Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-391-RC2, 2018

Biogeosciences Discussions
Open Access

Interactive comment on "Impact of peatlands on carbon dioxide (CO_2) emissions from the Rajang River and Estuary, Malaysia" *by* Denise Müller-Dum et al.

Anonymous Referee #2

Received and published: 30 October 2018

Comments to the manuscript by Mueller-Dum et al., "Impact pf peatlands on carbon dioxide (CO2) emissions from the Rajang River and Estuary, Malaysia".

General comment: The manuscript focus on an important topic that I believe is suitable for publication in Biogeosciences. The transport and emission of carbon/GHG′s from river networks has repeatedly been concluded during the last decade as a highly significant component when for example estimating landscape C budgets at various scales and biomes. Although the importance is well-recognized, I would claim that relatively little is known about large rivers and their source contribution of atmospheric CO2. The knowledge that exists is largely restricted by the spatiotemporal resolution of the measurements or by using data being based on indirect measurements of pCO2. There is also a clear bias in existing data-sets towards northern hemisphere river networks and with limited information of tropical rivers, especially south-east Asian ones. In this context this study aims to fill an important gap in our understanding concerning large scale drivers of aquatic C in river networks. The influence of peat deposits in the catchment on the pGHG in the water has been shown for various biomes and river network sizes but more extensive investigations are needed. Hence, this is a highly relevant topic especially for a tropical region like this.

Although the aim of manuscript is important I have some concerns on how suitable the manuscript is for publication in its current form. My main concerns are: 1) How the actual emissions are calculated. I understand that this is a data scarce region but the way the authors have estimated the emissions is not especially convincing. The author's measure pCO2 in a satisfactory way but the entire k calculation component feels very shaky. No actual measurements of any of the input parameters are conducted. A vague estimate of a fixed water velocity is used in combination with modelled wind data. Three different k parameterizations are then used gaining slightly, to very, different outputs. The model producing intermediate k estimates are then used without any stronger further motivation. The whole procedure feels as I already said very shaky, without knowing anything about the river, investigating seasonal differences in emissions and then using a fixed water velocity sounds for example very strange. On top of these vague calculation steps there are no uncertainty estimate of the calculated emissions (or lateral exports of inorganic and organic C!!). To describe and estimate this in a transparent way would be a requirement in my eyes, especially due to the scarcity in data for the k calculations. If this is problematic to handle, one suggestion is to skip the emission data and solely present the pCO2 patterns and how it varies with wet and dry season and the influence of peatlands. Personally I think this would be the way to go and would be highly interesting in itself. 2) I am not totally convinced of the interpretations of the 13C-DIC data, I am surprised by the generally high 13C-DIC values, the authors claim that the contribution by carbonate containing bedrock to the

Printer-friendly version

Discussion paper

riverine DIC is minimal in the area and that the river is affected by tidal water sustaining the estuary with marine DIC. That is likely correct but the high 13C-DIC is found even in upstream non-peat area, is the evasion the sole explanation for that? Maybe not relevant, but what about methane production, I understand that methane might have been included in the original plan, but if methane in the peatlands is mainly produced by CO2 reduction this will heavily influence the 13C of the CO2 being delivered to the river (See Campeau et al. 2018 for example). Overall, I find the interpretation of the 13C-DIC data quite short and not as well developed as it could be. 3) Is it really correct to talk about seasonality when just two measurement campaigns are conducted, i.e. wet and dry season? I am not familiar with the region but to call something seasonality or similar would in my mind require a higher sampling resolution in time.

Detailed comments:

P3 Ln 1-10, there is a mix of wetland and peatland, consistency or a clear separation would be good.

P3 Ln 11, Odd formulation and scientifically a bit weird. To claim that something is the highest worldwide is only true until someone else present a higher number. I would recommend to be more open in this formulation.

P5 Ln 20-25 and 30, what about correction for salinity on the pCO2 and emissions?

P6 Ln 8-10 Isn't water velocity dependent on discharge, why is a fixed value used???

P6 Ln 10, Is there no wind data to validate this modeled data with? How accurate is the wind data compared to conditions over the river is tricky to judge. Feels very vague and uncertain!!!

Also, how was water depth measured, it is not mentioned as far as I see, but included in the B04.

Based on the fixed water velocity and fixed wind?? Is a constant k used for each season independent of location along the river?

P7 Ln 29-30, a bit odd that POC was measured but not DOC. Hard to redo the study but how relevant are the literature DOC values for this study, please motivate better!

P8 Ln 25, please clarify what pH that is for wet resp. dry season.

P9 Ln 10-12, was not the purpose to investigate if the peatlands have an influence on the pCO2 in the river. Feels a bit strange then to say that too few 13C-DIC samples were taken.

P9 Ln 20, here and elsewhere, what is “distributaries”, isn't just tributaries enough???

P9 Ln 21-23, important sentence but feels more like discussion than result!!

P9 Ln 27-28, again, feels more like discussion to me.

P10 L4, what does the +-0.52 and +-0.45 mean? Some kind of uncertainty or just spread? Please clarify in the methods. The emission rates (and lateral exports of C) are hard to get a feeling of, how uncertain are they? Impossible to judge for the moment.

P10 Ln 17-20, Feels from a reader perspective a bit odd to start to say that the findings are the same as found in other studies. I think the authors could “sell” their study better than that. It is important information but I would not place it first in the discussion. Also, maybe a matter of personal taste, but why not start with the main focus of the manuscript in the discussion (pCO2 patterns and maybe emissions if included), the SPM and POC story is secondary as I see it.

P12 Ln 11-14, Likely true but there is also a strong fractionation in 13C-DIC related to changes/differences in pH which could be up to ca 10 per mille.

Table 2.What is the +- of the emissions, the SE of the mean? I.e. some kind of measure of the spatial variability? Is this driven by something else than just variability in pCO2? Is k fixed for all data? According to the methods I get this feeling. Please clarify in the methods.

References: Campeau, A., Bishop, K., Nilsson, M. B., Klemedtsson, L., Laudon, H., Leith, F. I., Öquist, M. G., Wallin, M. B., 2018. Stable carbon isotopes reveal soil-stream DIC linkages in contrasting headwater catchments, Journal of Geophysical Research – Biogeosciences, 123 (1), 149-167, doi:10.1002/2017JG004083

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-391, 2018.