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Interactive comment on "Lateral carbon fluxes and CO_2 outgassing from a tropical peat-draining river" *by* D. Müller et al.

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Response to Anonymous Referee 1

We thank you for your constructive feedback and your detailed comments and suggestions. Please find our replies to all specific comments below.

General comments: This is an interesting paper worth publishing. Tropical peatlands are an important part of the global carbon cycle and large areas are being converted to commercial use which has an impact on the carbon storage and dynamics. This paper makes a valuable contribution by providing baseline data on concentration and age of carbon released from pristine tropical peatlands hence aiding the assessment of the effects of disturbance in other studies. The authors

also measure pCO2 in this river draining pristine peatland which may provide an important indicator on the peat decomposition. CO2 efflux is influenced by water flow velocity and turbulence and therefore comparing it to more degraded systems may be difficult unless information on hydrology is also available hence pCO2 data provided offers a better point of comparison. The paper is generally clear and well written, some minor technical corrections are listed at the end of this file. The authors also discuss uncertainty thoroughly which is important in addressing the limitations of the study.

Specific comments: My main comment relates to the upscaling of the results to an annual budget. I appreciate that detailed measurements were not available for calculating discharge and the authors handled the issue by using multiple ET values to derive an estimate. However, why was the annual precipitation for 2013 used for both 2014 and 2015? The authors mention that 2015 was particularly wet with flooding so it doesn't sound like 2013 rainfall values would be entirely appropriate. Given that year 2015 is not yet complete, what about using rainfall one year backwards from end of each sampling (April 2013-March 2014 for 2014 and April 2014-March 2015 for 2015)? Or perhaps producing just one export budget using the pooled TOC data from both sampling years and maybe a long term average rainfall? Another limitation of producing the annual budgets is that the authors used the average TOC concentrations which were solely measured during decreasing discharge whereas concentrations are expected to vary between seasons. The authors themselves mention that DOC is expected to decrease during the peak monsoon and that it was lower during the rainier 2015. They also discuss issues related to the lack of seasonal measurements. Given that there is considerable uncertainty in TOC values too, perhaps it is not worth upscaling the 2014 and 2015 values separately especially if precipitation data is only available for a single year (2013)?

Additional precipitation data were now kindly provided by the Department of Irrigation and Drainage (DID) Sarawak for the years 2012-2015. Therefore, we followed your first

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suggestion and calculated the TOC yields backwards from the end of each sampling. As a result, the TOC yields are now somewhat lower than before, so that we had to adjust our discussion on page 10407, line 15.

p. 10401 line 20 "The river was strikingly undersaturated in oxygen, ranging from 29 to 58 μmol/L in 2014 and from 26 to 42 μmol/L in 2015." Could the authors report what the saturated concentration would be in study conditions either in the text or on the figure 3?

The average oxygen saturation concentration was 262 μmol/L in 2014 and 263 μmol/L in 2014. In-situ oxygen saturations ranged between 11 and 22 % saturation in 2014 (NP samples) and between 9 and 20% in 2015. We included the saturations in the revised text, but we would prefer not to change the Figure, as we would have to extend the vertical axis – the dissolved oxygen values in the water were nowhere near saturation.

p. 10402 line 5 "The age determination of our two samples from 2014 revealed that DOC contained 106.6 $\pm$ 0.3pMC and 106.1 $\pm$ 0.4pMC, indicating a large contribution of modern carbon to the overall sample age." –From which sampling points were these two samples collected?

One sample was taken at the most upstream station (river km 14) and one sample was taken further downstream (river km 8). We added this information in the Methods section of the revised manuscript.

Figure S3 what is the significance of dark blue on the graph?

Dark blue refers to the two sigma calibrated age range. We added this information in the caption.

Figure 4 a, why is there a gap in the CONTROS pCO2 data?

There was a technical problem with the data cable, so that data storage was interrupted. We fixed the problem during our overnight stay in the Maludam national park. We added a sentence in the revised text explaining the gap in the pCO_2 data.

p. 10403 line 16 "CO2 concentrations showed a weak negative relationship with

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DO" – Was it significant? Was this true in 2014? It does look like it on the plot.
It was significant in 2015 ($r = 0.74$, $p<0.0001$) but not in 2014. We added this in the revised manuscript.

p. 10404 line 7 "(V = 0.2 m/s)" –what was the standard deviation? Did the efflux rate increase with flow velocity?
$V = 0.2 \pm 0.1$ m/s – we added the standard deviation in the revised manuscript. Since we measured velocity only in 2014, and we conducted floating chamber measurements only at four stations during that year (see Fig. 4b), we could not establish a relationship between flow velocity and flux.

Minor technical corrections:
page 10392 line 14 (Miettienen and Liew, 2010) is this the correct spelling of the first author? Should it be Miettinen?
This is correct, we misspelled it. This was corrected in the revised manuscript.

p. 10419 Fig 1 caption " the diamond shows" for clarity change to "the green diamond shows". Strictly the grey squares are not dots, this could be changed to "grey and black symbols denote sampling locations".
This was changed in the revised manuscript.

p. 10396 line 19 "No bigger rainfall events occurred during the campaigns." Bigger than what? Would you have information to state the maximum rainfall? If not, maybe just change to "no large rain events occurred".
We rephrased that no large rain event occurred. There was some drizzling in 2014, but we have no means of quantifying the precipitation rate during this event.

p. 10400 line 18 "..we used the annual average precipitation for the year 2013." Should this say total annual precipitation? As far as I can see data came from one station not as average of many.
Yes, this was to say the total annual precipitation at Maludam station. We corrected this.

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p. 10401 line 6 "In the NP, all samples contained freshwater, as indicated by a low conductivity between 72.5 and 100.3 μS/cm (2014)." –were the samples only from 2014?
Conductivity was measured at every station in 2014 with the WTW sensor. The WTW data logger had to inlets, which were used for oxygen and conductivity in 2014, while pH was measured with the HANNA pH meter. In 2015, those two inlets were used for the oxygen sensor and a pH sensor. Therefore, conductivity was only measured on the way upstream until it was obvious that no marine influence was present. This point was already reached at the first station, which was located close to the border of the national park and exhibited a conductivity of 71.3 μS/cm. After that, we have no record of conductivity, but we assume that it is <71.3 μS/cm as all other samples were taken further upstream from station 1. In the revised manuscript, we added the information that 2015 samples are all expected to contain freshwater, as a conductivity of 71.s μS/cm was detected at the station that was farthest downstream.

p. 10410 line 4 "..by employing the floating chamber.." -should it be "by deploying"?
Yes, this was corrected.

Figure 2, 3 and 4a There seems to be an error in the legend as the black and grey symbols are repeated whereas the I guess there should also be yellow symbols.
We added the yellow symbols in the Figures 2, 3 and 4a of the revised manuscript.

Figure 5 the symbols are repeated unnecessarily as there are no yellow ones on the graph.
We don't full understand this comment. There are no yellow symbols neither in the graph nor in the legend.

Figure 4b is lacking a legend or the caption a statement that the symbols are the same as in the previous figures.
We added a statement that the legend applies to all panels in the captions of Figures

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2, 4 and 5 of the revised manuscript.

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Reply to Anonymous Referee 2

We thank you for your feedback and your helpful comments on our manuscript. We provide detailed answers to your comments below.

GENERAL COMMENTS Tropical peatlands are under tremendous pressure from extractive industries (e.g. timber, mining, etc.) and agriculture, with potentially wide ranging consequences for regional/global C balances, climate, water quality and biodiversity loss. We have very little data on fluvial C fluxes from many tropical peatland ecosystems, particularly from Southeast Asia, where the pace of land-use change has been extremely rapid in recent decades. This manuscript

is therefore interesting and novel because it is one of the few studies to provide baseline data on fluvial C fluxes prior to land-use change, and could serve as a useful touchstone for future studies of anthropogenic impacts on C fluxes from Southeast Asian peatlands.

However, there are some limitations to the research here; for example, the approach used to estimate annual fluvial C fluxes is based on the calculated difference between precipitation (PT) and evapotranspiration (ET) (see page 10400), and is subject to uncertainties in measured rates of ET and the assumption of steady-state conditions.

Fluvial and gaseous measurements were also only conducted in a single season in both years (i.e. post-monsoon). Lastly, gas evasion measurements provide only a partial picture of water-air exchange, because there are questions as to how spatially representative the measurements were, and if water-air exchange is influenced by wind/turbulent flow (although the authors argue this is a non-issue because of relatively sheltered river conditions).

Yet despite these limitations, I believe this study makes a valuable contribution to the wider literature on tropical peatlands, because so little is known about undisturbed peatland systems, particularly in Southeast Asia. Moreover, much to the authors' credit, they have openly and transparently discussed the potential sources of uncertainty in their measurements (see section 4.4 in the Discussion). This is to be commended because it enables readers to assess the data for themselves, acknowledges any potential biases in the estimates of C flux, and also provides a starting point for identifying how future studies of this kind could be improved. In my overall assessment, this is good work, given the challenging field conditions and limited infrastructure, and will extend our knowledge of these regionally/globally important but understudied ecosystems.

Specific comments on individual sections of the text are provided in the section

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below.

SPECIFIC COMMENTS

1. Page 10395, line 19: Use of abbreviations like “NP”. This is a subjective stylistic point, but where possible, I think the text would read more elegantly if abbreviations were only used sparingly. While in some commonly-used terms like “Peat Swamp Forest” (PSF) might be better abbreviated due to their length, other shorter terms (like “National Park”) may be better referred to in full. Abbreviations tend to interrupt the flow of the text, and I prefer only using abbreviations for very wordy or long terms.

We avoided the abbreviation “NP” in the revised manuscript.

2. Page 10396, line 27 to Page 10397, line 29: In the section on sampling procedure and instrumental analysis, it would be useful for the authors to state the precision of their measurements for the TOC, AMS and IRMS (typically reported as the Coefficient of Variation for the standards). Precision is reported for the IRGA/headspace method described on page 10398, line 13 (i.e. <2.5%), so no need to discuss this further here.

This was done in the revised manuscript.

3. Page 10402, lines 20-24: Do the investigators have the C/N ratio of the surrounding peats? Depending on the degree of microbial processing, many peats typically have C/N ratios similar to that of plant material (e.g. 40-60), with higer C/N values more common in forested peatlands with a larger proportion of woody debris. It is therefore likely that the DOC & POC consist of a mixture of phytoplankton, terrestrial plant material AND decomposing peat C, not simply the first 2 constituents.

This is a valid point. Although we do not have C/N ratios for peat, we added the possibility of decomposing peat as a source of POM with reference to C/N ratios measured by Baum 2008 in Indonesian peat.

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4. Page 10402, line 25: Do any data exist on the 15N values of organic material? These may provide useful insights into the degree of N-limitation in the system.
d15N analysis was only performed on 2014 POM samples. Those samples were first analyzed for C/N contents and then for isotopes. However, the amount of each sample available for isotope analysis was insufficient for the determination of d15N with the exception of three samples. Of those three samples, only two were taken inside the national park and one was taken downstream of a waste water treatment facility outside the national park. Therefore, only two samples are actually representative of our study system. Their d15N values are 2.35 and 2.24 ‰ which is very slightly enriched if compared to peat d15N (Baum, 2008), suggesting that fractionation is small and that the system is N-limited. However, this data seems too scarce to support an actual statement about nutrient limitation, also seeing that the origin of POM cannot be assigned to only one source, which means that the d15N value is a mixed signal. Therefore, d15N is not so insightful, which is why we prefer to leave it out of the revised manuscript.

5. Page 10404, lines 15-22: With respect to CO2 fluxes, it is possible that some of this apparent "spatial" variability may also be reflective of temporal variability/antecedent conditions. For example, if there were sustained winds or large gusts prior to sampling, surface waters may have become depleted in CO2 due to enhanced outgassing driven by turbulent flow. In addition, spatial and temporal variability in conditions might synergistically interact. For instance, if certain stretches of river are more protected from the effects of wind, it is possible that they will (relative to more exposed reaches) show consistently higher dissolved CO2 concentrations and higher apparent diffusive fluxes, because there could be less turbulent fluxes from the water-atmosphere interface.
We added this point to the discussion.

6. Page 10405, lines 14-15: Consider slightly rephrasing the sentence "Enhanced CO2 is generally associated with oxygen depletion..." as this could be misinter-

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preted to mean that more anaerobic conditions reflect or are conducive towards greater organic matter decomposition. Revising this sentence could make the meaning clearer, e.g. "Enhanced CO_2 is generally associated with oxygen depletion, with lower oxygen levels reflecting high levels of organic matter decomposition and subsequent oxygen consumption by heterotrophs..."
We changed the sentence according to your suggestion.

7. Page 10405, lines 16-17: Part of the "natural variability" could also arise from the fact that this is an open system, and oxygen re-charge from the atmosphere could obscure/confound the effects of heterotrophic oxygen consumption. Without direct measurements of biological oxygen demand, it would be challenging to find very strong relationships between CO_2 and O_2.
Unfortunately, we do not have measurements of BOD. As we expected O_2 to be replaced approximately as quickly as CO_2 is released, we expected a somewhat stronger relationship between CO_2 and O_2. However, as the data shows, due to the fact that it is an open system, as you say, the relationship is not very strong. We added some additional discussion in the revised manuscript.

8. Page 10406: With respect to the 14C data, would it be possible for the authors to estimate or speculate as to what proportion of the DOC was arising from recent material and how much from older carbon? Do the authors have 14C estimates for the peat material and more recent plant compounds? The 14C data (potentially combined with 13C data) could assist in partitioning the decomposition sources into old versus recent material, depending on the precision of the 14C measurements.
Unfortunately, we do not have 14C data for peat, for leaves and neither do we have 13C data for DOC (only POC). However, Raymond et al. (2007) used an age attribution model in order to resolve the contributions of different time periods to the bulk DOC. If we use the age attribution model for the years after 1970 (as did Raymond et al. 2007), assuming that the peat column is intact and that the bulk DOC-age is

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indicative of post-bomb carbon, we find that 71 % is less than ten years old, and 91 % is less than 20 years old. This is in line with the average age obtained using the CaliBomb program. Note that this age attribution model has certain limitations, e.g., the 14CO2 curve used (Hua et al., supplemented with data from Levin et al.) extends only until 2012; and the assumption of a constant decay rate down the peat profile (Raymond's model 1) seems unwarranted. At the same time, this age attribution model does not change or improve our interpretation, which is, that DOC is mainly derived from recently fixed carbon. Given those limitations, we would prefer not to include the age attribution model in the revised manuscript.

9. Page 10406 – 10407, section 4.2: It would be useful, within the context of understanding the effects of land-use change, if the authors could draw some comparisons with fluvial C fluxes from managed/human-affected tropical peatlands. Simply speaking, are the fluxes from this near-pristine system on par, lower or greater than for human-affected systems?
There are some constraints to this comparison. First, since the TOC yield seems to depend on the peat coverage, we have to find a system with the same peat coverage (100%) for comparison. Such systems were studied by Moore et al. (2013), so they offer a point of comparison. However, the second challenge are the different characteristics of the peatlands, e.g., due to higher precipitation in Sarawak if compared to Central Kalimantan. Nevertheless, we compared our data to that of Moore et al. and discussed those constraints.

10. Page 10407, section 4.3: Two points; first, similar to point 9 above, would be comparisons of gas evasion from this system compared to managed systems (if these data exist). Second, could the authors elaborate on this concept of short residence time and CO2 concentration/gas evasion rate? From other studies, what would be considered moderate or long residence times? How would this difference influence CO2 fluxes and what type of mathematical relationship does water residence time have on gas evasion rates? E.g. is gas evasion rate linearly

related to water residence time?
First point: Unfortunately, this data does not exist. To our knowledge, there is no published estimate of CO2 evasion from a tropical peat-draining river, managed or unmanaged, other than ours.
Second point: We don't know of any publications that have established a relationship between in-stream water residence time and gas evasion, but it was mentioned before that it is an important factor (e.g., Cole et al. 1994, Battin et al., 2008, 2009; Moody et al. 2013), mainly for two reasons: Firstly, longer in-stream residence times facilitate equilibration between soil and water. Secondly, DOC decomposition occurs exponentially over time (e.g., Rixen et al., 2008), and therefore, decomposition rates are integrated over time. As a result, the relative amount (in %) of DOC decomposed in a peat-draining stream depends on in-stream residence times (Moody et al., 2013). Conclusively, in systems where in-stream DOC decomposition is a relevant source of CO2, in-stream residence time exerts a control on the buildup of CO2 in the stream (as suggested for lakes by Cole et al., 1994 and for a Hawaiian river by Paquay et al., 2007). When residence times are short, a relatively smaller fraction of the DOC will be degraded, and CO2 buildup is moderated. We added some of this additional explanation and the additional references in the revised manuscript.

11. Page 10408-10409: With respect to the uncertainty estimates, one thought I had is that it might be possible to show the range of estimates for the different fluxes in a table? For example, reporting the median, mean, range, minima and maxima for each of the fluxes? This might be a straightforward way of condensing this information.
We provided a summary table for the 2014/2015 TOC/CO2 data and the TOC and CO2 fluxes as suggested.

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