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Interactive comment on "Fate of peat-derived carbon and associated CO_2 and CO emissions from two Southeast Asian estuaries" *by* D. Müller et al.

D. Müller et al.

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We thank Anonymous Referee 1 for the positive evaluation of our manuscript and the constructive feedback. Please find detailed answers to all general and specific comments below.

General Comments: The authors describe a series of observations along two Tropical estuaries in Malaysia. Specifically, the authors present measurements of POC, DOC, CO2, CO concentrations and their respective isotopic signatures. In addition air-sea exchange measurements using a floating chamber are presented. These observations are discussed in the context of C-export from land to sea, C-transformations along

this route and emissions of CO2 and CO to the atmosphere. The authors found that substantial transformations of terrestrial C occur in the estuary, related primarily to biological (respiration) and photochemical processes. These processes moderate the export of organic C to the adjacent shelf and result in substantial efflux of CO2/CO from the estuary to the atmosphere. This is a very well written paper which provides rare data in an environment which is not well understood in the context of C-cycling and emission of greenhouse gases. I am sure that this work involved considerable logistical difficulties, but I really enjoyed the ingenuity shown by the authors in overcoming these. I have a number of comments which the authors should address, but have no hesitation in recommending this paper for publication in BG.

Specific Comments:
1) Abstract: The authors state that 'suspended matter. . . limiting the light penetration depth' was responsible for lower CO fluxes compared to other regions. I agree with this statement, but not everyone is a photochemist, so the authors should clearly state that suspended matter would be expected to supress CO photoproduction.
This will be pointed out in the Abstract of the revised manuscript as suggested.

2) p.8303, line 24: What is the tidal range for the two "macrotidal" estuaries?
The reported tidal range at the reference port for these two rivers, Pulau Lakei, is 3.1 m (Mean higher high water – mean lower low water). In the rivers themselves, tidal ranges of 3.5 m (Lupar) and 4 m (Saribas) are reported with reference to this port. In the publication, we will add this information.

3) p.8302, line 7: The authors state the "peat draining rivers exhibit extraordinarily high DOC. . .". Please give some representative values and references for the non-expert reader.
We will add three references (Müller et al., 2015, Moore et al., 2011, 2013) and state that DOC concentrations of up to 5667 μmol/L have been measured in tropical peat-draining rivers (Moore et al., 2013).

4) p. 8304, lines 10-15: Data are grouped by salinity for the two campaigns, but was there a geographic overlap as well? e.g. Did the "mid-estuary" regions overlap at all or were they in different places because of differences in river discharge?
There was a geographic overlap. Although this is not explicitly indicated, it can be seen in Figure 5. Note that the salinity distribution of the Lupar river is not displayed in this Figure, this was a technical issue. We will submit the revised Figure with the revised manuscript (see attachment). Additionally, we will indicate in the Results section that although water with higher salinities progressed slightly upstream during the dry season, the wet and dry mid-estuary location overlapped. We will also change the terminology to lower, middle and upper estuary, as mid-estuary salinities were also observed in the (geographically) outer estuary.

5) Section 2.2: In the absence of measurements, the calculation of river discharge probably contains substantial uncertainty, but I find the authors' approach very clever. The same applies to section 2.6, where a "flower pot" was modified to serve as a gas exchange chamber. It is nice to see that ingenuity and that the authors are honest about the materials they used in the field.
Discharge measurements would have certainly reduced the uncertainty of our estimates, but unfortunately, they were not available. We thank the referee for his sympathy with logistical constraints.

6) p. 8305, line 5: How was the pH calibrated? With NBS/NIST buffers or borate buffers? What activity scale is pH reported on? Total-scale? NBS-scale? Please clarify this as the scale can make a difference of 0.2 units in seawater.
We used NIST buffers and report pH values on NBS scale. We will add this information in the revised text.

7) p.8305, line 18: How long were nutrient/DOC samples stored for?
Both DOC and nutrient samples were stored up to two months until analysis. They were preserved as indicated in the text and kept frozen until analysis. We will indicate the duration of storage in the revised text.

8) p.8306, line 7: What was the volume of the vials used for delta-13-C-DIC? What reference are the results reported against? PDB?
We used 10 ml vials for the determination of d13C-DIC. The results are reported against PDB. We will add this information in the revised text.

9) p. 8306. line 14: How was the equilibrated air "dried" before FTIR?
We used a Nafion® drier and a magnesium perchlorate moisture trap, as described in Griffith et al. 2012. We will point this out with reference to Griffith et al. in the revised text.

10) p.8306, lines 23: Where did the gas standards come from? What were they reevaluated against after the cruises? Please give suppliers of secondary and primary standards.
The reference gases are gravimetrically prepared gas mixtures (Deuste Steininger). They were re-calibrated against WMO reference scale (for the available range of concentrations) at the Max-Planck-Institute for Biogeochemistry in Jena, Germany. This information will be added in the revised manuscript. The "re-evaluation" seems to be misleading. We referred to the fact that we did measure all reference gases up to 10000 ppm CO2, but then we saw that maximum CO2 mixing ratios were only up to 5000 ppm. Therefore, we used only those data points for calibration that covered the range of measured values. We will rephrase this in the revised manuscript.

11) p.8314, lines 12-14: The authors should clarify that these flux rates for CO/CO2 refer to their flux-chamber data, not the calculated flux from Wanninkhof, 1992 (W92). Perhaps the use of a subscript throughout the manuscript would help differentiate these, e.g. FFC from FW92
This seems to be a good way to clarify which flux we refer to. This will be done in the revised manuscript.

12) p. 8316, lines 2-3: The authors state that they did not "expect" the low DOC contribution from peatlands, given that these occupy such a large fraction of the catchments.

The authors should elaborate on this point. I am less "surprised". My understanding from temperate peatlands is that runoff in these is limited to the uppermost layer of peat and only occasionally (if not rarely) flushes the deeper layers, so their DOC contribution/age/composition does not reflect the amount of C stored in peat. Perhaps the authors could explain why they "expected" a higher contribution.
Tropical peat swamp forests have a dense vegetation. As we suggested in another publication (Müller et al. 2015), most DOC is actually derived from the upper peat layers or surface runoff. Although the DOC concentrations do indeed not reflect the peat pore water concentrations, they are still quite high. We determined an average DOC concentration of 3690 μmol/L in a peat-draining river on the Maludam peninsula, between the Lupar and Saribas rivers. This is more than ten times higher than the DOC concentrations in the Lupar and Saribas rivers (upland region). Although this particular river flows directly into the South China Sea, many other peat-draining rivers from the Maludam peninsula and from other peatlands in the Lupar and Saribas catchments flow into the Lupar and Saribas estuaries. Therefore, we expected to see a signal of elevated DOC in these estuaries. We will make this clearer in the revised manuscript and extend the discussion at this point.

13) Section 4.2: The discussion of DOM photochemistry seems thorough, even though the conclusion is that the results are "inconclusive". Undoubtedly, diurnal CO variability points to a photochemical source, but the authors are correct to point out that this is probably limited by high light attenuation over the whole water column.
We agree that the diurnal variability of CO clearly indicates a photochemical source. We were unable to quantify in how far the bioavailability of DOC was influenced by photochemistry though, which is why we think this question would merit further investigation.

14) p. 8320, lines 1-4: The conclusion that W92 is not appropriate for calculating gas exchange in estuaries is not new, but this is frequently neglected elsewhere. In fact this applies to all wind-driven turbulent diffusivity models and the authors are correct to

point this out, but this conclusion should feature more prominently in the abstract. e.g. Wind-driven turbulent diffusivity models (such as W92) are inappropriate in estuaries where turbulence is mainly induced by tidal-flows and river discharge as well as their interactions with channel-morphology rather than wind-shear.
We will mention this finding in the abstract and the conclusions of the revised manuscript, as suggested.

15) p. 8321, lines 5-9: The authors state that pH in the estuary may drive the carbonate system to high CO2, as observed. Though this is true, one could also argue the opposite, i.e. that high CO2 reduces pH. I'm not sure that "causality" can be established here. On the one hand, most estuaries are heterotrophic environments where respiration produces CO2 which would be expected to decrease pH. Nevertheless, the change in ionic strength at the freshwater-seawater interface may also cause a respiration-independent decrease in pH which could increase CO2.
Our thought was that the input of acidic waters from peat-draining rivers might decrease pH. On the other hand, certainly, respiratory CO2 might have an effect on pH as well. We agree that it is difficult to determine the direction of the causality with the available data, but we will try to provide a more balanced and extended discussion at this point.

16) Conclusions: This section is good, but the authors should highlight the inadequacy of wind-driven turbulent diffusivity models for calculating fluxes in estuaries.
This will be done as mentioned in our reply to comment 14.

References

Griffith et al. 2012: A Fourier transform infrared trace gas and isotope analyser for atmospheric applications. Atmospheric Measurement Techniques 5, 2481–2498. doi:10.5194/amt-5-2481-2012

Moore et al. (2011): Fluvial organic carbon losses from a Bornean blackwater river. Biogeosciences, 8, 901–909. doi:10.5194/bg-8-901-2011

Moore et al. (2013): Deep instability of deforested tropical peatlands revealed by fluvial organic carbon fluxes. Nature 493, 660-664. doi: 10.1038/nature11818

Müller et al. (2015): Lateral carbon fluxes and CO2 outgassing from a tropical peat-draining river. Biogeosciences Discussions 12, 10389-10424, 2015. doi:10.5194/bgd-12-10389-2015

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C5220/2015/bgd-12-C5220-2015-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 12, 8299, 2015.