 I would argue that the assumptions into estimating annual accumulation are rather uncertain and a potential source of unquantified error

We agree that the quantification of the flux from the litter layer is subject to some uncertainty, which derives from the limitations of methods available. Besides an assumed measurement error, we assessed this uncertainty in the revised manuscript by performing a sensitivity analysis. Instead of assuming linear accumulation since afforestation, we tested which effect an assumed linear accumulation since canopy closure had on the overall flux (L784).

- 5129-21 provide citations

The missing citations have been added (L 642)

- 5130/5131 the site is what it is, regardless of the methods used for measurement Rephrased (L 690)

- 5132 sweeping conclusions like that are unjustified. It is not even clear whether the site is a sink or source during this one year, let alone anything longer.

The NEE estimate by the eddy covariance methods identifies the site to be a sink of -2 t C ha-1 for 2008. Considering the overall GHG balance, the N2O effluxes compensate this C uptake by almost 50%. Although the N2O fluxes are associated with high uncertainty deriving from the gap-filling, the low measurement frequency and the heterogeneous soil conditions leading to a large spatial variation, we demonstrate in the manuscript, that the potential errors deriving from the application of the method itself will lead rather to an underestimation than an overestimation of the flux. It is well established that the C assimilation of a forest depends, besides climatic and physiologic factors, on forest age and that NPP generally decreases when a certain age is reached (Gower et al. 1996, see also Ryan et al. 2004, Ecological Monographs, "An experimental test of the causes of forest growth decline with stand age"). Naturally, younger forest before canopy closure have a lower C assimilation potential because of their lower leaf (assimilation limitation) and root biomass (nutrient limitation). Therefore, our conclusion that the C sink potential will lower with age is not speculative but reasonable.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C4672/2013/bgd-10-C4672-2013-supplement.pdf

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

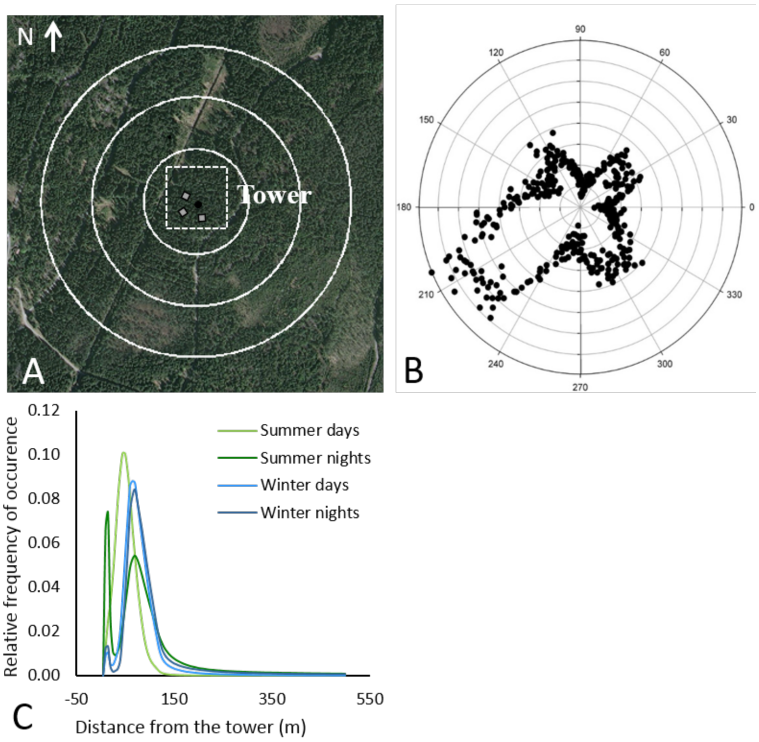
N
Tower
A
90
120
60
150
30
180
0
210
330
240
300
270
B
0.12
0.10
0.08
0.06
0.04
0.02
0.00
Relative frequency of occurence
Summer days
Summer nights
Winter days
Winter nights
-50
150
350
550
C
Distance from the tower (m)

Fig. 1.