We are very grateful to the four anonymous Referees and Referee Lars Kutzbach (Referee #3) for their effort and time to prepare these detailed and valuable feedbacks to our manuscript. We agree with most of their comments and will alter the manuscript accordingly. Other comments we don't completely agree with and explain below, how we address them in the revised version.

Major comments, including all referee comments:

1) **Measurements should be continued to reflect climatic variability, and this should be mentioned in the outlook (Referee #1).**
   Response: We fully agree with this comment. However we feel such a statement should be directed to funding agencies but not be addressed in a scientific publication.

2) **Throughout the manuscript it is sometimes difficult to keep in mind which of the 2 sites is the natural one and which was drained and afforested. A consistent naming (natural – formerly drained) could help reading (Referee #1).**
   Response: Thank you, done.

3) **The age of the spruce trees has to be mentioned at least in the site description. Productivity is clearly related to the age of a forest (Referee #1).**
   Response: We added the tree age to the site description and the abstract. Furthermore, a citation is added in the discussion of the revised manuscript, that states that 44 years old Norway spruce trees have almost reached their maximum productivity.

4) **Methane fluxes methane fluxes should be considered as well (Referees #1).**
   Response: We agree methane exchange is an important part of the GHG-balance. Unfortunately, methane fluxes are not available at both sites, and thus we cannot compare these two sites in terms of their global warming potential. This is now emphasized at the end of the introduction. We added some discussion on methane balances in peatlands drained for forestry and in natural peatlands in Section 3.5, based on results from literature.

5) **Using different devices for CO_2/H_2O measurements at the two sites (LI200 vs. LI7500), (all Referees).**
   Response: Thanks for pointing out this potential difficulty. We did look into this, and came to the conclusion that the bias between the two sensor models is small, but failed to explain this in the original manuscript.

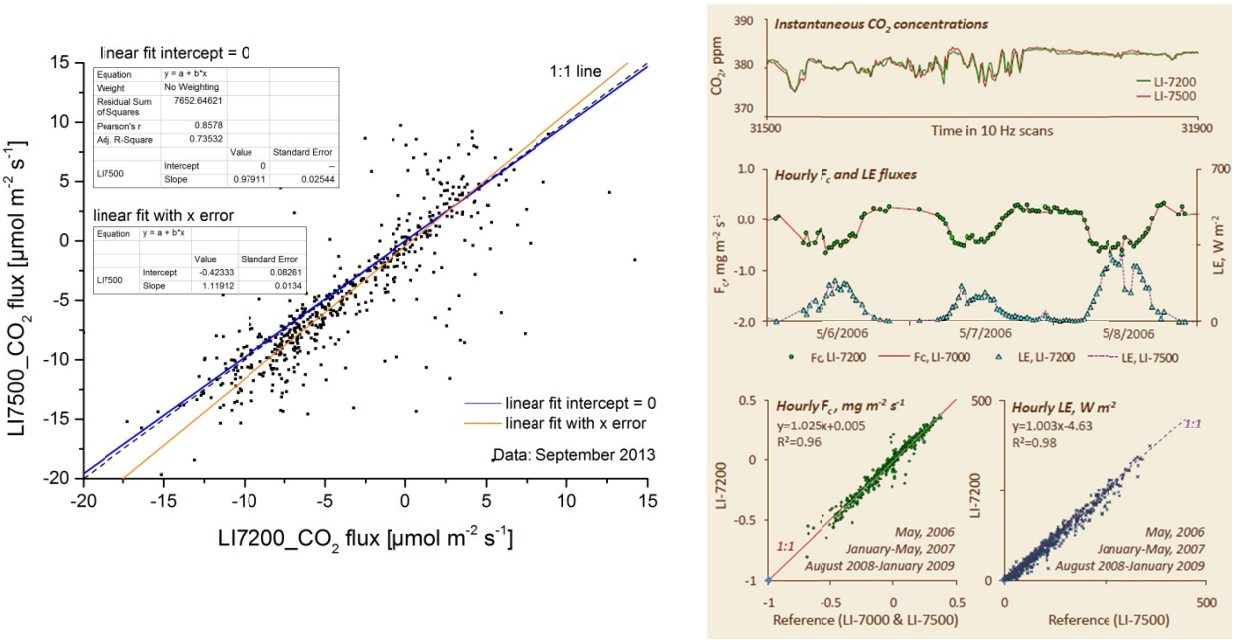

Figure 1: CO_2 flux of LI7200 vs. LI 7500. Left: Data of one month from a heterogeneous bog pine site. Measurement systems were separated by 35m. Right: Figure is taken from Burba et al. (2009), presenting a very good agreement between the sensor types.

Burba et al.(2010) found a very good agreement between the CO_2 and latent heat fluxes measured with LI 7500 and LI7200 (Figure 1, right). They detected almost equal concentrations and a slope of almost 1 of the linear regression between CO_2 and latent heat measured with LI7500 and LI7200, respectively. Thus the bias is likely negligible for the site comparison. This is now added to in the Section "Uncertainty of annual sums" (Appendix):

> "However, two different measurement devices were used for H_2O and CO_2 flux measurements (open-path (LI7500, Li-Cor) and closed-path (LI7200, Li-Cor)) at the two sites, which may lead to a bias in the site comparison. Based on a study of Burba et al. (2010) and Burba et al. (2009), CO_2 and latent heat fluxes measured by LI7500 and LI7200 show a very good agreement. The slope of the linear regression between latent heat- and CO_2 fluxes measured with this two devices for several months is almost one, and the coefficient of determination is better than 0.96. Thus the expected bias is negligible and not addressed in the uncertainty estimation for the site comparison."

Additionally, the following sentences were added to the Section "Instrumentation" to make our reasoning more clear:

> "The measurement principle of LI7200 is based on the absolute Non-Dispersive-Infrared (NDIR) design of the LI7500 (Burba et al. 2010), which leads to a good agreement between the two measurement devices. The closed path system has a considerable advantage, because rain drops and fog do not compromise the measurements of the close path system, which leads to higher data coverage (see Section 2.3)."

In addition, we operated a LI7500 and LI7200 in September 2013 at Schechenfilz. Although, the distance between the sensors was 35 m, leading to a difference in their footprint the CO_2 fluxes match very well (see Figure 1, not shown in manuscript).

6) The impact of the carbon storage should be quantified (Referee #1, #2, #5?)

Response:

Discussed CO_2 exchange in our manuscript considers only annual budgets or cumulative fluxes. For timescales larger than a few days, the storage term cancels and is therefore negligible. Thus, the storage term is not considered in the uncertainty assessment of annual budgets in the Appendix. Furthermore, the determination of storage change flux contributions is controversial, especially in non-homogenous canopies, and we do not recommend to apply flux corrections based on such estimates. Instead, we decided to reject the CO_2 fluxes which are likely to be decoupled from the ground vegetation layer. The standard practice is the so called u_* threshold (e.g. Aubinet et al.,

2012; Goulden et al., 1996; Schmid et al., 2003). The application of this threshold worked very well for our sites, as fluxes above the u_*- threshold were independent of u_* (Fig. 2). This criterion is the a widely used method in eddy covariance research (Aubinet et al., 2012).
Therefore, the storage term could not affect our gap-filling procedure that is based on half-hourly fluxes, because data that may be biased by storage change flux contributions are rejected and gap-filled.

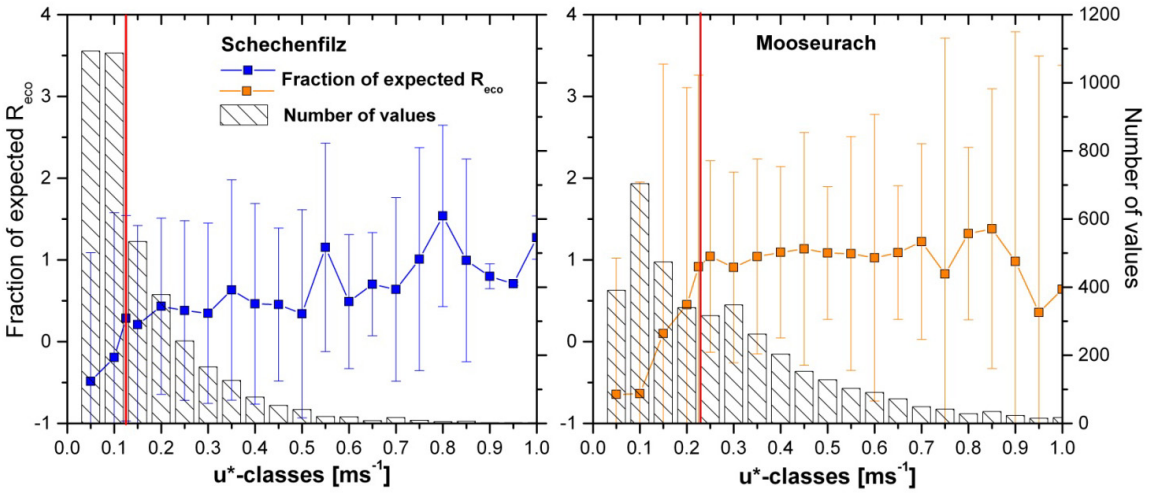

Figure 2: Binned values of the fraction of expected nighttime ecosystem respiration versus friction velocity (u_*). The expected ecosystem respiration is modeled by temperature dependence. Red line indicates u_*-threshold, whereby fluxes are independent of friction velocity. Bars are the number of values within the u_*-bins (data exemplary shown for 2011). Vertical bars denote the standard deviation of each bin.

7) **Burba correction (Burba et al., 2008):** Referees #2, #3 and #5 note that the so called "Burba correction" should be applied on the data measured by the open-path analyzer, to avoid biased flux measurements due to instrument heating
Response: The sensor head of the open path analyzer LI7500 is heated which in principal can generate convection within the sampling volume and therefore may have an influence on the application of the WPL adjustment. However, the correction after Burba et al. (2008) is not transferable to all measurement set ups, as the correction depends on inclination of the sensor head and on the wind speed. According to the book chapter "Corrections and Data Quality Control" in Eddy Covariance- A Practical Guide to Measurment and Data Analysis (Foken et al., 2012), the Corrections due to sensor head heating is not generally recommended: "No general consensus has been reached yet as to which [correction] method is most effective and efficient. All corrections depend on wind speed and the inclination of the sensor. Therefore, any correction should be applied with care." For this reasons, such corrections are not applied to our measurements.

8) **WPL-conversion**: Referee #3 criticize that the WPL-conversion was not applied for flux data of LI7200:
Response: It is not explicitly mentioned in the manuscript, but the LI7200 measures the mixing ratio of CO_2 to dry air (mol mol^{-1}).
Thus the WPL adjustment is inappropriate, when the mixing ratio output of LI7200 is used.

9) **The presented literature research on previous work on the topic of the effect of draining peatlands for forestry needs to be improved (Referees #3).**

Response: Thank you for proposing these references, as only Hargreaves et al. (2003) is considered in our Discussion Paper. We extended the discussion in Section 3.3 "Annual CO_2 exchange" and in Section 3.4 "Long-term carbon balance". Furthermore, most of the recommended studies are now mentioned in the introduction of the revised manuscript.

10) **The authors refer to the old map of peat depth from 1940 concluding that due to anthropogenic activity the peat layer decreased by 1 meter within the last 70 years. And based on this assumption the estimated total carbon loss from this bog within 44 years between 1967 and 2011. Although this might be a truth, I would not trust so much the old maps, as we do not know how they were prepared and what is accuracy of a peat layer depth assessment. As this is uncertain I would use e.g. expressions as" potential peat loss" or "possible peat loss" on Page 2194 lines 25-28). Are the same maps available for natural site? (Referee #4)**

Response: We altered the revised manuscript and gave more details on the data: "Based on a 75 m gridded stratigraphic survey of peat thickness (including about 70 measurements in the target area (Fig 1) from the end of the 1930s (data courtesies by Distinct Government of Upper Bavaria), the former peat thickness at the location of the current soil measurements was 4.4 m. This is about one meter thicker than today, illustrating the peat loss in the last 70-80 years."

A similar map is not available for the natural site, but there is no evidence of peat loss there for the equivalent time period.

11) **More details to the footprint analyses (Referee #4)**

a) **Height of the EC installation above canopy level (we know only the height of the towers**

Response: In the revised manuscript, the aerodynamic measurement height is mentioned in Section 2.2 "Instrumentation"

b) **Specify what is your area of interest at natural site – looking on figure 1a we have bog-pine forest nearby the tower surrounded by single pine trees area– is this area also considered?**

Response: We are sorry that the term "Single pine trees" led to confusion, and in fact this term is not quite correct upon re-extermination: the entire target area is classified as bog forest, but the core area has a higher tree density. The term "Single pine trees" is now replaced by the term "Low density bog-pine forest". The former "Bog-pine forest" in the legend of Fig 1a is replaced by "High density bog-pine forest". Both areas present the target area, which is now mentioned in the Figure caption and also addressed in Section 2.3 "Data handling".

c) **Please deliver information about the footprint size and prevailing wind direction (wind rose) for both sites in the methods**

Response: The mean footprint size is presented in Figure 1 a,b, which gives an idea of the footprint size. The prevailing wind direction is mentioned in the Section "Site description" and can be derived from the mean footprint in Figure 1.

12) **Estimate of long term carbon balance (Referee # 3, #5)**

Response: The referees did not agree on an assessment of this section, which indicates to us that we did not clearly state how our discussion of the long-term context stands in comparison to the direct measurement results. We think the carbon balance estimate of such ecosystems over two years of measurements alone provide only a biased view of the behavior of these ecosystems on the longer time scale. The two sites differ not only by their current management, but by their management history. Although the effects of this difference in management history is outside of the range of direct measurements, we consider it important discuss and appraise the likely cumulative effects of peat drainage at the one site. This is highlighted now at the beginning of the Section 3.5, as well as in the abstract and conclusion. Furthermore we improved the discussion of the long-term carbon loss estimate, based on results from other drained peatland forests.

Specific/Minor Comments Referee #1

P 2190, l 15 and elsewhere: higher productivity of spruce trees may also be attributed to age of the forest, which is about the most productive state in a forest life cycle (forest age should be mentioned in abstract already)

Response: We adopted the proposal and mentioned the forest age in the abstract and in the site description.

P 2191, l 18: carbon emissions instead of CO2 emissions
Response: Done

P 2192, l 12-14: formulation to be changed: methane should be considered in future/ by additional measurements
Response: We changed the formulation but did not follow the suggestion. We want to underline that methane and the greenhouse gas balance is not the topic of the manuscript. The paragraph is changed as follows: "Methane is likely also important for full greenhouse gas estimates in peatland ecosystems. Unfortunately, methane fluxes are not available for both sites, and thus we cannot compare these two sites in terms of their global warming potential."

P 2192, l 16: 'measurements made over two years with the eddy covariance technique, from…'
Response: Done

P 2193, l 15: link to be activated or removed
Response: Link is not a Hyperlink, Link denote the botanist Johann Heinrich Friedrich Link, who firstly described the subspecies *Pinus mugo ssp. rotundata* (Link).

P 2196, l 6-7: it should be noted over which period zero mean wind speed is ensured
Response: We added following sentence to the revised manuscript. : "Furthermore, we applied the planar fit method after Wilczak et al. (2001). One regression plain was determined biannually to ensure zero mean vertical wind speed for this time period."

P 2196, l 10: gaps do not occur due to the instrument diagnostics. The diagnostics deliver a measure for data quality on which the user decides whether to use the data or not. Causes for gaps might e.g. be rainy or foggy conditions.
Response: Thanks for the hint. We agree and deleted the term

P 2196, l 25: does that mean the area of interest is matched at any time? To clarify I suggest to add an estimate of the footprint area graphically in fig. 1
Response: Please refer to our response to major comment 11

P 2197, l 27: '…a positive NEE dominant CO2 release by the ecosystem.'
Response: Done

P 2198, l 5-6: ' …exponential relation between nighttime CO2-fluxes and temperature…'
Response: Done

P 2198, l 14: In Lloyd and Taylor (1994) E0 is kept constant and is not called K, but the unit is K. Please ensure which are the fitting parameters and clarify.
Response: We agree, Lloyd and Taylor (1994) kept E_0 constant, but E_0 is widely used as fitting parameter (e.g. Drösler, 2005; Lohila et al., 2007; Reichstein et al., 2002; Ruppert et al., 2006). However, we rephrased the sentence: "The relation used based on an empirical equation of Lloyd and Taylor (1994):".

P 2198, l 15: ' and T (_C) the measured half-hourly temperatures providing the best fit.'
Response: Done

P 2198, l 21ff: For GPP, respiration determined with night-time relation and day-time temperatures was subtracted from measured NEE (I would suspect). Then the GPP

relationship (alpha, GPPmax determined by regression) was determined with eq.2 and afterwards modelled? Please clarify. It has to be considered (at least mentioned) that day- and night-time respiration are different.
Response: To clarify we changed Equation 2 in the revised manuscript:

$$NEE = \alpha' \times PPFD \times GPP_{\max} / \left(GPP_{\max} + \alpha' \times PPFD\right) + R_{eco}\,.$$

Additionally, we added following sentence: "Note that here $R_{eco,\ day}$ indicates ecosystem respiration determined by the nighttime relation, but day time temperature only."

P 2199, l 9ff: with a linear regression usually also an offset is determined. Is the offset set to zero or close to zero? Otherwise it has to be taken into account and is not negligible.
Response: To clarify, further information was added to the revised manuscript: "The coefficients of determination (R^2) of the linear regressions of half hourly meteorological values at the sites indicate a correlation better than 0.97, the slopes of the linear fits are close to 1 and the offset is almost zero."

P 2201, l 1ff: what about possible influences of the water table?
Response: We added the following sentence." As VWC and water table depth indicate similar fluctuations (Fig.3), a similar dependence between temperature, R_{eco} and water table is assumed. "

P 2202, l 23ff: '…, if only carbon dioxide is considered.'
Response: We rephrased the sentence in the revised manuscript: "Over the whole two year measurement period, the total net CO_2 uptake by the drained ecosystem at Mooseurach was -429 ±73 g C m^{-2} and ‑126 ±45 g C m^{-2} by the natural ecosystem at Schechenfilz (Fig. 8)."

P 2203, l 4ff: as was explained before, soil moisture did not have any influence. It can be expected that despite low water tables, trees are never water limited. This may be reflected by still high soil moisture content.
Response: Thanks for the hint. To clarify, we added the following sentences to the revised manuscript: "Overall, despite noticeable water table drawdown at the drained site, the spruces never seem to be water limited. This is additionally indicated by the absence of a link between CO_2 exchange and soil moisture (Fig 5)."

P 2204, l 21: '…different land uses requires a longer-term perspective and the determination of methane fluxes.'
Response: We reparsed the sentence in the revised manuscript: "For meaningful comparisons between peatland forests and full evaluation of the climate impact of different land uses requires a longer-term perspective, and in addition the determination of methane fluxes. However, we don't have any reliable information on methane for this time period. Thus, we attempt to assess roughly the biome carbon balance in its long-term context based on CO_2 only."

P 2207, l 7: '…carbon loss of +550 gCm2 a1 for previous years.
Response: Thank you, we followed the suggestion

Table 1: 'Long-term yearly averages of meteorological parameters…'
Response: Done

Fig. 1: What exactly are the target areas? Green triangles are difficult to find. What is the main wind direction? A graphical footprint area could help to clarify the text as well.
Response: Please refer to our response to major comment 11

Fig. 6: do the error bars include the uncertainty determined from bootstrapping as well as random error?

Response: The bars in Fig. 6 present the standard deviation of the mean annual NEE, calculated for 13 different averaging periods. The bars should present the variability caused by shifting the averaging period, but not the uncertainty induced by gap-filling or turbulence sampling. We rename the error bars to vertical bars.

Language: Usage of commata to be checked
Response: Done, hopefully

Specific/Minor Comments Referee #2

P 2193, l 5: "Maximum precipitations occurs during summer" – which share? 50%, 80%?
Response: Done

P 2193, l 7: avoid single sentence paragraphs.
Response: Done

P 2193, l 12: this is unclear, the peat layer is still pristine but was affected by peat cutting – this is contradictive, please reword.
Response: Thanks for pointing this out, we clarified the sentence as follows: "However, in the present observation area, located in the center of the bog complex, the peat layer is still pristine"

P 2193, l 15: woody area? This sounds like few sparse trees, is this true?
Response: We replaced woody by wooded

P 2193, l 17: average leaf area index – what kind of average, how many measurements? Is this LAI or PAI?
Response: We agree and amended LAI to PAI in the revised manuscript. The given PAI-value is the average of 100 individual measurements, which is mentioned in Section 2.2 "Instrumentation" in the discussion paper.

P 2194, l 2: how variable is the C/N ratio. Provide numbers.
Response: Done

P 2194, l 21: fraction of "available" nitrogen due to the drainage.
Response: Done

P 2194, L12ff: this is very nice and detailed explanation of how much data had to be rejected or how much data has been identified of good quality.
Response: Thanks

P 2199, l 14 and l 25: Why not using control & treatment site instead of these rather difficult names
Response: Thank you, done.

P 2200, l 2: please specify, 50% of what? per day or per month in this case november? what is the average?
Response: This is specified in the revised manuscript

P 2200, l 4: how many cm on average or sth what give the reader an idea
Response: Done

P 2200, l 16: why infinite?

Response: GPPmax (maximum carbon uptake) is defined as the maximum carbon fixation rate at unlimited PAR. To clarify, we replaced carbon uptake by GPP

P 2200, l 26: Why was Reco normalized for LAI? Please explain
Response: We gave further information in the revised manuscript: "Following the notion that more biomass usually produces more respiration, we normalized R_{eco} with PAI. This results in very similar normalized emission rates (+283 and +299 g C m^{-2} a^{-1} at the drained forest and +364 and +403 g C m^{-2} a^{-1} at the natural pine-bog, respectively) between the sites for both analyzed periods."

P 2202, l 20: similar to a finnish site, which is located in the boreal region. This would mean the fluxes at your site are rather small or the fluxes at the finnish site are rather larger. Please comment.
Response: We rephrased the paragraph

P 2203, l 2: both component fluxes or just one?
Response: Both. This is clarified now in the manuscript.

P 2203, l. 16: replace maximal with highest
Response: Done

P 2203, l. 22ff: This is very interesting. If the authors or someone else will take tree cores in the future this should be highlighted, since such second growth period can lead to a second tree ring.
Response: Thanks for highlighting this.

P 2204, l. 12ff: This paragraph is unclear. What are you trying to state? Please reformulate.
Response: Thanks for pointing this out. We rephrased the paragraph to obtain more clarity: "At this site the overall CO_2 exchange is more in balance, due to the low growing activity of the bog pines on the one hand, and the suppressed soil emissions caused by high soil water level on the other hand. In contrast, at least the respirative parts of CO_2 exchange at the drained spruce site are sensitive to changing environmental factors, like periods of increased temperature and water table drawdown. However, whether warm and dry anomaly periods increase or reduce net carbon uptake at the drained site depends on soil temperature and spruce phenology and thus on the season."

Figures are well prepared
Thanks

Figure 5: Please explain the fraction of Reco
Response: Now fraction of R_{eco} is explained in the caption: "Relationship between VWC and R_{eco} (non-gap-filled), air temperature and the fraction of expected R_{eco} (measured R_{eco}/modeled R_{eco} by temperature relation)."

Figure 7: why annual and showing months?
Response: Months on the x-axis in Fig 7a mark the start of averaging period, but all bars present annual means. Thus, the unit of the y-axis has to be NEE in g C m^{-2} a^{-1}.

Specific/Minor Comments Referee #3 (L. Kutzbach)

Page 2191, lines 2-3: The reference does not really fit to the statement. Post et al.(1982) do neither talk much about carbon exchange nor about peatlands' area.
Response: Thank you for the hint, we agree and rephrased the sentences and added a correct reference in the revised manuscript.

Page 2192, lines 10-12: Please consider the paper of Hargreaves et al. (2003).

Response: Done

Page 2193, line 17: Is this average LAI of the whole canopy or of individual trees?
Response: We replaced LAI by PAI.

Page 2194, line 21: Better "soil material is mineral"
Response: Thank you, we followed your suggestion

Page 2194, lines 25-27: Please give more information about this map? What type of map? Topographic? Geologic? What scale? Please give a full reference for the map.
Response: Please refer to our response to major comment 10

Page 2195, line 25: Better "thermistor temperature probes (type 107, Campbell Scientific)"
Response: Thank you, we followed the suggestion.

Page 2196, lines 2-6: See general comments, paragraph (B)
Response: Please refer to our response to major comment 8

Page 2196, lines 6-7: Please give more details on how many wind direction sectors, how long average time?
Response: Done, we added following sentence: "Furthermore, we applied the planar fit method after Wilczak et al. (2001). One regression plain was determined biannually to ensure zero mean vertical wind speed for this time period."

Page 2196, lines 22-25: Specify how large the target area around the tower was.
Response: Done

Page 2197, lines 14-16: Please write more precise: that you calculated moving window averages and 99% confidence intervals for these averages.
Response: Done. We wrote: "We calculated the average of each moving window, and checked that every 30-min flux value in the window is within the 99% confidence interval of the mean, otherwise the value was excluded from further calculations"

Page 2198, lines 17-18: This is surprising. Could you please give some more details on how this was checked?
Response: Actually, we would have been surprised if we had found a dependence between the nighttime respiration-temperature relation and tree phenology in an evergreen coniferous forest. For example, also Hargreaves et al. (2003) did not find any evidence that the night-time CO_2 emission-temperature relation varied seasonally in Scottish afforested peatlands. This could be checked by plotting the fraction of expected R_{eco} against PPFD or date.

Page 2199, lines 8-11: R2 is not the correlation coefficient. It is the coefficient of determination. It is not a sufficient measure of similarity of the meteorological conditions at the two sites. You should also look at the slope and offset of a linear regression line that relate the two compared time series. (E.g., if precipitation at the one site would be always exactly double the precipitation at the other site, the R and R2 would be still 1.)
Response: We amended our mistake. Additionally, we mentioned the slope and the offset in the revise manuscript.

Page 2200, lines 17-18: Do you use "carbon uptake" in this sentence synonymously to GPP or to NEE?
Response: To clarify, we replaced carbon uptake by GPP

Page 2204, lines 25ff. See General comments, paragraph (C)

Response: Please refer to our response to major comment 12

Page 2206, line 26: Do you mean here with "carbon uptake" GPP or NEE (or both)?
Response: We mean NEE, but it's true for both

Page 2207, lines 6-7. Please write more precise: "…resulted in an average annual carbon loss of +550 gCm-2 a-1 over the last 70 years.
Response: Done

Page 2207, lines 18ff: See general comments, paragraph (C)
Response: Please refer to our response to major comment 12

Page 2217, Table 1: Why only 2011 is shown and compared to the reference period? The meteorological conditions of 2010 and 2012 would be also interesting
Response: Thanks for the note; we added the meteorological conditions of 2010 and 2012 to Table 1.

Technical comments

Page 2190, line 4: I suggest " …same soil formation.."
Response: Done

Page 2190, line 18: Remove commas before "if" and before "since".
Response: Done

Page 2191, line 17: I suggest hyphenating: "climate change-induced"
Response: Done

Page 2191, line 23: Remove comma before "and"
Response: Done

Page 2191, line 24, remove comma after "it"
Response: Done

Page 2191, line 25: Remove comma before "or"
Response: Done

Page 2192, line 12: remove comma after "although"
Response: Done

Page 2193, lines 26-27: I suggest hyphenating: "water-saturated" (also below)
Response: Done

Page 2194, line 1: Rewrite sentence. Elemental analyses cannot show pH-values.
Response: Done

Page 2194, line 2: Remove "very"
Response: Done

Page 2194, lines 4-5: remove commas before "as" and after "composition"
Response: Done

Page 2201, line17: Remove commas before "as" and after "as". Use plural form: were" instead of "was"
Response: Done

Page 2202, line 3: Better: "mea annual CO2 uptake"

Response: Done

Page 2202, line 13: Remove "very"
Response: Done

Page 2202, lines 23-25: I would move these lines to the end of the previous section (3.3.).
Response: In the revised manuscript, the discussion on the previous section 3.3 is changed /extended. Thus the sentence does not match very well at the end of section 3.3. However, the sentence is now combined with the following paragraph to avoid single sentence paragraphs.

Page 2203, line14: Place comma before "we"
Response: Done

Page 2203, line 16: Place comma before "and".
Response: Done

Page 2204, line13: Remove commas before "caused" and after "level"
Response: Done

Page 2204, line 23: I suggest hyphenating: "peat loss-induced"
Response: Done

Page 2205, line 29; Place comma before "and"
Response: Done

Specific/Minor Comments Referee #4

Page 2193 lines 10-15 from the description of the natural site we know that the northern part of the natural site was affected by cutting and restored in 2001? How big was the degraded part of the peatland, how the site was restored and how far is this area from the footprint area of EC tower. This is just a question, If this restored peatland area may affect the measured fluxes? I think it might be useful for the interpretation of the data to add the main footprint area to the figures of both sites.
Response: The northern part has been restored by refilling the drainage ditches. Generally, the flux footprint does not reach this area (Fig 1a).

Pages 2193, 2194, 2195, LAI – how LAI was derived? If it was derived just from measurements conducted with optical method (based on Sunscan DELTA-T system), as it is written in page 2195, then the values given in the paper are related to an effective Plant Area Index (PAI), which includes foliage but also branches and stems. To derive LAI you need to subtract the area of branches and stems from PAI, or which is more reasonable – to change LAI to PAI in the paper. Another issue is, if the PAI values presented in the paper are related to trees only, or includes also sedges etc. The optical method used by the author assumes that PAR is measured just above the soil surface (in lowest part of the canopy) and above canopy level (where the reference PAR measurements should be done). My question is, how it was measured in Mooseurach site, where trees have 21 meter height? Can it be clarified in the method section? The paper of Chen (1996) may be useful here as a reference. Chen, J.M. (1996) Optically- based methods for measuring seasonal variation in leaf area index in boreal conifer forests. Agricultural and Forest Meteorology 80, 135-163.
Response: Thanks for pointing this out. We replaced LAI by PAI

In the site description of the discussion paper, we mentioned the value of LAI (PAI) of the bog-pines and the spruce forest. Thus the PAI just considers the trees, not the understory. The PAI/LAI is measured to a reference sensor that is exposed to direct sunlight (without shadowing). We found this in a clearance. Otherwise, the reference sensor could be mounted to the top of the 30 m tower.

Page 2195, lines 2-6, from this we know how high the towers are at both sites, but there is not clear at which height the EC systems were installed, at the top of the tower, how many meters above canopy level? please clarify it
Response: Done

Page 2195, line 11-12 was a steel tube heated or not? I know that this has no importance in case of CO2 fluxes, but lack of heating might impact H2O fluxes measurements
Response: As mentioned in the manuscript, the tube was not heated but insulated. I the year 2010 this steel tube was provided and recommended by Licor Biogeoscience.

Page 2195, lines 14-20, a) what was the height of T/RH, and PPFD sensors installation? 2 meters above the surface, or above canopy, or near the surface?
Response: Done

b) where the rain gauge were installed? Above or below canopy? Only one rain gauge was used per site- if it was installed above canopy – then it would be fine, but if it was somewhere inside forest canopy then the results might be uncertain. If rain gauge was heated, please mention about it in the text.
Response: Done: "…precipitation was detected by a heated tipping bucket rain gauge 52202 (Campbell Scientific) at both sites (1 m above canopy)."

c) Can you give any information about distribution of wells?
Response: Done

d) in case of LI190SL please kip in mind that this quantum type sensor measures Photosynthetic Photon Flux Density (PPFD), which has units of quanta (photons) per unit time per unit surface area and not just PAR in watts/m2
This implies that PPFD and not PAR should be used in the text and in figures.
Response: Thank you for pointing this out, we amended the mistake and changed PAR to PPFD in the whole manuscript.

Page 2195 line 21,
a) I think it should be "indicating" instead of "integrating"
Response: We replaced integrating by averaging, which is the most correct term.

b) once analyzing the CS616 data You should consider that the sensors were originally calibrated for mineral soils, hence the measurements conducted in peat might be highly uncertain, especially in natural site, where the average WTD level was higher than 10 cm below the peat surface (3/4 of TDR was permanently in water) From the manual of the Campbell CS616 we know: " These coefficients should provide accurate volumetric water content in mineral soils with bulk electrical conductivity less than 0.5 dS m-1, bulk density less than 1.55 g cm-3, and clay content less than 30%."
Response: We adopted the calibration coefficients reported by Yoshikawa et al. (2004) for organic soils that consist of dead sphagnum material. We mentioned the reference in Section 2.2 "Instrumentation" and updated Figure 3 and 5.

Page 2195 line 25 ,
1) use 0.1 m instead of 10 cm;
Response: Done

2) use "thermistores probes" instead of "T-107-probes" and put T-107 to brackets.
Response: Done

This is really a pity that peat temperature was measured only at one depth at Schechenfilz site, where there was observed rather high WTD level of a few cm below the surface. That means, measurements of T refer mostly for the water saturated peat layer for most of the analysed period. In fact, this may result in a low daily and seasonal variation of T, which might be used to Reco estimation. If yes, then I suppose that Reco fluxes might be underestimated for this site, what finally may bias the estimation of GEP.
Response: Unfortunately, temperature sensors in higher soil layers were damaged by rodents. Of all remaining, the temperature in 10 cm depth provided the best fit.
Some peatland studies used temperatures even of a deeper soil layer, e.g. Sottocornola and Kiely (2005; 2010) and Dunn et al. (2007) used the soil temperature in 20 cm depth for their gap –filling in a blanked Irish bog and in an afforested Black Spruce forest, respectively. Yamulki et al (2013) used the temperature in 15 cm depth to model the soil respiration of an afforested raised peatbog in Scotland.

Page 2196 line 16, data coverage of 91% for Mooseurach and 71% for natural site…is it correct, considering longer periods with gaps and more periods when Mooseurach station was not working correctly I would say that it should be in the opposite.
Response: Higher data coverage at Mooseurach, despite longer periods with gaps, is due to closed-path CO_2/H_2O measurements at this site. Thus less measurements are affected by foggy and rainy weather conditions. The proximity to the lake Starnberg in the north of the natural site (equipped with the open path sensor) leads to frequently occurring fog events, particularly in autumn.

Page 2197 lines 18 –number of missing values might be slightly misleading, as only 9% of data are missing at drained site and the other 29% were rejected at this site because they were outside the footprint area
Response: We agree, and rephrased the sentence

Page 2198 line 10, -1) use 0.1m instead of 10 cm.
Response: Done

2) As mentioned before- I would be careful with using 10cm depth peat temperature to model Reco at natural site, as T sensor was in a water saturated peat layer by most of the year. How justify to use this T, considering that most CO2 is produced in the near-surface unsaturated peat layer (when soil respiration is considered) and from autotrophic respiration of plants?
Response: Please see above.

Page 2200, lines 18 and 26, please explain how GPP and Reco were normalized with LAI? Please consider my previous comment related to PAI. I am not sure, but I would assume that in the drained site the ratio of foliage area to stem and branches area might be different (smaller) than in the natural site, hence the GPP and Reco normalization procedure may lead to biased results.
Response: The normalization does not affect the measurement results, as it is used simply in aid of interpreting the measured difference between the sites.

Page 2193, line 15, - delete [LINK]
Response: Link denotes the botanist Johann Heinrich Friedrich Link, who firstly described the subspecies *Pinus mugo ssp. Rotundata* (Link).

Page 2198 line 15, Please verify the To value. In Lloyd and Taylor (1994) equation To equals 227.13oC

Response: Thanks for pointing this out. As our gap-filling routine uses the correct value for T_0 we corrected the typing error.

Page 2198 line 20, I would suggest to use "by the same fitting parameters", instead of "correlation coefficients ", which is not correct
Response: We agree and adopted the suggestion

Page 2199, line 9; R2 is a determination coefficient, and not correlation coefficient
Response: We corrected the mistake, thanks for pointing this out.

Page 2199, lines 23-25, as the analyzed 12-month periods are not related to the calendar year I would suggest to use word "period" instead of "year"
Response: Done

Page 2200, line 15, 1) use PPFD instead of PAR, the relationship is between GPP and PAR, not between PAR and GPP, please change the order
Response: Done

Page 2200, lines 21,27 please add " for both analyzed periods respectively"
Response: Done

Page 2201, lines 20,23; I would suggest to use "period" instead of "year"
Response: Done

Page 2202, line 3, add "net" before "uptake"
Response: Done

Page 2202, line 16, 19, 20 is it "net" uptake?
Response: Yes it is. To clarify, we added" net" in the revised manuscript

Page 2205, lines 15-20, convert tC ha-1 to gC m-2 to be consistent with other values given in the text
Response: In this paragraph we consider long-term budgets, but not annual budgets. Annual budgets are consistently expressed in g C m^{-2} a^{-1} in the entire manuscript. Long-term budgets are much larger than annual budgets, such large amounts are easier to handle in t C ha^{-1}.

Page 2205, line 15 – shall it be gross uptake?
Response: No, this is net uptake. We added "net" in the text

Page 2205, line 19, add "net" before"uptake
Response: Done

Page 2205, line 27, PAI instead of LAI
Response: Done

Fig.1. 1) I would increase slightly the font size of legend,
Response: Done

2) why there is a word "Key" before names of the sites? Is it colloquial name reflecting the shape of the site? If yes, I would recommend to not use it.
Response: Key is deleted in Fig 1.

Fig.3 . 1) Please add the height of air T measurements,
Response: Done
2) depth of soil T measurements,
Response: Done

3) use volumetric water content (%) instead of water content – here you should add that this is the average for 30 cm upper peat layer,
Response: Done
4)average WTD
Response: Done

Fig. 4. 1) Are the figures 4a and 4b related to the same periods of 2011? I assume not, at the same PAR Response in winter and summer GPP should be different.
As it may lead to confusion, please explain it in the figure caption.
Response: Figure 4 a and b refer to the same period (the whole year 2011).

2) Specify the height of Tair measurements.
Response: Done

3) Use PPFD instead of PAR
Response: Done

4) why not to use 30 minutes averages, instead of averages of 100 half-hourly measured values?
Response: Differences of CO_2 exchange and the response of CO_2 to PPFD and Tair at the two sites is more clear when binned values are used.

Fig.5 . please explain what you mean by "fraction of expected Reco" – is this REco normalized with temperature?
Response: This is now explained in the caption: "Relationship between VWC and R_{eco} (non-gap-filled), air temperature and the fraction of expected R_{eco} (measured R_{eco}/ modeled R_{eco} by temperature relation).

Fig. 6 . I would suggest to use "period" instead of "year"
Response: Done

Table 1. I would suggest to calculate sum of precipitation and average T and RH for 12 month periods analyzed in the paper between July 2010 to June 2011, July 2011-June 2012. This would be more useful for the interpretation of the data. Use RH, instead of rH
Response: Done

Specific/Minor Comments Referee #5

More detailed comments: -p.2193 line 17: Which LAI? Total, projected or half of the total? What about the LAI of the sedges and heather?
Response: We amended the manuscript and changed LAI by PAI

-p. 2194 line 2: Could you please give the CN range here, and add them to Fig. 2 as well?
Response: Done

-p. 2194 more information on the peat cutting and agricultural history is needed. How much peat was removed during the peat cutting activity? How long was the site cultiated; from the beginning of the century until 1967? Was the site fertilized or prepared otherwise? All this history makes me think whether the site should be defined as "afforested (agricultural) peatland" or "afforested cutover peatland", instead of "a

peatland drained for forestry (which typically means only drainage, sometimes also fertilization).
Response: We are sorry about the confusion we caused concerning the land use history of the drained site. Only the large bog-complex in the north of the research area (Weidfilz) was affected by peat cutting. As the target area in Mooseurach was never affected we removed references to peat cutting. This is clarified in the revised manuscript: "The Mooseurach site (70 ha) is part of the large bog complex (250 ha, Fig 1b) that was drained at the beginning of the 20th century. Initially, the research area was used for agriculture. However, due to unfavorable agricultural site conditions, such as nutrient deficiency and still relatively high water table, agricultural use was discontinued after only a few years. The area was used as grassland and pastureland for about two to three decades. In the 1960s forestry became more important."

-P. 2194, line 10: You mention that the site has suffered from a still high water table, but from line 16 onwards you highlight the impacts of good drainage. There is a slight conflict, please clarify.
Response: We don't see any conflict. After drainage, the water table was still too high for profitable agricultural use. Nevertheless, the soil is drained and we mentioned the effects of the "moderate" drainage in the manuscript. However, we altered the manuscript and added a "relative": … "….because of unfavorable agricultural site conditions, such as nutrient deficiency and still relatively high water table."

-p. 2194, lines 13-14: Again, define which LAI you are talking about.
Response: Done

-p. 2194, line 20: how was the peat thickness measured? In how many points, and how were these distributed in space? Please give the SD. This variable is very important, as you are deducing the peat loss rate by observing the peat depth.
Response: The peat depth was analyzed by stratigraphical soil analysis. The determined peat thickness is based on one soil profile.

-p. 2194, line 23: Also here, please give the range of observed CN ratio
Response: Done

-p. 2202 line 13 onwards: what about Lohila et al. 2007 (Boreal Environ. Res.), that's afforested site, isn't it? Also that of Meyer et al. 2013 (in BG) could be added here, except if you are referring to only bogs, which you should state on line 13. For the more general discussion on the C balance in forestry-drained peatlands I might recommend adding the paper of Simola et al. 2012 (European J. Soil Sci.)
Response: Thanks for the recommendations; the revised manuscript considers the publications of Meyer et al. 2013 and Simola et al. 2012.

-p. 2202 lines 20-21: what do you mean by similar site conditions? The Finnish site had not experienced agriculture or peat cutting. Please clarify
Response: We rephrased the whole paragraph and deleted the unclear comment.

p. 2208 lines 10-14: what about the Burba correction, or the impact of storage fluxes, which were likely manifold in the drained site?
Response: At the drained site we used the closed path device LI7200. For this sensor type the "Burba correction" is inappropriate. The open path IRGA was used at the natural site, please refer to our response to major comment No. 7. In terms of carbon storage, please refer to our response to major comment 6.

Chapter 3.4: The tree uptake has been given by referring to an unavailable document. If the tree growth is to be used in the main conclusions of the paper, it is essential that the biometry model and the assumptions used in it is described in the mat&met. This is a highly important number if one wants to say something about the current state of

the peat soil CO2 sink/source. Why not to calculate the present tree C uptake using the same model? How was the model calibrated for the Mooserauch site? Or is the presented value some kind of general mean for forests of this age? Did you make any biometric measurements at your site?

Response: See also response to major comment 12.

We assume, the Referee refer to section 3.5 and not 3.4. In this section (Page 2205, line 13 onwards) we wrote that the carbon fixation by the spruces was determined by biometry and forest growth modeling. Actually, the modeling results were not used in our manuscript. Thus we rephrased the paragraph: "The current standing biomass of the 44 years old forest, above and below ground, was determined by biometry and common allometric relations as 86 t C ha^{-1} (S. Röhling, personal communication, 2012." Biometry results of Röhling et al. are not used to estimate C-uptake rate, but only to estimate standing biomass.

As the results of Röhling et al. are still unpublished, the citation is replaced by a personal communication.

About the peat subsidence: the authors have made many assumptions which make the uncertainty (which was not estimated) of this calculation huge. For example, how can you know that the bulk density of the already disappeared peat layer has been the same you have found from the site now? Also, the bd values used are all from farmed peat soils, how well do they represent the forest conditions?

Response: See also response to major comment 12.

We considered constant carbon content of 49.7% (Fig 2) and a dry bulk density of 0.15 g cm^{-3} of the first 20 cm top soil layer, in the last decades. These values were measured at our site as mentioned in the site description. The extrapolation to previous decades is a necessary assumption, leading to a very rough estimate, which is highlighted in the discussion paper and even stronger underlined in the revised manuscript. A reliable uncertainty assessment of the carbon loss rate is not possible.

Technical comments: -p. 2205 line 8 –> is/was based

Response: Done

-p. 2206 line 6 Saarino –> Saarnio (not in the reference list)

Response: Done

- p. 2209 line 10: remove "and"

Response: Done

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