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

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The manuscript titled "Fluvial organic carbon losses from a Bornean blackwater river" by S. Moore, V. Gauci, C.D. Evans, and S.E. Page provides original data on dissolved and particulate organic carbon obtained from two sampling campaigns (dry season and wet season) in a tropical blackwater river system (River Sebangau in Indonesia). Based on the present data, Moore et al. extrapolated the specific annual POC and DOC exports obtained for the Sebangau River over the peat covered area of Indonesia in order to confirm the first estimate performed by Baum et al. (2007) from another watershed. This paper is highly welcomed, because as stated in the introduction, Indonesian rivers are considered to be large contributors of DOC and it might be a reference for other studies as such data are still very scarce at the global scale.

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The manuscript is well-written with up-to-date references and reasonably well-organized as a whole, but several paragraphs need significant reorganization to reduce/avoid redundant statements (maybe due to the manuscript organisation with “Results” separate from “Discussion”) that interrupt the logical flow of the text.

Data interpretation in “Discussion” should be clarified and completed because it is not always convincing (speculative interpretation; e.g. biogeochemical and/or physical processes explaining the spatial and temporal variations in POC and DOC concentrations). For example (p.8327; l.6-9) “the large within-river variability seen across both seasons can be attributed to the influence of the fourteen channels”. With data on water discharge and concentration obtained in channels and in the Sebangau River, Moore et al. can demonstrate this assumption (mixing calculation). I really do not believe that one may examine POC and DOC behaviour in two profiles without physico-chemical and water discharge (or velocity) data, and particle characteristics (e.g. grain-size).

In my opinion, the authors could strengthen this output by focusing more on the spatial DOC evolution with regard to DOC comprises between 88% and 94% of TOC.

The sampling strategy (longitudinal profiles; ~50 sites) is well-adapted to a major objective of the study: quantify organic carbon dynamics from the source to the mouth. However, I am wondering if this strategy performed on a single watershed on two separate occasions may produce sufficient and reliable results for adequately quantifying annual POC and DOC flux considering the sampling frequency limitation and the representativeness of this watershed. This approach maybe provides a good approximation considering the low temporal DOC variations observed if both campaigns are really representative of contrasting hydrological conditions; the authors should set their sampling period on a hydrograph in order to validate this observation; they should be further justified “DOC concentrations should be relatively constant throughout the year” (P. 8327, L.24-27). I also extrapolated the specific POC flux exported by a single fluvial system (the Nivelle River) to the whole mountainous area of the Southern coast of the Bay of Biscay (Coyne et al., 2005; Biogeochemistry) in order to demonstrate small

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mountainous river accounted for 70% of the total POC inputs in the Bay. So, I am in no position to say that this method is not correct. However, the authors should give more information on others watersheds (similar land use and sediment yield?)

The data quality seems to be sound, despite the few commonly used methodology (Nalgene bottles, cellulose acetate membrane filters, ...). As typically done in previous studies on organic carbon, samples should be collected in glass-bottles and filtered through preheated and pre-weighted 0.45 μ m Whatman GF/F fibreglass in order to avoid/limit organic contamination. In this study, contamination can be considered negligible in relation to the high DOC concentrations (> 30 mg/l for almost all sites) measured in this system. However, the analytical quality control is completely missing. Precision and accuracy for analytical method applied for DOC and POC determination should be given. More information on the water discharge reliability is also required. How was water discharge measured? What is the likely reliability of the flow data? It may only be + or - 20%. All these issues will influence the reliability of the final load estimates and thus the reliability of final conclusions.

Accordingly, I cannot recommend it for publication in its present form; I strongly feel that this manuscript needs major revision.

I then list other minor specific issues in order of their appearance in the manuscript. P.8321: "According to various modelling estimates (Ludwig et al., 1996; Harrison et al., 2005) [...], the global river-to-ocean DOC flux [...] to be around 170-250 TgC yr⁻¹". If your mentioned these only two commonly accepted estimates, the global DOC flux ranges from 170 (Harrison et al. 2005) to 210 TgC yr⁻¹ (Ludwig et al., 1996). Figure 1: It is difficult to understand Fig 1; what is exactly the Sebangau watershed boundary? P. 8325: Figure 3 appears in the text before Figure 2. Figures 3, 4, and 5: Poor printout quality of the figures. I can not distinguish the DOC and POC results achieved for channels. The interpretation of results is not sufficiently supported by the figures. For example, P. 8326 (l.11-19). "Percent estuarine DOC removal at high tide (dry season) was estimated by extrapolating linear regressions [...] Linear regression lines were

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derived using the last three samples". The authors should add linear regression lines. P.8326, l. 26-28: "Figure 4 shows [...] POC follows a similar trend in both the dry and wet seasons". I do not see that in the Figure.

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