Responses to Referee # 2

Below we give a full or excerpted version of the reviewer's comment in italicized text followed by our response.

General comments:
This manuscript presents a case study where the impact of drought – or lowered water level – on CO2 fluxes and plant biomass is examined. The arrangement is interesting in the sense that the control and drained plots are situated very close to each other, having the same climate.
…
Extrapolating the results to warmer and drier climate, as done in the paper in a qualitative way, would lead to dramatic loss of CO2-C from Canadian treed bogs. In fact, the loss of C reminds me of that in the drained peatlands taken into agricultural use. The fact that the fluxes shown in this paper only represent the growing season, makes the annual C loss even greater. This makes me to suspect the quality of the CO2 flux measurements, which I see is the most vulnerable and uncertain part of this paper.

Thank you very much for confirming the importance of our research on the impact of drainage on biomass of treed peatland. We also highly appreciate your constructive criticism that will help us to improve this paper greatly. Although these losses may seem high, even higher CO_2-C losses have been reported. Soil respiration emissions of 513 to 6516 g CO_2 m^{-2} yr^{-1} were reported for drained forestry peatlands in Sweden (Arnold et al., 2005). This compares well with our estimated growing season respiration emissions of 1080 to 1800 g CO_2 m^{-2} (290 to 490 g CO_2-C m^{-2}). Simola et al. (2012) also estimate a net loss of soil carbon from Finnish drained forestry peatlands reporting a mean value of a loss of 150 g C m^{-2} yr^{-1}. We estimated growing season net loss of C from the ground layer of the drained site of ~200 g C m^{-2}, but this also includes tree root respiration. Our tree root respiration estimate at the drained site was about 27% of total respiration or about 56 g C m^{-2} over the growing season, placing our estimated soil carbon losses in line with those reported by Simola et al. (2012). Waddington et al. (2002) also report high CO_2-C losses from Canadian peatlands drained and extracted for horticultural peat of 88 to 399 g CO_2-C m^{-2} per growing season." This discussion will be added at the end of line 7 page 15003 and references will also be added to the existing Reference list.

First of all, I do not find any description of how the respiration has been measured. The chapter 2.2 only describes the measurements done on light conditions. However, I assume that respiration has been measured either by doing the measurements in night time, or by darkening the acrylic chamber by a dark cover during daytime. I ask the authors to add a detailed description of the respiration measurements.

Line 2 of page 15006 describes how we measured respiration. Measurements were made by covering the chamber with an opaque shroud. Section 2.2 clearly details measurement of CO_2 fluxes. However, for convenience we add here more details regarding the query on flux measurements. We used clear chamber for CO_2 flux measurements. Observing this protocol, we took a total of 5 runs (4 runs for NE_{ff} and last run for respiration) each of 1.75 minutes: 2-3 full sun, single shade, double shade and finally opaque tarp (for respiration). The chamber was flushed (vented) for enough time between the measurements to bring the headspace concentration in equilibrium with ambient air concentration. Therefore we measured respiration as the final measurement (after about 18 minutes) at each collar/plot. In this way any buildup of CO_2 in the soil would have already been flushed.

Both of Lai et al. (2012) and Koskinen et al. (2013) who used automated chambers for measuring respiration only have problems with flushing of CO_2 at night whereas we used closed clear chambers with opaque tarp during the day for measuring net exchange of CO_2 and determining gross primary production and respiration. Therefore their night time respiration measuring method problems do not apply to our daytime measurement method for; gross primary production/net exchange using clear chamber and

respiration which using opaque tarps covered clear chamber. The overestimation during night time measurements had been observed by Hermle et al. (2010).

We have retained the exponential fit to estimate CO_2 flux from our chamber data given its physical basis as outlined in Kutzbach et al. (2007).

To conclude, it is hard for me to believe that such a great seasonal CO2 emission from their drained site can be possible. I might be wrong, but I would like to see this tested. Related to this issue, I also would like to see some comparison to earlier studies regarding the CO2 balances, now the quantitative literature review on CO2 fluxes is totally missing, which would put this result in a better perspective.

As stated above, we do not think that these CO_2 balances are unrealistic. In response to reviewer 1 we will more fully discuss some missing components (e.g. belowground GPP and litter) that will decrease the loss slightly, but we do not think we are overestimating respiration due to flushing given the method we have described and our response above. We will add a few additional references on CO_2 exchange as outlined in our response above and those measured in continental bogs to the discussion (e.g. Wieder et al. (2009)) to help put the results in context. However, there are not many studies completed in this region and most are already cited in the text (Adkinson et al., 2011; Syed et al., 2006).

Response to specific comments:
(p 15003 line 17-20): the sentence is strange, please reformulate
Thanks for correction. The sentence will be reformulated in the revised paper as,
"Drought response experiments have been conducted in Eastern Canada where generally most of the peatlands receive high precipitation leading to high surface humidity and are characterized by their open nature that often lack tree cover, for example; Waddington and Price (2000) and (Strack et al., 2006)."

(p 15005 line 8): "60 cm × 60 cm × 30 cm"
We will make this correction.

(p 15006 line 7-8): since they have an important role in the paper, please specify what exactly are GEPmax and NEEmax? How they are calculated? Are they related to one measurement occasion or do they represent longer period or average?
The GEP_{max} and NEE_{max} are specifically defined at p 15006 line 7-9. They represent GEP and NEE when the photon flux density of photosynthetically active radiation is greater than 1000 μmol m^{-2} s^{-1}. As modeled values of the maximum rate of photosynthesis are likely never achieved in reality, these values represent a more realistic estimate of CO_2 exchange when light is not limiting as discussed in Bubier et al. (2003). We use these to statistically compare between plots to better understand processes (changes in plant cover, species type, water table, etc.) that affect CO_2 exchange. Data reported are averages for the study seasons for all occasions when PAR > 1000 μmol m^{-2} s^{-1}. We will make sure that this is clear in the revised manuscript.

(p 15006 line 11-12): please reformulate the sentence. It is not clear, if the parameterization was done separately for different years or not, and if not, why? Do you think the parameters should stay the same during different years, so that all differences are only arising from meteorology?
Thanks for asking for clarification. The parametrization was performed separately for each of the two years. Therefore "separately" is added to end of the first sentence at line 13 at p 15006.

(p 15006 line 15): remove comma after "where"
This will be corrected.

(p 15007 line 12): Chapter 2.2.2: which data was used for validation?

Of all the data measured in the field, we chose one third randomly and did not use for model construction. These data were later correlated with modeled data for model validation. Therefore "randomly chosen data that was" is added to line 12 inside the bracket.

(p 15008 line 25): define H and W, 25cm x 25cm
This will be defined clearly in the revised manuscript.

(chapter 2.3, last paragraph): would be useful to see the results of tree C uptake somewhere
We give the results for both tree biomass and annual increment in section 3.2.

(p 15010 line 11): Fig 1 should be Fig. 2
Thanks for correction. The "(Fig 1)" will be changed to "(Fig. 2)".

(p 15010 line 14): can you really say that 2012 was warmer? The difference was so small (0.02 deg) that I don't think it's significant.
We agree and the biggest difference between the years was really the lower precipitation in 2012. We will correct this in the manuscript to read: "In 2012, the reduction in rainfall by 121 mm led to a decrease in water table level at control".

(p 15010 line 17): "trenched" should probably be "drained"
"Trenched" refers to the act of constructing drainage ditches in this case. To avoid confusion we will clarify this sentence to refer to ditches instead of trenching.

(p 15013 line 1): can you really say that the difference between 40 and 13 g C m-2 is signifi- cant? Would be good to have uncertainties for these numbers.
We agree that the difference between 40 ± 24 (standard error) and 13 ± 6 was not significant. We have updated line 1 at page 15013 with the standard error values and ensured that standard error of estimates is included in all tables.

(p15013 line 8): related to an earlier comment, may be you cannot say 2012 was warmer
We agree and will remove "warmer" from this sentence.

(p 15013 line 14): Aurela et al. (2004), perhaps you should refer to Aurela et al. (2007)?
Yes, this has been corrected to "(Aurela et al., 2007)"

(Table 1): What is "SEE"? Please give number of observations (n=?). Again, please indicate if both years are included in the parameters.
SEE is standard error of the estimates in g CO_2 m^{-2} d^{-1}. Values of parameter estimates will be separated for the years along with number of observations/means in the revised version of table/paper.

(Table 4): please give the uncertainties for site NEE's, to help to see if the differences are significant or not
Table 4 will be improved according to the suggestion with standard errors included for all estimates.

(Fig. 1): You could mention also here that the measured data in this fig was not used for the model parameterization, and how was this data selected (may be it's enough to give the latter in the text)
Thanks for asking for the suggestion. The Fig.1 caption is improved as follows;
"Figure 1. Goodness of fit (r^2) between (a) modeled and measured GPP_{ff} and (b) modeled and measured R_{ff}. The measured data used in these figures were chosen randomly and were not used for construction of models. Both 2011 and 2012 data from control and drained sites are presented. Lines represent the 1:1 fit."

(Fig. 2): add "2011" and "2012" in the figure below the x-axis
The missing 2011 and 2012 will be added.

(Fig. 4): Perhaps because I have problems understanding the definition of GEPmax and NEEmax, and what time periods they represent, I don't understand why you are not showing the seasonal fluxes here, and what is the reason for comparing instantaneous values (and have they been measured at the same time, e.g. GEPmax and Rr?). Again, explain H and W, the figures should work independently from the text.
As explained in an earlier response that the instantaneous measurements of NEE (NE_{ff}) and R_{tot} (R_{ff}) were measured at the same day time occasion. The GEP_{max} and NEE_{max} are specifically defined at p 15006 lines 7-9. They represent the day time measured fluxes when PAR was $\geq$ 1000 μmol m^{-2} s^{-1} during the growing seasons of two study years and means for the entire growing season are given in this figure. They represent more of processes than CO_2-C balance and allow for statistical comparison of actual measured data instead of modeled values that include addition error from model fitting. On the other hand the seasonal flux estimates were modeled on the basis of $2/3^{rd}$ of these measured ones (plus other data measured with shading). A definition of H and W will be added to the caption.

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