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Biogeosciences Discussions

Interactive comment on "Fluvial organic carbon losses from a Bornean blackwater river" *by* S. Moore et al.

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General:

The authors did a very nice piece of work by reporting DOC and POC concentrations and calculating fluxes from a tropical blackwater river, data which are still rarely found in the international literature. Based on the available few results carbon export from these rivers to the ocean is probably quantitatively significant. Therefore, the data basis needs to be expanded. In this respect the manuscript by Moore et al. makes a valuable contribution. In general, it reports seasonal variations in DOC and POC along the course of a blackwater river on the Indonesian island of Kalimantan and it calculates fluxes into the ocean and extrapolates it over the entire Indonesian peatlands. The authors depict some similarities with a blackwater river on the neighbouring island of

Sumatra (Siak River, Baum et al., 2007) and come up with a quite similar estimate of DOC export. So far so good. As such it makes a nice piece of work. However, in the present state the manuscript is missing an opportunity to make a strong impact. It could shed more light on the processes responsible for input, transport and transformation of DOC particularly when related to land use/cover (some information is given in the introduction, but never referred to in the discussion). Moreover, it fails to put this newly gained information on a blackwater river into the global context of tropical blackwater rivers. There is more information available than just the Baum et al. paper repeatedly referred to. In its present state the manuscript does not go beyond contributing a new "number".

Although the manuscript has separate "results" and "discussion" sections, a mixture of both is presented in both sections. This needs to be changed. Either the authors separate more clearly or they provide a "results and discussion" section. Although they claim POC to be almost irrelevant for the Sebangau carbon budget they spend quite some effort on explaining sources and fate of POC. This, however, is mainly based on general considerations and not on facts from the set of measured data and background information on the river catchment. Therefore, I think, that this kind of "artificial" discussion can be reduced substantially. Instead, the authors should spend more effort on the DOC, first with regard to the local context, i.e. mentioned available information on land use/cover in the catchment, and second in the global context of tropical blackwater rivers. The authors are repeatedly comparing their results with those of temperate (partly peat-draining) watersheds, but fail to do this with tropical rivers except for the Siak River.

Overall, this paper will make a valuable contribution to the international literature. I recommend it for publication after it has undergone moderate/major revision.

Detail comments:

Title:

The title is informative, but I would not use the term "Borneo". The Indonesians call the island Kalimantan.

Abstract:

The abstract reflects the contents of the present version of the manuscript including the abovementioned shortcomings. In the first sentence you can simply delete the passage "plays an important role in the carbon cycle because it".

Introduction:

p. 8320, l. 18-20: The first sentence is a literal repetition of the first sentence in the abstract. Please rephrase.

p. 8320, l. 23-24: What is the passage in brackets good for? Looks like a redundant repetition.

p. 8321, last paragraph: There are some inconsistencies. First, you mention that "in most wetland ecosystems nearly 100 % of TOC is exported as DOC". This requires a close look at the term "wetland". If you include mangroves as wetlands, what is usually done, the previous statement is not true. You should either refer to peat swamps instead of wetlands or include the DOC and POC fluxes from mangroves in your discussion. Appropriate references for this are: Bouillon et al., 2008, Global Biogeochem. Cy. 22; Dittmar et al., 2006, Global Biogeochem. Cy. 20; Jennerjahn and Ittekkot, 2002, Naturwissenschaften 89: 23-30. Then you mention global DOC fluxes which are much lower than the "commonly accepted estimates" you mentioned in l. 6-8. This sounds a bit contradictory. Please modify it.

p. 8322, l. 14: How can you quantify organic carbon "dynamics"? I suggest to replace it by export or flux.

Methods:

p. 8322, 1st paragraph: You mention average temperature and rainfall citing Page et

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al., 2004, and then come up with a 30-year record of these data taken from another source (Hooijer et al, 2008). This is a bit confusing. The Hooijer et al. paper appears to be the appropriate source. Moreover, it would be good to have a graph showing the monthly averages over this 30-year period, particularly as you later use these data to define the seasons for the calculation of fluxes.

p. 8323: Here you provide a lot of interesting information on the land use/cover of the catchment and changes that have occurred over the years. All this information should be included in the map (Fig. 1). The map as is contains only little information. Including the mentioned information would make it much more informative and interesting for the reader and it would it would provide a good (and necessary) reference for the discussion of observed spatial variations of DOC and POC along the course of the river.

p. 8324, last paragraph: The information given here is redundant, because it simply repeats the names of tributaries shown in the map.

p. 8324, l. 3-6: I wouldn't call the tidal range "small", it is in the mesotidal range and therefore not really small. Moreover, I am wondering about the source of the tide data, "The United Kingdom Hydrographic Office". Do they collect tide data from Indonesia? It is not mentioned in the reference list.

p. 8325, l. 1: Drying at 80°C will most probably destroy part of the particulate organic matter and therefore lead to losses. Usually drying is done at 40°C to avoid POM destruction.

Results:

In general, results that refer to measurements in the past should be reported in past tense. Reporting them in present tense implies that the numbers are still the same as at the time of measurement.

p. 8325, l. 13: here you mention figure 3 for the first time without having mentioned

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figure 2 beforehand. Change numbering or cite figure 2 earlier.

p. 8326, l. 2-5: In fact the figure does not show such a trend along the course of the river, but just a relation between EC and DOC. The conclusion you draw sounds plausible, but the argueing needs to be modified a little.

p. 8326, l. 8-24: The first sentence is plausible, but the rest of the reasoning in this paragraph is very unconvincing. I simply do not understand how the authors calculate here and find the result rather speculative.

p. 8326, l. 27-28: You mention "a decrease in (POC) concentration from source to mouth", but ignore a big depression in the curve between km 50 and 100 (see figure 4). This requires a bit more explanation.

p. 8327, 2nd paragraph: The "large within-river variability" of POC is only discussed in terms of variations in the inputs from the various tributaries. What about primary productivity and organic matter decomposition in the river? No effect at all?

p. 8327, l. 16-18: Where are the discharge rates? We haven't seen them yet. They are not included in the table.

p. 8327, l. 20 ff: The dry vs wet season discussion requires a bit more detail. We need to know what you are talking about when you mention "dry" and "wet" seasons. What are the time spans you are talking about? Which months are the "dry season" and which months form the "wet season" and what are the differences in precipitation? Moreover, you may also have seasonal differences in litter fall in the tropics, for example, in mangroves.

p. 8327, l. 27-29: You mention average concentrations which differ from those given on page 8325. Please clarify.

p. 8328, l. 1-9: I cannot follow this line of reasoning. What is the use of it? You make general statements about storm events in temperate rivers which has nothing to do with the story of your manuscript. Moreover, the generalization on POC control in

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temperate rivers is based on very old data and wrong. It wouldn't mean a loss to the paper to simply delete the whole passage.

p. 8328, l. 9-13: I agree with the first statement, but cannot follow the last sentence. Why should "aerobic decomposition lead to increased amounts of POC being released"? I suppose that drying could support denudation of upper peat layers, but that wouldn't require decomposition of organic matter.

p. 8328, l. 15-28: For the calculation of the carbon fluxes it would be good to have the data basis presented. That means to define the seasons more clearly including monthly average precipitation data (in a figure as I mentioned earlier) and also the discharge measurements.

p. 8328, l. 18-20: Here you report data in less than thousands of Tg. That doesn't look very good.

p. 8329, l. 1-3: Repetition.

p. 8329, l. 6-12: This calculation seems a bit arbitrary at least with regard to the calculation basis presented here. As mentioned earlier the calculations require a more robust basis that is also presented in the manuscript. In the present version the conclusion drawn in the last sentence is rather a speculation.

Discussion:

What is completely missing in the discussion is the potential differece in sources related to differences in land use/cover. The authors provided a lot of details on land use/cover in the introduction, but do not use it at all. This is a pity! It would be extremely interesting to see if there are any variations in the DOC and POC that can be related to differences in land use/cover. This type of information is hardly available, but would be interesting also with respect to implications for land management in the context of carbon seqestration/loss.

The introductory passage of the discussion is partly redundant. The last two sentences

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could be deleted (l. 16-19).

I am wondering that the subchapter headlines are the same as in the results section and simply mention measured parameters and not the scientific issues addressed in this paper.

p. 8329, l. 25 ff: So, what is it: alternative or additional? Here you mention an important process: the microbial decomposition of organic matter! But in the following you mention "the most likely DOC removal mechanism is via flocculation to become POC or adsorption to existing POC". As yet I cannot see that there is any indication for that and the only arguments you mention in the text are general ones from other studies. You should have a look at the variations of EC and POC along the course of the river. It may support your assertion. Moreover, you have little POC and mineral matter. However, I think, that organic matter decomposition plays a more important role. If you had dissolved oxygen data you could examine it? Baum et al.'s is not the only study of an Indonesian blackwater river. We studied the nearby very small Dumai River and found indications for decomposition of DOC in the estuary (Alkhatib et al., 2007, Limnol. Oceanogr. 52: 2410-2417). In the present state the line of reasoning in this paragraph is a bit contradictory and needs to be modified putting more emphasis on organic matter decomposition as mentioned in the beginning sentence.

Subchapter 4.2 POC: The whole discussion on POC has little substantial information, but is based mainly on commonplaces and generalizations from other studies. As POC contributes only little to the carbon budget of the river the whole discussion on POC can be shortened.

p. 8331+8332, "flow rate" discussion: Again, for the "flow rate" discussion it would be good to have a more robust data base, i.e. the discharge data you collected.

p. 8332, l. 13: Why do you think that the Sebangau is "a major contributor of organic carbon to the ocean"? Without seeing data on carbon loads, I would think that the Sebangau is nothing compared to the Amazon. I agree that the Indonesian blackwater rivers have the highest carbon yields (i.e. per unit area!), but in terms of total load the Sebangau is probably not comparable to major world rivers like the Amazon. And what about other blackwater rivers? There is much more information available than mentioned in this study. Before you upscale to global budgets it would also be interesting to see a comparison of yields and loads among blackwater rivers and/or comparison to peat-draining rivers from other climate zones.

p. 8332, l. 20-28: The POC discussion here is largely a repetition of the previous.

p. 8333, l. 1-11: Here you come up with your final calculations of carbon fluxes from the River Sebangau and extrapüolate for whole Indonesia. However, I have some doubts on the figures given. On page 8329 you mention that "the River Sebangau discharges approximately 50 % more DOC to the ocean per annum than the Siak River". However, in your extrapolation for the whole Indonesia carbon export from peatlands you come up with a number that is 3 Tg smaller than the Baum et al. calculation although you included POC flux and used the same land and peat area as basis for calculation. What is the reason for this mismatch? Is there a calculation mistake or did I miss something? Apart from this mismatch in numbers which needs to be checked I agree with the authors' conclusion.

References:

The list of references is up-to-date except that it is lacking some papers on blackwater rivers. You will find some in the Alkhatib et al. paper.

Table:

The font size is much too small. Maybe just a matter of layout, but the font size has to be increased.

Figures:

The font and symbol size is much too small in all figures. A figure comparing concentrations and/or yields and/or loads of blackwater/peat-draining rivers on a global scale

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would be nice, for example.

Figure 1: It is good to have a map, but in the present version it contains only little information. Please include all the information on land use/cover, locations and sample locations given in the text. It will make it much easier for the reader to follow the reasoning.

Figure 2: Is it really necessary to have this figure? It does not present too much information in visualized form that we did not get in the text.

Figures 3 and 4: The curves presented in these figures show some excursions which are hardly discussed in the text. Why not? They are possibly related to some catchment features.

Figure 5: This figure is not cited in the text and does not contain any additional information when compared to figures 3 and 4. It can be deleted.

Altogether this is a very nice study of a blackwater river that you seldomly find in the international literature. The full story is not yet there, but I am sure the authors can add the missing ingredients. I am looking forward to read the final version of this paper.

Interactive comment on Biogeosciences Discuss., 7, 8319, 2010.