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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.

Anonymous Referee #1

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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 at-

mosphere. 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.

2) p.8303, line 24: What is the tidal range for the two "macrotidal" estuaries?

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.

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?

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.

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.

7) p.8305, line 18: How long were nutrient/DOC samples stored for?

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?

9) p. 8306. line 14: How was the equilibrated air “dried” before FTIR?

10) p.8306, lines 23: Where did the gas standards come from? What were they re-evaluated against after the cruises? Please give suppliers of secondary and primary standards.

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

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.

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.

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.

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.

16) Conclusions: This section is good, but the authors should highlight the inadequacy of wind-driven turbulent diffusivity models for calculating fluxes in estuaries.

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