

## ***Interactive comment on “Carbon dioxide flux and net primary production of a boreal treed bog: responses to warming and water table manipulations” by T. M. Munir et al.***

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Response to Anonymous Referee # 1

Specific comments

Page 12938, Line 6: tree root respiration was not actually measured but instead derived from measurements of ecosystem respiration and trenched (i.e. root-free) plots. I suggest rewording this sentence accordingly.

Yes, we will reword this sentence accordingly. A word “determined” will be added before “tree root respiration. . . . .”

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Page 12938, Line 9: Here and at other places the authors use the term ‘C balance’ when they actually refer to the ‘CO<sub>2</sub> balance’. Since non-CO<sub>2</sub> C fluxes via methane emissions and DOC export can be substantial and need to be included in the C balance in peatland ecosystems, I encourage the authors to avoid using the term C balance unless all these non-CO<sub>2</sub> fluxes are actually included.

Thanks a lot for the suggestion. In the revised manuscript, we will avoid using the term C balance unless we discuss actual C balance that includes CH<sub>4</sub> and DOC. We will change C balance to CO<sub>2</sub>-C at all places where applicable.

Page 12938, Line 11: . . . warmest 2013, the. . .

Yes, we will correct “The” to “the”.

Page 12938, Line 12-13: ‘. . . the experimental site. . . and the drained site. . .’ At various places throughout the manuscript I find that a ‘the’ is missing when referring to the sites.

Thanks a lot for the suggestion. We will fill in “the” at all missing places (when referring to the sites) throughout the manuscript.

Page 12938, Line 14: What does ‘although’ refer to? Its use does not seem to have a logic connection in this sentence.

Thanks! We will remove “although” and improve the sentence as “The short-term drainage at the experimental site resulted in small changes in vegetation coverage and large net CO<sub>2</sub> emissions at the microforms”.

Page 12938, Line 18: Given that NPP is one of the key objectives listed in the paper title, I suggest to present quantitative NPP data in the abstract.

Yes, we will include a sentence that summarizes the NPP results to the abstract in the revised manuscript.

Page 12939, Line 1: clarify if you refer to autotrophic, heterotrophic or ecosystem

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respiration here

We are referring to ecosystem respiration. Therefore, we will add “ecosystem” before “respiration”.

Page 12939, Line 18-19: because of a CO<sub>2</sub> fertilization effect of due to the changes in temperature and water cycle? Clarify.

Yes, we will reword this sentence to make it clear

Page 12940, Line 11-13: This observation is likely confounded by the simultaneous development of vegetation biomass. E.g. A day with 20 degC in April would have likely still resulted in a low CO<sub>2</sub> flux. I suggest revisiting this statement.

We agree with the reviewer. We have revisited this statement and will replace “due to” with “coincident with” (See line 70).

Page 12942, Line 22: remove ‘to’

This “to” will be removed.

Page 12943, Line 1: remove ‘severely sensitive’ or explain what is meant by that and on what criteria this judgment is based on?

Removed.

Page 12943, Line 3: replace ‘C balance’ with ‘CO<sub>2</sub> balance’

As we mentioned above, this will be done throughout the manuscript where applicable

Page 12943, Line 23: mention the water table depth below surface for the control site

We will now mention the water table depth below surface for the control site as “~38 cm below surface”.

Page 12946, Line 4: how are the chambers ‘corrected for transmittance’?

Thanks, asking for clarification. The chamber was corrected for transmittance in two

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steps: first, the portable PAR sensor was compared / calibrated with a standard PAR which was connected to the onsite weather station for continuous data logging (line 21-22 of existing manuscript); second, the portable PAR was placed and levelled inside the chamber during a clear sunny mid-day. Instantaneous data was then collected concurrently from both the open weather station PAR and the chambered portable PAR. The actual transmittance read by the portable PAR was calculated to be 88% of the open PAR.

Page 12946, Line 10: outside or inside the chamber? If outside then the PAR measurements overestimate the inside PAR by 12%? Clarify.

All CO<sub>2</sub> data collected using the portable PAR was multiplied with a factor of 0.88 to obtain corrected values of PAR. This sentence will be added to the revised manuscript.

Page 12946, Line 23-28: I cannot find the logic or point in this section, what does 'therefore' refer to and how does the nighttime CO<sub>2</sub> flushing effect (commonly observed in automated chamber systems) relate to the arrangement of the daytime manual measurements conducted in this study? Reword and clarify.

Thanks a lot for asking for clarification. We did not compare our day time CO<sub>2</sub> flux measurement methods with those of night time measurements; therefore, this section is not required and will be deleted.

Page 12947, Line 5-10: I disagree. One should be able to see a linear CO<sub>2</sub> concentration increase by diffusion if the chamber volume is large enough and the sampling duration adequate. I don't understand how the 'manipulation of the spontaneous CO<sub>2</sub> fluxes across the soil-vegetation-continuum' should affect the linear increase caused by diffusion. Surely, at some point the increased concentration in the headspace will slow down the diffusion rate but if the area and chamber volume are chosen well then this effect can be avoided. Even if the diffusion rate slows down at longer measurement times one could use the data from the initial linear phase only. A better or clearer explanation and justification for the choice of an exponential equation is needed here.

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It is true that an increase in CO<sub>2</sub> concentration in the chamber headspace should be linear if the chamber volume is large and is corrected for inside temperature and pressure following ideal gas law. However, covering the in situ soil and vegetation has been frequently reported to manipulate the spontaneous CO<sub>2</sub> fluxes across soil-vegetation-air continuum (Hanson et al., 1993; Davidson et al., 2002; Denmead and Reicosky, 2003; Kutzbach et al., 2007). The flux manipulations in the chamber headspace were found to be due to suppression of natural pressure fluctuations (Hutchinson and Livingston, 2001) and alteration in turbulence between measured intervals which resulted in underestimation of fluxes by upto 40% (Kutzbach et al., 2007). Therefore, an exponential regression was used following Munir et al. (2014) who determined that the exponential regression was actually a better fit than the linear regression. Also, to avoid any potential effect of slowdown of the diffusion rate we used the flux data from the initial phase to obtain stronger (actual) exponential relationship between flux and time (Hutchinson et al., 1993). The exponential regression was also supported by its better adjusted R<sup>2</sup> values than those of linear regression R<sup>2</sup> values between CO<sub>2</sub> flux and timed intervals. We will add these descriptions to the revised manuscript in terms of the suggestions.

Page 12949, Line 5-17: 1) Please clarify what these R<sub>r</sub> plots are, do they include both the trenched and control plots? If this acronym only refers to the trenched plots and the surface vegetation was only removed only there but not at the intact control plot where respiration from ground vegetation was included then the difference between R<sub>r</sub> and control plots would not equal tree root respiration but to that of tree root + vegetation respiration. 2) It seems odd to call the trenched plot R<sub>r</sub> (Respiration roots) if this is actually the plot that does not contain any roots, 3) specify the timing of the trenching. Usually this is must be done the autumn before the measurement year to avoid the initial burst in decomposition of cut off roots to result in overestimation of the flux from the trenched plot, which might occur if the trenching was done in spring just prior to the measurement season.

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Thanks for asking for clarification: 1) Rr is the acronym for tree root respiration (defined at page 12940 line 28 of existing manuscript). Tree root respiration (Rr) was calculated as a difference in respiration between the intact and trenched plots while surface vegetation was removed periodically from all plots; 2) It is already clarified that Rr is not a name for the plots but it is acronym for tree root respiration; 3) The plots were trenched in early May 2012 while respiration measurements were carried out in July-September 2012. We agree that while the trenching is used to separate Rr from Rff, it also adds fresh litter to the peat that can add to the existing heterotrophic soil respiration. It has been assumed in trenching experiments that the trenched roots die off within a short time and that afterwards the measured Rff can solely be attributed to heterotrophic soil respiration (Hanson et al., 2000; Hermle et al., 2010; Wang et al., 2008). Trenching immediately disrupts the supply of recent photosynthates to the roots and mycorrhiza and associated bacteria that suffer from the lack of labile C. In trenching experiments Bowden et al. (1993), Boone et al. (1998) and Rey et al. (2002) have shown that C content of decomposing fine roots in trenched plots contributed little to Rr and becomes stable a few months after trenching. The root exclusion experiment may not be useful if extended through a complete annual cycle, as over such a long period there is the possibility of reinvasion of roots into the previously root-free trenched plot (Edwards and Norby, 1999). While it is clear that findings from such trenching measurements should be interpreted carefully, the primary focus of this paper is to quantify Rff while investigating Rr to better understand and separate the contribution of various processes to shifts in Rff following drainage. We have added these descriptions to the revised manuscript in terms of the suggestions.

Page 12951, Line 4: how did the authors come up with this value of 17%?

Szumigalski and Bayley (1996) and Thormann and Bayley (1997) measured tree litter in an Alberta treed bog. They found that tree litter was equal to 17% of the above-ground incremental biomass of the trees. This description is present at lines 22-26 of existing manuscript.

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Page 12955, Line 14: 'did not increase'

Thanks for correction. It will be corrected in the revised manuscript.

Page 12955, Line 23: Introducing the new term 'canopy-layer biomass' is confusing, why not simply call it 'aboveground tree biomass' which is also the respective term used in the following text.

We agree with the reviewer and will adopt the suggested term.

Page 12958, Line 19: the unit needs some temporal component, e.g. per growing seasons (gs-1). Also, when comparing the total sums of fluxes between the two years, do these represent the same growing length or is an adjustment with season length necessary?

Thanks! The growing season length (1st May to 31st October) was consistent through the three study years. The growing season length will be mentioned in the revised manuscript in terms of the question.

Page 12958, Line 20-23: Avoid discussion elements in the result section

This will be deleted.

Page 12959, Line 15ff: I suggest including an estimate of error or uncertainty around these values to support the comparison among treatments and years.

Thanks! We will include the uncertainty values around these values to support the comparison among treatments and years. Also these values are compared in detail in Table 4.

Page 12960, Discussion: To make the discussion section better readable and structured I suggest to subdivided it into subtopics e.g. 4.1 weather effects, 4.2 water table effects, 4.3 OTC warming, etc

We will insert the suggested and appropriate subtopics in the discussion section of the

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revised manuscript to help improve readability.

Page 12964, Line 25: How is the response of trees to warming considered and incorporated in this discussion?

We did not apply warming treatment to trees but to the ground layer only. Therefore, warming effect on trees is not considered or incorporated. However, a warming treatment to trees should be considered in further studies to investigate the response of trees to climate change. We will make sure to highlight this in the discussion and conclusions of the manuscript.

Page 12965, Line 10: I suggest mentioning the values reported by Moore et al 2002 for comparison.

We will mention these values in the revised manuscript.

Page 12966, Conclusions: No results should be repeated in the conclusion section, instead it should consist of broader implications and conclusions (i.e. 'take home messages')

Thanks! We have omitted the results from the conclusions. Only broader implications and conclusive statements have been kept.

Page 12967, Line 19ff: The discussion of aerated peat thickness and water use efficiency in the conclusion section is out of place. If the authors have data on these aspects they could be presented and discussed earlier in the manuscript.

Thanks a lot for this suggestion. We will remove discussion of aerated peat thickness and water use efficiency from the conclusions, and improve the conclusions section in general in the revised manuscript

Page 12978, Table 4: how was growing season defined? Based on air temperature thresholds? Thanks for asking for clarification. To stay consistent, we kept the growing season the same through all study years, i.e., 1st May to 31st October. This is defined

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at many places throughout the manuscript as “May to October”. We will also add the exact extent of the growing season as a footnote to the table.

Page 12983, Figure 5: Should the legend say CO<sub>2</sub>-C as suggested in the Figure caption?

Thanks! Yes, it should. We will correct this in the revised manuscript.

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