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Discussions

Interactive comment on "Spatial and seasonal contrasts of sedimentary organic matter in floodplain lakes of the central Amazon basin" *by* R. L. Sobrinho et al.

Anonymous Referee #1

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Journal: BGâĂÍTitle: Spatial and seasonal contrasts of sedimentary organic matter in floodplain lakes of the central Amazon basin Author(s): R. L. Sobrinho et al.âĂÍ MS No.: bg-2015-208âĂÍ MS Type: Research Article

General Comments:

This study examines a suite of organic biomarkers and bulk chemistry in the surface sediments of the five major floodplain lakes in the central Amazon River during four seasonally distributed expeditions. The primary goal was to determine the relative contribution of upland (e.g. Andean) soils, flooded/non-flooded forests, macrophytes, and phytoplankton to floodplain sediments.

The authors conclude that the majority of floodplain sedimentary organic matter (SOM) is derived from flooded forests and aquatic macrophytes with minimal contributions from all other sources. The most convincing data are the C:V values observed for lignin phenols. The estimation that 20-30% of SOM is derived from macrophytes based on a simple mixing model is reasonable and based on established knowledge of endmember compositions. However, this is the only truly quantitative conclusion that can be made from this dataset as presented.

The other organic parameters measured are not adequate for quantifying the relative contribution of the desired OM sources beyond vague inference. For example, the authors somehow conclude that flooded forest vegetation is the primary source of SOM without any actual quantification of this source presented. The composition of lignin phenols cannot be used to differentiate between flooded versus non flooded forest vegetation/soil sources (or suspended POM for that matter) in this case.

Similarly, the authors estimate the contribution of phytoplankton based on the abundance of crenarchaeol. However, as the authors note, this compound is not produced solely by phytoplankton. Crenarchaeol has been found in nearly every type of environment (e.g. soils, sediments, rivers, lakes, and oceans), making any type of quantitative differentiation between endmembers dubious at best. Illustrating this point, the authors inconsistently state what crenarchaeol was used as a proxy for. For example, the abstract states it was used to identify river suspended POM, the introduction states that it was used to determine soil sources, and the results/discussion state that it was used to "indirectly" quantify aquatic production.

The other main conclusion made is that floodplain hydrodynamics seem to be the most important factor controlling SOM composition. Although this is probably true, the authors provide no discussion or data related to floodplain hydrodynamics. The only hydrologic data presented is discharge at Óbidos, which gives very little insight into the complex floodplain dynamics or possible drainages from the surrounding (non-flooded) landscape. A detailed modeling exercise would be required to adequately represent the complicated floodplain hydrodynamics and watershed inputs. Insights from the literature were not presented in this regard. Further, the collection of sediments at only 2-3 locations per floodplain lake does not provide a robust assessment of these environments, which the co-authors have reported as highly spatially heterogeneous in previous publications.

Overall the manuscript provides data for a collection of organic parameters that may be useful for other researchers in the region. Aside from the estimation of macrophyte contributions to SOM, very little quantitative conclusions are made, which greatly limits the potential impact of this work. The authors state many conclusions that appear to be inferred hypotheses at this point. The manuscript could be improved by describing the ambiguity of the measured parameters in greater detail and moderating/removing conclusions that are not quantitatively grounded.

Specific Comments:

P4, L7: "…the organic matter (OM) produced in the floodplain lakes fuels the outgassing CO2 in the river system (Abril et al., 2014)."

There are several issues with this statement. First, the cited reference suggests that direct inputs of CO2 from (flooded) plant respiration is a significant source of CO2 to the system, not just the breakdown of OM derived from floodplain plants. Second, the cited reference suggests that the above floodplain CO2 sources are the primary source for CO2 in the central Amazon, not the entire Amazon system. Finally, the cited reference describes floodplains as a source for labile OM, such as lignin macromolecules that have been shown to be quite reactive, but fail to mention that the terrestrial (non-flooded) environment is also a large source of these types of molecules. "Terrestrial" carbon was only attributed to radiocarbon-depleted headland sources rather than vascular plants around the drainage basin. This statement should be moderated. For example, consider something along the lines of: "Further, inputs of CO2 from plant respiration and reactive OM produced in floodplain lakes is a significant source of CO2

outgassed in the Central Amazon River."

P4, L22: "…the contribution of the multiple sources of OM (up- land soils, flooded forest, aquatic macrophytes, and phytoplankton) remain uncertain"

P4, L17 describes that Suspended POM is primarily derived from forests and upstream soils. Why are forests (non-flooded) not mentioned as a potential source for sedimentary OM in floodplains?

P5, L15: "Lignin is a recalcitrant organic macromolecule…"

This statement is in conflict with the statement made at P5, L20: "but also a relevant source for the outgassing of CO2 in the Amazon River (Ward et al., 2013)." The cited reference showed that lignin can be very reactive in certain environments such as the Amazon River mainstem near the mouth and more studies finding high rates of lignin turnover in other settings are emerging. The authors should consider the evolving philosophy on "recalcitrance/lability" vs. "reactivity" (Schmidt et al., 2011 Nature). Organic compounds are not intrinsically "labile" or "reactive" based only on chemical structure, but, rather, depend on the culmination of ecosystem properties.

P5, L21: BrGDGTs and crenarcheol have been found to be not exclusively of soil origin in different environments around the world. Other potential sources should be described here as was done on P16, L10. The authors note in the discussion that these are not useful indicators for soil OM.

P6, L4: "…provides new insights into the link between the hydrology of the Amazon basin to the sources of SOM in floodplain lakes."

It is not clear what linkages to hydrology were made here in this study. The only hydrologic data provided or discussed was discharge at Obidos during the study period with no discussion of the complex hydrology/hydrodynamics of floodplains. This statement is also made in the abstract (P3, L19) and in the conclusions.

P7, L21: Previous studies in these floodplain lakes describe immense spatial variability in biogeochemical characteristics. Do the authors feel that 2 to 3 sediment samples is a robust representation of these systems? Also it is not clear in the text where sampling stations were distributed.

P8, L19: In order to assess contribution of inorganic nitrogen ($NH_4^+ + NO_2^- + NO_3^-$) to TN, TN (wt. %) and TOC (wt. %) were correlated ($R^2 = 0.89$; $p < 0.001$; $n = 57$).“

This is a confusing way to calculate inorganic nitrogen...please clarify. Also, does the calculated C:N ratio represent TOC to TON or TOC (i.e. TC in this study) to TN?

P9, L1: “Approximately 500 mg of freeze-dried sediments and macrophytes were analyzed for lignin monomers using the alkaline CuO oxidation method”

This method is not for analyzing lignin monomers. The purpose of the CuO oxidation is to break apart macromolecules into monomers that can be analyzed, and, thus, represents the combination of macromolecules and monomers. Free lignin monomers typically make up less than 1% of the total lignin content.

P9, L12: What type of detector was used on the gas chromatograph (e.g. GC-FID)?

P9, L13: Please clarify whether the recovery standard was added before CuO oxidation/extraction or before analysis on the GC.

P12, L6: “The C : N ratio did not reveal significant spatial and seasonal variations (Figs. 3b and 4b)”

Could this possibly be related to the fact that a “correlation” with TOC was used to calculate inorganic (and subsequently organic) N concentrations? Do the calculated C:N values represent real C:N values, or simply C:C(multiplied by some factor)?

P12, L11: These are large ranges. It would be interesting to know the spatial distribution (e.g. where was -19 per mil and where was -29 per mil).

P15, L17: “The averages of important lignin parameters ($\lambda 8$, S : V ratio) but also the C : N ratio of the wood samples are significantly different from those for the sediments,

which clearly indicates only a minor contribution of woody material to the SOM.”

The authors should note that source signatures for lignin phenols are obscured by processes such as leaching, sorption, and biodegradation (e.g. Hernes et al. 2008, GRL and others). Vascular plant-derived OM will not have the same signature as a plant endmember after it has been mobilized into streams and altered by biological processes.

P16, L20: “Crenarchaeol is, therefore, considered as an (indirect) indicator of aquatic primary production. The enhanced concentrations of crenarchaeol in SOM thus indicate a contribution from this source.”

This relationship seems dubious, especially considering that in the introduction it was stated that crenarchaeol was used as a soil OM indicator and in the abstract it was stated that crenarchaeol was used as a suspended POM indicator.

P17, L20: “Consequently, the remaining 40–60 % of the SOM might be derived from other sources of OM such as the flooded forests (Eq. 3)”

There is no quantitative basis for this statement. You could just as easily say 40-60% might be derived from terra firme, headland, and/or SPOM sources. This sounds like a “guess.”

P17, Line 24: “Thus, the seasonal and spatial contrasts in the SOM should be investigated in order to better understand the connectivity between these compartments.”

Some insight from the authors on what else could be done would be appreciated. This study claims to address this and “provide new insights”, but not many revealing trends were observed aside from the contribution of macrophytes. How can we improve on this?

P18, L8: “Consequently, the bulk parameters apparently mix and homogenize the long time scale (year), while the biomarkers are more sensible to changes in short time scale (months) at the sediment surface.”

How was this determined? All parameters were only measured 4 times over a 2 year period, not monthly.

P20, L15: "Since the concentration of crenarchaeol (a marker for aquatic production)..."

The introduction states that crenarchaeol was/is typically used as a marker for soil OM. The abstract states crenarchaeol was used as an indicator for SPOM. Previously the authors mention that crenarchaeol can be found in nearly any type of environment.

P20, L16: "...we conclude that such increase in the concentration of the lignin phenols in the RW and FW seasons and the brGDGTs in the FW season is not derived from the water column, riverine SPOM or in situ production but from the soil and leaf runoff."

How was this determined quantitatively, or is it just assumed/hypothesized?

P20, L25: "Thus, based on the hydrodynamics of floodplain lakes and the concentration of the biomarkers applied in this study, in the RW and FW seasons, these organic molecules are mainly derived from the drainage of local wetlands soils. "

This study did not include any assessment of "hydrodynamics." How was this conclusion reached?

P21, L2: "However, even in lake Curuai, where the primary production and the riverine SPOM is admittedly an important source of SOM (Moreira-Turcq et al., 2004; Zocatelli et al., 2013), the interface between the floodplain lake and the flooded soil drives the sedimentation of the organic compounds."

How was this conclusion quantitatively determined?

P21, L6: "The vegetation coverage of the wetlands (flooded forests) are the most important source of SOM in floodplain lakes of the central Amazon basin. The macrophyte community in the floodplain lakes is also an important source of SOM whereas the river SPOM contributes to a minor fraction of it."

It is not clear how this conclusion was reached. The authors provide no quantitative index for flooded forests. Further any such index would be obscured by the contribution of terra firme forests. No modeling or spatial analysis was used to determine potential inputs from flooded forests vs. terra firme forests. The only endmember model that the authors provide is for macrphytes and phytoplankton and it is assumed that the remaining 40-60% of SOM is derived from flooded forest with no regard for SPOM or terra firme forests.

P21, L15: "The sedimentation of OC in the floodplain lakes are linked to the periodical floods."

Hydrology/hydrodynamics are not discussed beyond discharge at Óbidos. This study makes minimal, if any, connections between OM sources and hydrology as stated. Further, most results were reported to not vary "significantly" over space and time.

P21, L19: "Hence, together with wetland vegetation, the hydrodynamics of the floodplain seems to be the most important controlling factor on the composition of SOM in the floodplain lakes of the central Amazon basin.

See above comment.

Technical Corrections:

P4, L7: Capitalize "and"

P6, L11: "rivers" or "River" should be added after the river names (e.g. "Tapajós River")

P6, L21: Capitalize "River"

P7, L18: It is unclear what the "CBM" code is referencing. Why not just refer to cruises as HW, LW, etc as was done in Figure 1 for better clarity?

P8, L17: Delete "and"

P9, L18: Change "chromatography" to "chromatograph"

P10, L25: What brand/model HPLC-APCI-MS was used?

P10, L9: Change “chromatography” to “chromatograph”

P10, L18: It doesn’t seem as though any statistical information has been reported in the results other than the number of samples. Perhaps p values should be reported.

P11, L4: Change “value” to “values”

P12, L19: It should be noted that Lambda 8 is the “amount of lignin” normalized to OC.

P12, L20 (and more): The term “significant” was used 8 times in page 12, 6 times on page 13, 2 times on page 14, and 1 time on pages 15 and 16 with no statistical information given. Perhaps p values should be reported as was alluded to in the brief methods section (3.6). The use of “significant” is quite redundant.

P17, L7: Add a space to “samplescan” P17, Line 22: “Periodical” should be replaced with “periodic” here and elsewhere.

P19, L14: “Accordingly, the percentage of C4 plants in the upstream lake is only 3 %, but for the downstream lake 22 %. “

This should say “the contribution of C4 plants to SOM.” It sounds as if the authors are stating that C4 plants only make up 3% of plant biomass in the region, when they are actually referring to the amount of C4-derived SOM.

P20, L15: Change “manly” to “mainly”

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