

Interactive comment on “Impact of peatlands on carbon dioxide (CO₂) emissions from the Rajang River and Estuary, Malaysia” by Denise Müller-Dum et al.

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We would like to thank you for taking the time to review our manuscript and for the overall positive evaluation. Below, we detailed how we are going to address your specific comments.

The manuscript (MS) submitted by Müller-Dum et al. investigates the C exports from the Rajang River and Estuary (Indonesia) based on sampling cruises during wet and dry season. That includes observations of CO₂ partial pressures (pCO₂), calculation of CO₂ emissions from the water surface, and lateral exports of DOC, POC, and DIC. pCO₂ and emissions are detailed for the peat-draining, non-peat-draining and estuar-

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ine parts of the river. One important result is that although the peat cover in the basin is significant, its contribution to C exports from the river system is not visible, as the peatlands are concentrated around the river delta. The manuscript of Müller-Dum et al. is of interest for the readership of Biogeosciences, because it reports the first pCO₂ and CO₂ emission estimates of this important river in SE-Asia, which is surprisingly different from what would have been expected from observation from over peat draining rivers in this area. The methodology is well described and seems to be sound. The MS is in most parts well written. The results support the main conclusions drawn in the MS. The discussion of results is thorough and covers well the state of the art with respect to literature references. I suggest the publication of the MS after some moderate revisions. Please, find my comments to the authors below.

Major comment: You have been measuring pCO₂ for quite different parts of the delta system delta (estuary and peat part of the river network) during the wet and the dry season. That becomes quite apparent from the figure 4. Did you do anything to compensate for the discrepancy in observed delta parts? If not, I would suggest that you calculate and report the average wet and dry season pCO₂ only for the parts you have been sampling in both seasons.

We agree that in order to compare data from two seasons, we have to make sure to compare data from the same geographical extent. This was done for the non-peat reaches of the river, as they extended from Kapit to Kanowit during both seasons. The peat reaches differed geographically, due to the different salt intrusion limits during wet and dry season. For the estuary reaches, we reported the averages for the two different seasons accompanied by their salinity and the remark that different locations were sampled. However, we see how this might be confusing. Therefore, in the revised paper, we will try to make the comparison more precise by comparing distinct parts of the river: non-peat (Kapit-Kanowit), peat that was non-saline during both seasons (Kanowit-Sibu), and the delta. This way, at least peat and non-peat areas are directly comparable because they geographically overlap. We think that using these three “new” categories does not only make more sense, it also improves the readability

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of the manuscript, since only three categories are used (non-peat, peat, delta) instead of five (which were partially overlapping: non-peat, peat, estuary, delta, freshwater).

At the same time, we would take your comment into account about weighting emissions by stream width. This is now easily possible, as a recent publication by Allen and Pavelsky (2018) provides us with stream widths for the entire river. With those at hand, we are able to calculate CO₂ emissions for each category separately using specific stream widths. In addition, we will estimate the fraction of the catchment that belongs to each category and calculate area-weighted means of the measured parameters (SPM, POC, pCO₂, DO. . .).

In Summary, we suggest the following main changes to the manuscript:

- Definition of the three categories (their characteristics like water surface area, catchment fraction etc. will be summarized in an own Table)
- Instead of reporting freshwater averages and then peat/non-peat averages, values will be reported for the three categories peat, non-peat, delta, and in addition, an area-weighted mean will be calculated for peat and non-peat area combined. This way, we could merge what are currently Table 1 and 2.
- River loads will be recalculated using the new categories.
- The estimate of river surface area will be improved by using the now available GRWL Database.
- The estimate of catchment area will be improved and catchment fractions will be calculated for the three categories.

Further minor changes will be made as detailed below.

General comments:

Abstract: The abstract is comprehensible and summarizes well the main findings. However, the abstract would need some minor restructuring:

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P2, L8-9: It's not easy to see here how these DIC and delta13C values show that peatlands are not the main source. That would require some more explanation within the abstract. Maybe you could discard these two number from the abstract.

Agreed, we will delete the sentence about DIC and delta13C.

P2, L10: This sentence is repeating what was stated two sentences before.

We will delete this sentence in the revised manuscript.

P2, L10-12: "Thus: : :". I feel this sentence should conclude the abstract.

We will move this sentence to the end of the abstract.

P2, L13-15: "CO2 fluxes: : :". This sentence should come slightly earlier and directly follow your statements related to the pCO2.

We will move this sentence up, so that the statement about the CO₂ fluxes directly follows the statement about pCO₂ values.

Introduction:

P3, L2-3: Make clear that you are talking about terrestrial derived C fluxes.

We will change the first sentence to: "Tropical rivers transport large amounts of terrestrially derived carbon to the ocean and the atmosphere (Aufdenkampe et al., 2011; Raymond et al., 2013)."

P3, L13-14: Could you report the proportion of the water flux for comparison?

We will add the following information: "Because of these high DOC concentrations, Indonesian rivers may account for 75 % of the DOC flux into the South China Sea (SCS) while accounting for 39 % of the discharge (Huang et al., 2017)."

P3, L25-26: Did you do longitudinal transects from no-peat-influenced river reaches to river reaches surrounded by peat? If yes, it would be good to state that here.

Yes, we will add a sentence for clarification: "To this end, we surveyed longitudinal transects from river reaches that were not influenced by peat to the peat-covered delta."

P3, L26-27: Maybe you should discard that last sentence.

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The sentence “We expected to see a clear peat signal, i.e. elevated CO₂ concentrations in the peat area” will be deleted.

Methodology:

The only thing I miss is an explanation why you observed the delta13C of DIC, and maybe the endmembers you used for your isotopic mixing model, if you applied one.

We will add the following justification: “In August 2016, water samples were also taken for the determination of dissolved inorganic carbon (DIC) and the isotopic composition ($\delta^{13}\text{C}$) of DIC, because the isotopic composition of DIC can help in identifying its sources.” An isotopic mixing model was not applied.

Results

P9,L5-12: With regard to the positive correlation between delta13C and DIC concentration in the estuary: What is the marine endmember of delta 13C in DIC here?

We measured a marine end-member at one station off the Rajang River mouth during our survey in August 2016 (DIC = 2347 $\mu\text{mol/L}$, $\text{d}13\text{C-DIC} = -1.97 \text{ ‰}$). This value (for DIC) was used for the CO₂ mixing model as described in the Supplement. If you mean a calculated (theoretical) end-member, it depends on the DIC value that we consider marine, and that, in turn, depends on the salinity that we consider the marine end-member to have. In this part of the South China Sea, a salinity of 33 can be considered marine. The calculated DIC end-member would be 2310 $\mu\text{mol/L}$ at this salinity, and the calculated $\text{d}13\text{C-DIC}$ would be -2.96 ‰ . We think that using the measured end-member is more appropriate, as done in the CO₂ mixing model. However, since we are not applying an isotopic mixing model, we found it unnecessary to mention these numbers in the text. However, in the revised manuscript, we will add a Keeling plot showing the regression line for freshwater samples, where the y-axis intercept should give the source $\text{d}13\text{C-DIC}$.

With regard to the negative correlation between delta13C and DIC concentration in the freshwater part: Is that correlation even stronger between delta13C and pCO₂?

No, it is weaker. This can partially be explained by the sampling: $\delta^{13}\text{C}$ and DIC were

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analyzed from the same discrete water sample. CO_2 , in contrast, was monitored with the equilibrator, so taken from a slightly different spot (the water sampler and the water intake for the equilibrator were in the front and rear of the boat, respectively).

P9, L8: "Calculate DIC for the wet : : :". For which part of the river network? The freshwater part? Please, clarify!

Yes, this value states the calculated DIC for the freshwater part of the river. In the revised manuscript, this sentence will have to be changed due to the new three categories.

P9, L21-13: Is it possible to distinguish $p\text{CO}_2$ observations you made during high, rising, falling, and low tide during your cruises? Or were your cruises in the delta predominantly done during a specific part of the tidal cycle? Were those different for wet and dry season cruises?

Tidal variability of $p\text{CO}_2$ was only observed at the river mouth. In large parts of the delta, $p\text{CO}_2$ was relatively uniform, as can be seen in the Figure below. The only information about tidal variability was therefore obtained from the stationary measurement in Sarikei at the Rajang River mouth, where tidal variability is quite substantial for both water level (up to 5 m) and $p\text{CO}_2$ (between 2000 and 6000 μatm). We will show the time series of $p\text{CO}_2$ and the water level for both January and August in the Supplement of the revised manuscript. Please see Fig.1 below.

P9, L27-28: Does that mean you cannot distinguish the diurnal variations from tidal variations for the delta? And you do not have enough data from the non-tidal part to identify a diurnal signal? Please, clarify.

This is both correct. In the tidal part, we cannot distinguish tidal and diurnal signals, because we essentially have one stationary measurement that was conducted overnight. This measurement (will be shown in the Supplement, see comment above) suggests that the variation with tide dominates over the diurnal variation. In the non-tidal part, we do not have enough night-time data to identify a diurnal signal at all. We will add the following explanations: "Unfortunately, our data did not allow to identify a diurnal signal

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of either $p\text{CO}_2$ or DO. In the tidal part of the river, we have only the one stationary measurement overnight, when a diurnal signal could not be identified due to the strong tidal signal. In the non-tidal part of the river, we do not have enough night-time data in order to make a sound statement about a difference between day- and night time $p\text{CO}_2$ and DO.”

P9, L30 – P10,L1: How did you calculate those gas exchange velocities? I see how your calculations compare well to the A11 model, but R01 model seems to be quite far off. Are those the results for the whole river system?

The calculation of gas exchange velocities is detailed in Section 2.3 and results are compared in the Supplement. When we report our main results here, the used parameterization for the calculation of the gas exchange velocity is stated in parentheses (k600, B04). We will add “using the B04 model” in this sentence to make it more obvious to the reader which model was used. It was also stated in the following that “Fluxes reported in this study are calculated from the B04 model, which yielded intermediate values. It was chosen because it recognizes flow velocity as a driver of turbulence in addition to wind speed. Results for the other two models are compared in the Supplement.” Those results are for the whole river system. The B04 model was constructed for estuaries based on measured relationship with wind speed and a parameterization for streams with current velocity as driver from the literature, thus seems appropriate for both river and estuarine reaches. As for A11, the river reaches that we studied ranged in width from 271m to several kilometers in the delta, thus meet the criteria for a “large river” defined by the authors (>100 m width). R01 used data from several rivers and estuaries to derive their predictive equation, among which are different rivers and estuaries for which the width is not given. After receiving your comments, we looked into it and found that the R01 rivers and estuaries probably range in width from 600m to several kilometers. This could be the reason why R01 differs so substantially from the other two models. However, R01 was used in our manuscript only for comparison, to show the range of possible values with implications for the uncertainty of emission estimates (which is always high when k is not directly measured). In the

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revised manuscript, we will present all three emission estimates as equally valid and present the range of values that they give instead of picking one of the models. This is due to a major comment of Referee 2, who suggested to improve the discussion of uncertainties that arise from the k models.

P10, L3-5: Those emission rates refer to the entire observed river network? Did you weight the emission rates along the longitudinal profile by stream width?

Those emission rates were calculated for the freshwater part of the river. They were not weighted by stream width, because the values represented emission rates per water surface unit area (gC/m²/d). In the revised manuscript, we will calculate those emission rates separately for peat, non-peat area and for the delta and calculated emissions (g/month) using the water surface area in each of the categories.

References

Allen, G. H. and Pavelsky, T. M. Global extent of rivers and streams. *Science* 361, 585-588, doi: 10.1126/science.aat0636, 2018.

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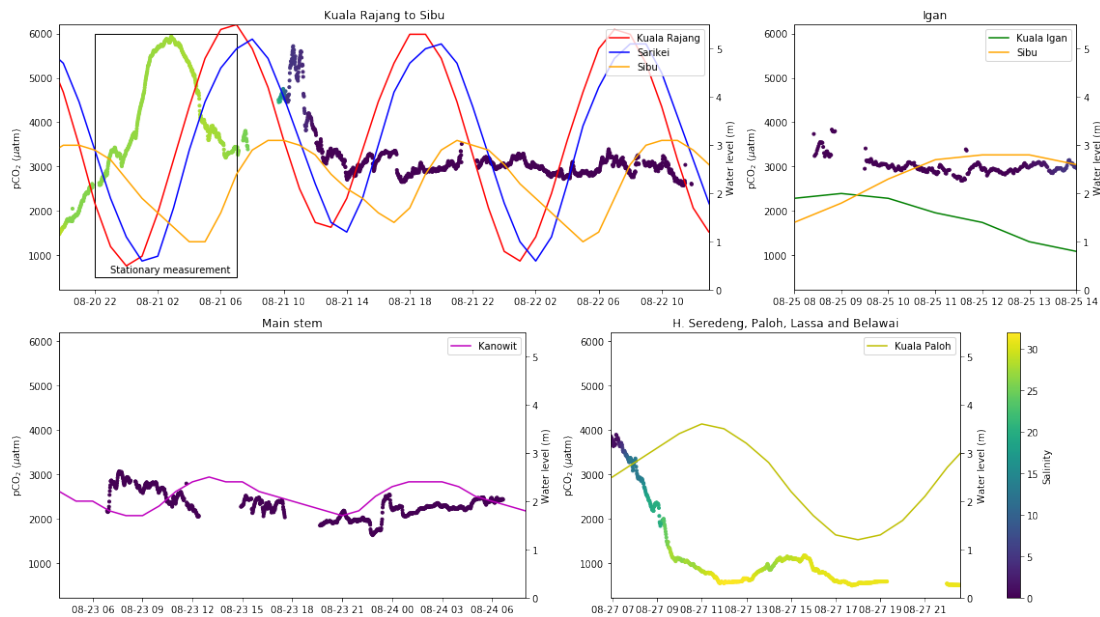


Fig. 1.

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