Final author comments, BG-2011-175

Referee #1

Abstract

-The first line of abstract sounds vague. I was rather surprised to see the usage of the phrase "likely to change " when several papers published by these authors have clearly shown that the GHG biogeochemcistry does change after the drainage of natural peatlands.

*Response: The first line of abstract was rephrased and now sounds: "Drainage for forestry purposes increases the depth of the oxic peat layer and leads to increased growth of shrubs and trees."

"... is likely to change..." was changed to "change"

-Line 17: change 'loose' to 'lose'.

*Response: corrected

-Line 18: Please qualify the ecosystem by the major forest/vegetation type . *Response: The site type, dwarf-shrub pine bog, has been indicated in the next sentence, on line 19.

-Lines 21 -22: Relatively little change in WT level – compared to what? Compared to the natural peatland that existed there? What was the basis for comparison?

*Response: Typical water level in dwarf shrub pine bogs is -20 cm. Thus the WTL drawdown at Kalevansuo has only been about 20 cm, which is relatively little, compared to the natural variation and comparing to other forestry drained areas where the drawdown is typically 30-60 cm. We added text to Chapter 4.1 to give more insight into this issue. At the same time, some paragraphs related to this were split and/or relocated to make the text more fluent and logical. A sentence concerning the tree stand transpiration was removed as speculation.

-Lines 22-24. The range in NEE - what does the range refer to? Please clarify.

*Response: This refers to the set of annual CO₂ balances calculated with a 365-day moving windows over the whole period of 16 months (2 September 2004 – 31 December 2005). In other words, the first period is 2 Sept 2004 – 1 Sept 2005, the second is 3 Sept 2004 – 2 Sept 2005... and the last is from 1 Jan – 31 Dec 2005. This variation originates from the differences in the fall-time CO₂ balances, as the data set included two autumns. For clarity, we removed the rest of the sentence "*varying from*..." from the abstract, and deepened the description of the range issue in the results section.

-Line 30-32: While stating the novelty of this work, the authors fail to recognize the fact that there are other studies in Finland that have reported the drained peatlands to be sinks for C. The novelty of this work is that a forestry drained peatland has been shown to be a C sink with EC for the first time.

*Response: Yes, studies have been done in Finland suggesting this same idea that nutrientpoor peatlands may act as C sinks, and some of these studies are referred to in our manuscript. But these papers are based on indirect methods where the long-term sink/source functioning has been deduced from peat cores, not from direct present-time gas fluxes. And this is actually what we tried to say, that we do not know any publications on the net CO₂ exchange of the whole ecosystem on forestry-drained peatlands. For clarification, we removed two commas on lines 31-32: "…and the first time C accumulation has been shown by EC measurements to occur in a drained peatland" -Please qualify the ecosystem studied appropriately.

*Response: This has already been done in abstract, lines 18-20 (see also comments above concerning line 18)

-Also the last sentence of this section is incomplete and therefore, not clearly readable. *Response: Could it be possible that the referee has read the older version of the manuscript, and not the one which was uploaded after the Quick Reports? In this later version the sentence reads: "Our results suggest that forestry-drainage may significantly increase the CO₂ uptake rate of nutrient-poor peatland ecosystems." In the first version, the word "suggest" was missing.

-Introduction

This study refers to a paper (Pihlatie et al 2010) that has reported data collected from the same site after the time period which the present paper refers to. There is nothing wrong with that. But a question does arise in readers' minds. If more data exist, why then the specific limited time period has been selected for inclusion in this paper? As the authors duly realize, C exchange measurements vary from one season to the other. Inclusion of data from other years would be scientifically more rewarding.

*Response: The referee is right: we have continued the measurements some years after 2005, and these data were not included in this manuscript. The main criterion for the selected time period was that we have also collected N_2O and CH_4 flux data by chambers during this very same time period, from autumn 2004 to the end of 2005. By using these data we were able to show that, in addition to the forest being a very strong CO_2 sink, the CH_4 and N_2O fluxes do not, however, change markedly. Hence, drainage increases the CO_2 uptake but does not induce high N_2O emission, which is a typical consequence when draining mires for agricultural purposes (Maljanen et al., 2010). Therefore, the climatic impact of the forestry-drainage at such ecosystems as ours seems to be cooling, or at least not warming.

Another reason for not including a multi-year data in this manuscript was the space limitation: detailed study on inter-annual variation would have either expanded the manuscript or cut off the other issues currently raised in the discussion.

Materials and Methods

-Line 86: The sentence about the drainage does not agree with your own statement in the abstract. *Response: I do not see a great disparity between the Abstract and the Material and methods; they both agree that water level was on average 40 cm below the surface. However, we have added more discussion on the water level issue on P. 14-15 and reorganised the text.

-The authors do not say anything about the energy balance closure at the site although they have all data needed to assess this aspect of EC measurements.

*Response: Energy balance closure at the site was 91% (intercept $11Wm^{-2}$), r²=0.86; this was added to Ch. 2.3, together with a new reference.

-Line 185 : Modelled values – how were the values modeled? This appears for the first time in the paper. The reader is left wondering about modeling until the reader reaches the gas filling section discussed in the results section. I would therefore, suggest the authors to move the gap filling section to the M&M section.

*Response: The sentence about replacing the removed values with modelled ones was removed from here, and the gap-filling section was moved to M&M. Reference to Fig. 8 was removed to avoid a wrong order when referring to Figures.

-How was the storage flux calculated? Reference to a paper that describes the method? *Response: Calculation of the storage flux has been explained in more detail.

-Line 177: Why 70% was chosen as the threshold level for representativeness as estimated by the footprint model? Why a more stringent limit has not been set? *Response: The limit is always a compromise between the representativeness of measurement and the amount of data. For example, by using a criterion of 80%, we would have lost more than 50% of the data existing before applying a footprint criterion. With a criterion of 70%, we only lost 20% of the data.

How big a problem is that part of the flux originates outside the representative forest area? First of all, outside winter, the problem is mainly related to night-time, i.e. respiration data, since in day-time the footprint is typically smaller than in nights having more frequently stable atmospheric conditions. In our day-time data, on average 90% of the observed flux originates from the representative forest area.

In a situation where 70% of the cumulative flux originates from the representative area, 30% of the observed flux is biased, i.e., the observed flux $F_0=0.7*F_R+0.3*F_{UR}$, where F_R and F_{UR} are the fluxes from representative and unrepresentative surfaces. The bias can be significant only if the surfaces are very different. Changing the criteria from 70 to 80% does not have a significant impact on our data: the average night-time flux in May-September decreases by 2-6%. Only in sector 157-180° does the flux increase by 20%. As only about 10% of all the night-time data in May-September originated from this sector, the impact of the footprint limit on the annual CO₂ balance is of minor significance.

Results

-Line 224: Meteorology, in my opinion, is not a proper title for this section. *Response: Changed to "*Meteorological conditions*"

-Line 308-310: Please specify the 365-day periods.

*Response: Done

-Lines 311-312: Please note that the NEE is positive during the growing season only after the peak NEE.

*Response: I do not understand what the referee means with this comment. In my opinion, the text (in the revised version in Ch. 3.3, Lines 334-335) and the data of autumn 2004 shown in Figure 10 are consistent.

-Lines 395-397: Without any detailed hydrological characterizations, these statements about water movement within the peatland appear to be speculative and should be indicated as such. *Response: The arguments of slow water movement and blocked ditches were based on visual observations. However, the text was modified by adding "*likely*" to the last sentence to better reflect the partly speculative nature of these observations: "*As the site was rather flat, water discharge was very slow apart from the snowmelt events. Most of the ditches were blocked by vegetation and therefore functioned poorly. It was therefore likely that the drainage at the site was maintained mainly by the transpiration of the tree stand*..."

-Line 404: The sign convention for C flow due to leaching reversed here? Any flow out of this ecosystem is assumed to positive.

*Response: Yes, the sign convention was indeed illogical for the CO₂ fluxes. Part of the sentence was removed; now the sentence does not involve any sign convention anymore.

-Lines 456 – 474: As the relationship between NEE and VPD could be confounded by TER, could the authors look at the relationship between GPP and VPD?

*Response: Yes, we looked at this relationship, but could not find a significant dependence. One possible explanation for this could be that the GPP, which is obtained by subtracting the modelled TER from the measured NEE, is confounded due to, e.g., slightly overestimated temperature response of TER. VPD and TER have a rather similar temperature response, both increasing NEE (i.e., decreasing CO_2 uptake) with higher temperatures. Hence, the impact of VPD on photosynthesis may be transformed into the respiration component in the flux partitioning, and no relationship between GPP and VPD can be observed. Therefore we attempted to find the relationship between the directly measured NEE and VPD using a very narrow (one degree) temperature range. If the temperature remains constant, it's masking effect on respiration can be removed and the resulting relationship should reflect the real VPD-GPP relationship.

Referee #2

1. General comments

-This is a well-written and well-structured manuscript. The subject and motivation of the study have been summarised appropriately; the authors seem however intent on demonstrating the novelty of their study which doesn't seem wholly necessary as this work complements previous NEE assessments taken at other peatlands in the northern hemisphere.

*Response: We see it worth of mentioning that the C balance of peatlands managed by artificial drainage to improve forest growth is still a big question mark, and no direct gas flux measurements, including all the components involved in CO₂ production/uptake, have been made. It is still under a debate whether this land use is a favourable option in climate change mitigation, or whether it is a GHG hotspot, and, above all, should one avoid a simple characterization when speaking about forestry-drained peatlands, admitting that the carbon balance may be highly variable and dependent on vegetation type, fertility, etc.

2. Specific comments

-Page 4, line 100: is it reasonable to assume that the pH remained constant over the 2-3 years between studies?

*Response: We have no reason to assume that it would have changed much. This information is part of general site description.

-Page 4, line 105: how were the dimensions of the transects chosen? Did you use footprint estimates? *Response: The dimensions are a compromise between an adequate footprint and the number of sample plots (and related work). However, given that the footprint maximum of all the accepted daytime fluxes was located on average at distance of 11 and 26 m from the EC mast in summer and winter, respectively, it can be concluded that the biomass transects represent relatively well the main source area of the flux measurement.

Page 5, lines 134-141: provide more details of the corrections applied and discuss how the storage term – was calculated.

*Response: More details of the corrections have been inserted in the text. Calculation of the storage flux has been explained in more detail.

-Page 7, line 186: how was the u* threshold obtained?

*Response: The threshold was determined statistically. The selection criterion has been explained in detail in Page 7. For clarity, Figure 3 was edited by changing the error bars from SD to SE.

-Page 8, second paragraph: is there any sense in using all available data, regardless of the goodness of fit? You seem to prioritise quantity over quality. Since fluxes were small, you could instead select good fit data and discuss how these compare with the dataset as a whole.

*Response: Poor fits are mostly caused by small fluxes, when the concentration changes during the chamber closure are close to the noise level of the measurement method. Deleting these small fluxes from the data would overestimate the annual flux (assuming that the soil is producing the gas). This is often the problem especially in winter with low temperatures and fluxes, but for methane also in summer in cases where production at the certain point equals oxidation. By omitting these data would most likely seriously bias the results. For this reason, we decided not to change the selection criterion for the chamber flux data.

-Page 11, line 310: specify what the "different 365-day periods" were.

*Response: Done

-Page 12, line 346: is the peat depth variable throughout your site?

*Response: Peat depth is similar in all methane plots.

-Page 13, lines 369-70: insert range (e.g. SD) of CO2 uptake and C accumulation in tree biomass for ease of comparison.

*Response: Uncertainty estimate has now been given for both values. A new Appendix chapter (B) was introduced to describe the methods of calculating the errors. The error in the C accumulation in tree biomass was calculated by P. Ojanen, who was also added into the author list as a new author. In this procedure, the tree biomasses were recalculated using different biomass models, however resulting in fairly similar C accumulation (160 vs. 175 g C m⁻² yr⁻¹) (see Page 4). Text attributed to this has been revised and new references have been added. "*Much*" was replaced with "*very likely*" on Page 14, Line 384 ("*The annual net CO*₂ *uptake at our site was much very likely higher than the amount of C accumulated in the tree biomass...*").

-Page 14, paragraph 2: does this mean that your site isn't fully drained?

*Response: We are not sure what is meant by "fully drained"? The drainage is deep enough to enable tree survival and relatively good tree growth. Deeper drainage would most probably not increase tree growth much, since low nutrient level is restricting growth. Improved drainage would not be economically sensible. In that sense it is drained well enough for forestry purposes.

-Page 16, paragraph 2: I'm not sure I understand what you mean by the "development stage" of the two forest considering that you later state that the one at Hyytiälä is "younger thus growing faster". As I see it, you aren't comparing like with like although the ballpark figures are worth mentioning. *Response: The referee is correct. We have changed the text now: what comes to similarities, we now refer to the total stand volume and mean height, and discuss about the soil condition as a main difference between the sites, which also affects the tree growth.

-Page 17, paragraph 2: provide the modified equation.

*Response: With the modified equation we here mean the Eq.4. To prevent misunderstanding the first sentence of the paragraph was changed to: "*Removing the VPD response function* (f_{VPD}) from the NEE-model (Eq. 4) would reduce the fit between the modelled and measured values only slightly (r2=0.86 against r2=0.88). However, using f_{VPD} corrected..."

3. Technical corrections

-Page 5, lines 129-30: what was the tube diameter? *Response: OD1/4'', ID 1/8''. Information on ID added to the text.

-Page 9, paragraph 1: could you summarise all this data in a graph?
*Response: A new figure (Fig. 5) has been inserted, presenting the monthly temperature and precipitation during the measurement period and during the 30-year period.

-Page 9, line 239: do you mean deepest or shallowest by "lowest average WTL"? *Response: Deepest. Corrected in the text.

-Page 12, line 336: is the "radiation-response model" eq. 3?

*Response: Yes it is (Eq 4 in the new version). To help the reader we modified the text: "If the gaps were filled using Eq. 4 that includes the radiation response, the wintertime CO_2 balance...".

Outside the referee comments, we have added some sentences and two new, recently published references (Straková et al. 2010, 2011) to the text on Page 15, Lines 416-418, discussing the possible reasons for the net C uptake by this ecosystem.

References:

-Maljanen, M., Sigurdsson B. D., Guðmundsson J., Óskarsson H., Huttunen J.T., and Martikainen P. J.: Greenhouse gas balances of managed peatlands in the Nordic countries – present knowledge and gaps, Biogeosciences, 7, 2711–2738, 2010.

-Straková, P., Anttila, J., Spetz, P., Kitunen, V., Tapanila, T., and Laiho, R.: Litter quality and its response to water level drawdown in boreal peatlands at plant species and community level, Plant Soil, 335, 501–520, 2010.

-Straková, P., Penttilä, T., Laine, J., and Laiho, R.: Disentangling direct and indirect effects of water table drawdown on above- and belowground plant litter decomposition: consequences for accumulation of organic matter in boreal peatlands, Global Change Biol., doi:10.1111/j.1365-2486.2011.0253.x, 2011.