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Biogeosciences Discussions

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

A. Meyer et al.

astrid.meyer@yahoo.de

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Author reply to the comment by Annette Freibauer on the manuscript

bg-2013-76: "A fertile peatland forest does not constitute a major greenhouse gas sink" by Meyer et al.

We thank A Freibauer for her very helpful and constructive comments. We revised the manuscript thoroughly and further clarified the methods and discussion especially with regard to the mentioned methodological issues. In order to clarify the impact of the use of a model to extrapolate soil respiration, we performed a regression model and inserted the new results in the manuscript. In relation to the comment by P Ojanen, an alternative calculation for the heterotrophic net efflux RSOM was tested but

not included in the manuscript because the authors are convinced that this approach would neither limit uncertainty nor produce more reliable results (as discussed in the response). Following the recommendation by reviewer 2 we restructured the discussion and inserted a new section 4.5 to discuss uncertainties of fluxes in detail.

Please find below our response to the general and specific comments and questions. For better visuality we attached the manuscript with marked changes.

General comment 1

In order to investigate the impact of environmental factors on soil respiration, we applied a simple linear regression model between soil temperature and soil respiration and included a section in the text to explain this in detail (L444). The impact of the water table depth was also tested but including ground water did not or only slightly improved the goodness of fit between modelled and measured flux, so for simplicity we used the soil temperature only. The soil temperature explained on average 75% ($\pm$9.1) of the soil respiration ECO2. Summing up the modelled values to the annual basis showed that the modelled result was very close to the previous calculation so that the annual flux estimate became 13.0 instead of the previous 13.1 t C ha-1a-1. The new result is used for the overall budget calculation and is discussed in in the discussions section.

With regard to the uncertainty of the N2O flux estimate we also tested a regression model to soil temperature and water table depth based on daily averages. However, the relationship was not very strong, and variation between plots high (pearson r between 0.1 and 0.4, only one plot 0.79). Thus, we did not apply a general model to extrapolate N2O fluxes. We added a paragraph in the new discussion section 4.5. to discuss these results (L820). Since the overall flux balance is in the focus of this study and not the detailed elucidation of the flux response to environmental factors, we limit the discussion on this.

With regard to the possible bias introduced by the only fortnightly measurement frequency, we modified and complemented the discussion and also present annual flux estimates of the previous and following year (2007 and 2009) to show how the 2008 results range in comparison to other years (L664). Furthermore, to elucidate overall uncertainty of the N2O flux, we assessed the impact of gap-filling by determining the gap-filling uncertainty for each gap-filled day. This was done by taking the standard deviation of the flux data used to fill the gap into account, which is described in detail in L 464. We are aware that the fortnightly measurement frequency probably produces a bias on the result and that some peak emissions might be missed. We cannot directly assess the bias introduced by this. However, this would lead to an underestimation of the measured net efflux which would imply that the net GHG sink potential of the site is even lower. Other uncertainties exist with regard to the application of the static chamber method, as now discussed in more detail in 4.5.4, which likely leads rather to a flux underestimation, but the net effect of these is difficult to assess.

General comment 2

We amended the text with regard to more details on the spatial variation of soil temperature and water table level (L494) in order to further discuss the possible dependencies of CO2 on these factors (L530-531). We decided against including a new figure demonstrating the annual dynamics of soil temperature in order not to extend the quite long manuscript further. Since the annual flux balance and not the spatial and temporal flux analysis is in the focus of this study, we only shortly discussed possible dependencies because a more detailed analysis would, in our opinion, go beyond the scope of the manuscript.

General comment 3

The long-term overall GHG balance of forests depends on the one hand on the GHG flux during the growth phase, on the other hand on the fate of the wood logged and other GHG emitted by management practices. If the wood is e.g. used as building material, it will conserve C in the long term, possibly for several 100 years, if it is used

as a fuel, it will return all stored CO2 immediately. Additionally, the GHG balance depends on the logging practice, i.e. total clear-cut or selective.

We therefore do not agree that it is the soil GHG balance that predominantly counts. Since the overall long-term balance including management and logging is dependent on so many factors, a more detailed discussion on the effect of harvest of biomass and its implications can only be very speculative.

In this manuscript we focus on the growing phase of the forest so that we restrict ourselves to the measured data and to implications for the total growth period. As we can show, other studies (Black and Gallagher 2010, Gower et al. 1996) indicate that a boreal spruce forest reaches its period of maximum biomass production around an age of 60 to 70 years. This implies that the annual tree growth will decrease with forest age and that the GHG sink strength of the study site will probably lower.

General comment 4

In order to elucidate the discussion on the determination of EF's we clarified statements in the introduction section and also introduced a paragraph in the discussion (L680)

Specific comments

P 5109 4 the references have been deleted P 5109 5-17 and P 5110 15-18 With regard to the comment on the GHG reporting we think that this is only applicable to those countries that voluntarily use the 2006 IPCC Guidelines. However, Sweden has not yet adopted to apply these, so that the NIR report is based on the 1996 and 2003 Good Practice Guidelines. These declare N2O reporting “may be applied optionally where data are available”, (GPG (2003) Chapter 3 Forestry, pp 3.46). In the Swedish NIR 2012 (pp 275) it is stated that: “According to UNFCCC (decision 13/CP.9), reporting emissions of nitrous oxide from drainage (N2O-direct Ndrainage) is optional. One reason for that is the limited understanding of the processes controlling the emissions. No N2O emissions from drainage of soils will be reported this year, but some preliminary studies indicate that reliable methods may be available in a few years."

In the next COP meeting it will (hopefully) be decided to adopt the IPCC 2006 Guidelines together with the wetland supplement for reporting GHG's, so that Sweden will have to include N2O emissions from drained forest organic soils in the NIR reports.

In order to clarify the Swedish GHG reporting we revised the relevant parts of the introduction to make them more precise.

P 5113 23 We regret that TDR data are mentioned in the methods but are not used otherwise. We corrected the methodology and introduced a paragraph about the measurement of water table depth data, which we finally used for the manuscript (L189).

P 5114 4 Forster et al. 2007 has been added

P 5115 9-15 We added few sentences describing, that in this study a trenching experiment was performed which required to separate between the different components of Rh (L227).

P 5115 26 We added canopy height to the site description (L158).

P 5118 We amended the description with details on the measurement set-up, number of replicates and measurements frequency (L181, 390). In order to keep the method section compact, the table A1 in the appendix summarizes al further details on the instrumentation of the different methods.

P 5119 Also P Ojanen commented on the method used for receiving the respiration from SOM. In our response (please see also reponse to P Ojanen) we argue that neglecting the decay from trenched roots and modelling the root litter production by turnover rates introduces a large bias on the results due to the large uncertainty of root turnover rates in literature. Also, root turnover rates as determined from field studies derive from more or less intact systems. In our experiment however, all roots has been trenched, which mean that the system is seriously disturbed. Consequently, roots will not decompose according to a modelled root litter production. Thus, it is not possible to

consider the modelled flux from trenched roots and the modelled root litter production together. We show an alternative approach (please see the response to P Ojanen) in which we assume a root litter production according to modelled values but neglect the decay from trenched roots. This approach leads to an even higher uncertainty of the total NEE due to the major uncertainty associated with the root turnover rates. Furthermore, the measured efflux ECO2 derives from the disturbed ecosystem and will produce a bias due to the additional flux from trenched roots. We are therefore convinced that the approach presented in the manuscript delivers a more reliable result.

P 5120 14 We added a section in the discussion (L784) in which we demonstrate the uncertainties by assuming a linear litter accumulation since afforestation. We present sensitivity analysis showing, which effect an assumed accumulation since canopy closure has on the total GHG flux.

P 5120 23 Further information on the description of stations was added at the beginning of the method section (L177-181).

P 5121 5 We a sorry for the lacking accuracy in the description. In order to get more accurate annual estimates of the CO2 flux, we now applied a linear regression between CO2 flux and soil temperature (30 cm depth) in the revised manuscript. The description was added in section 2.8. The result of the annual estimate was very similar to the previous estimate which was only based on the interpolation of measurements. Compared to the previous result of 13.1 t C ha-1a-1 the total net CO2 efflux ECO2 by the regression model amounts to 13.0 t C ha-1a-1. Figure 3, Table 4 and the text were updated with the new results.

P 5122 22 Uncertainties were inserted

P 5127, 5130 typing and unit errors have been corrected

P 5132-20 We changed the wording so that the sentence is now consistent with the findings

Table 4 all units have been changed to Ceq.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C4655/2013/bgd-10-C4655-2013-supplement.pdf

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