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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.

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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

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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.

Reply: First of all, we would like to thank the referee #1 for the constructive comments. We believe that, after the corrections proposed by the referee #1, the quality of our manuscript will be greatly improved. More detailed rebuttals are provided below. The principal objective of the present work is to quantify the fractions of the principal sources of sedimentary organic matter (SOM) in floodplain lakes of the central Amazon basin. It is expected that the sedimentation of OM in this ecosystem is linked to the periodic floods (e.g. Junk, 2010). In order to observe any possible changes in the spatiality and seasonality related to such link, we collected 57 surface sediment samples during four periods of the flood cycle (hydrological seasons) in the five major floodplain lakes (Figure 1). This sampling set provided approximately 12 samples per lake and 15 samples per season, collected in distinct sites of each lake. The number of samples was enough to make statistic comparisons (ANOVA) with the data found in the literature and to observe significant ($p<0.05$) changes in the spatiality (upstream-downstream) and seasonality. Differences among sites in each lake might be an important factor. To understand such differences, more detailed studies in each lake should be performed in future. For that purpose, a higher number of samples, including samples from flooded forest soils and non-floodable soils, are necessary. However, this is beyond the major purpose of this study. Here, we rather focused on the general role of the periodic floods on the SOM, by applying a mixing model. The mathematical approach applied to quantify the sources of SOM was the three end-member modeling. Due to the complexity of the ecosystem and the behavior of each parameter in the environment, only the C:V ratio among the lignin parameters could be applied as an specific source (i.e.

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macrophytes). However, as observed in Figure 5, the riverine suspended particulate organic matter (SPOM) is a potential source of crenarchaeol to the SOM, since the soils have significantly lower amount of this compoundon. Unfortunately the data of crenarchaeol in the water column of the floodplain lakes are not available to this study. Consequently, it is impossible to disentangle the major crenarchaeol source between the riverine SPOM and the lacustrine SPOM based on the data available. Therefore, we will consistently use a more general term “the aquatic source” for crenarchaeol. It is unexpected that the primary production performed by the phytoplankton in the floodplain lakes is an expressive source of SOM, if compared to the others sources investigated in this work (e.g. Moreira-Turcq et al., 2013). Consequently, we will apply the crenarchaeol to quantify the fraction of aquatic OM or, in other words, as an indicator of the SOM derived from the aquatic environment (both river and lakes) in the revised version. Furthermore, we will make clear that we do not use crenarchaeol to estimate the contribution of phytoplankton since this compound is produced by Thaumarchaeota. On the other hand, it should also be noted that the abundance of crenarchaeol alone is not used to trace the soil input but the ratio of crenarchaeol and brGDGTs. The rise and recede of waters in the floodplain lakes follow the river main channel, since they are connected. Thus, we believe that the data from Obidos are enough to illustrate the flood cycles and the periods when the sediments were sampled. Finally, we will provide information on the measured parameters in greater detail and will also moderate conclusions in the revised version as requested by the referee#1.

Specific Comments:

- Comment #1 (P4, L7): “...the organic matter (OM) produced in the floodplain lakes fuels the out- gassing 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 vas- cular 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.” Reply: We agree with the Referee #1 for this point. We will add the sentence suggested by the Referee #1 as follows: “Further, inputs of CO2 from plant respiration and reactive OM produced in floodplain lakes are significant sources of CO2 outgassed in the Central Amazon River.”

- Comment #2 (P4, L22): “...the contribution of the multiple sources of OM (up- land soils, flooded forest, aquatic macrophytes, and phytoplankton) remain uncertain”

Reply: We will change it as recommended by the referee as follows: “...the contribution of the multiple sources of OM (up- land soils, flooded forest, aquatic macrophytes, and phytoplankton) remains uncertain”

- Comment #3 (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?

Reply: The major potential sources of OM are described in P14, L15, and the non-flooded forests are included among them as follows: "To determine the origin of the SOM in the floodplain lakes, we considered five potential sources: (1) the terrestrial Andean clay-bounded and refractory SPOM, which may be transferred to the floodplain lakes via the Solimões-Amazonas and Madeira rivers (Hess et al., 2003), (2) “terra firme” soil and litter of the Amazonian lowland forest, which will be transferred to the floodplain lakes via streams, ...” We propose to make the necessary change in the

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text as follows: "To determine the origin of the SOM in the floodplain lakes, we considered five potential sources: (1) the terrestrial Andean clay-bounded and refractory SPOM, which may be transferred to the floodplain lakes via the Solimões-Amazonas and Madeira rivers (Hess et al., 2003), (2) "terra firme" soil and litter of the Amazonian lowland forest (non-floodable forests), which will be transferred to the floodplain lakes via streams,... "

- Comment #4 (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 main stem 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.

Reply: We agree with the Referee #1 for this point. We will revised the aspect related to the recalcitrance of the lignin compounds in the revised version.

- Comment #5 (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.

Reply: We will add more detailed information on the potential and diverse sources of brGDGTs and crenarcheol in the revised version.

- Comment #6 (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

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the complex hydrology/hydrodynamics of floodplains. This statement is also made in the abstract (P3, L19) and in the conclusions.

Reply: The hydrological cycles in the floodplain lakes are strongly linked to the the Amazon River main channel. Therefore, we believe that the data from Obidos are sufficient to illustrate the flood cycles for the investigated areas and the periods when the sediments were sampled. Further, all the lakes in the present study are strongly flooded by the Solimões-Amazon river system in the RW and HW seasons (white waters) with minor contribution (in some cases) by black waters. This reinforces that the data reported in Óbidos are directly linked to the floodplain lakes. The necessary changes in Table 1 will be made to clarify this statement

- Comment #7 (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.

Reply: We will provide more precise information on the sampling stations in the revised version. Although we sampled mostly at three sites in each lake for each period, we believe that our sampling site are representative for each lake with the most distinct regions of each lake: near the connecting channel, in the middle and near the floodable forests. This was sufficient to depict the major seasonal and spatial variations from upstream to downstream.

- Comment #8 (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?

Reply: The results of TN were corrected by the factor found in the referred correlation (0.06 wt.%). Once the values of TON were determined, the C:N ratio was calculated.

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The sentence will be corrected as follows:"In order to assess contribution of inorganic nitrogen (NH_4^+ + NO_2^- + NO_3^-) to TN, TN (wt. %) and TOC (wt. %) were correlated ($R^2 = 0.89$; $p < 0.001$; $n = 57$). The interception of the trend line in the TN axis (0.06) was interpreted as the percentage of inorganic nitrogen. Thus, this correlation showed that. . ..."

- Comment #9 (P9, L1): "Approximately 500 mg of freeze-dried sediments and macrophytes were ana- lyzed 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.

Reply: We will modify this part in the revised version.

- Comment #10 (P9, L12): What type of detector was used on the gas chromatograph (e.g. GC-FID)?

Reply: We used the GC-FID. We will add this information in the revised version.

- Comment #11 (P9, L13): Please clarify whether the recovery standard was added before CuO oxidation/extraction or before analysis on the GC.

Reply: The recovery standard was added after the CuO oxidation following the methodology presented in the literature (Hedges and Ertel, 1982; Goni and Hedges, 1992). We will add this information in the revised version as follows: "The recovery factor was calculated using the internal standard ethyl vanillin added after the CuO oxidation and prior to analysis (values above 60 % were considered)."

- Comment #12 (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)?

Reply: We also looked at the C/N ratio without the correction of the inorganic nitrogen, which revealed the same pattern. Hence, we believe that it is not probable that the correction used for the TON could affect the results presented.

- Comment #13 (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).

Reply: We will mention the spatial distribution of this parameter in the revised version.

- Comment #14 (P15, L17): "The averages of important lignin parameters (λ8, 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 end-member after it has been mobilized into streams and altered by biological processes.

Reply: This is a good point. It will be incorporated the literature mentioned by the Referee#1 into the revised version. Indeed, this subject is discussed in the text based on the (Ad:Al)v results (P15, L21). The effects of degradation can alter the composition of lignin phenols of each source and also in the sediment samples, where the (Ad:Al)v values are high. However, we could not find an appropriate approach to quantify this process. Based on that, we considered an error of 10% in our estimations of each fraction.

- Comment #15 (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

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as a soil OM indicator and in the abstract it was stated that crenarchaeol was used as a suspended POM indicator.

Reply: The crenarchaeol (iGDGT) itself is not indicative of soil input as mentioned above. It is typically produced in the water column, but can also be produced in other compartments such as soils and sediments. In the present work we interpret, based on Figure 5 and in the percentage of IPL fraction (Table 3), that the main source of crenarchaeol to the sediments of the floodplain lakes is the riverine SPOM (no data from lacustrine SPOM available). Consequently, our interpretation is based on the fact that, most of the crenarchaeol found in the river SPOM was produced in situ (Zell et al. 2013). Since none of our proxies trace the plankton primary production, we cannot estimate its fraction in the SOM but the crenarchaeol can be an indicator of the OM derived from the river and lake waters.

- Comment #16 (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.”

Reply: Equations 1 and 2 estimate the fractions of macrophytes and SPOM. Based on that, the remaining fraction must come from other sources, namely flooded and non-flooded forests.We will revised this sentence in the revised version as follows: "Consequently, the remaining 40–60 % of the SOM might be derived from the wetlands and non- flooded forests (Eq. 1). The periodic floods link the floodplain lakes and the wetland vegetation and soil. Thus, the seasonal and spatial contrasts in the SOM should be investigated in order to better understand the connectivity between these compartments.”

- Comment #17 (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

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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?

Reply: The mixing model presented the relevance of the interface between the lakes and the surrounding wetlands and non-flooded forests. To understand how this interaction occurs we investigated the contrasts in the spatiality and in the seasonality. The only clear trend in the spatiality was the δ13C values (Figure 3C), which was further investigated by the molecular δ13C in n-alkanes (P19). It showed that such trend was caused by changes in C4 plants communities (macrophytes and grass vegetation), more abundant in downstream open lakes. Based on that, we concluded that the interaction between the river main stream and the lakes is not the most relevant factor for the composition of SOM, but the macrophyte population and the surrounding soils (wetlands and "terra firme"). This corroborates the results indebted in the mixing model. Further, the seasonality showed significant changes in the λ8 and brGDGTs values. The higher concentration of these compounds in the FW season (more clear for the λ8 values) indicates that they are transported from the wetlands to the sediments. Finally, these observations, point to the conclusion that the river SPOM is not the main source of SOM, but the macrophytes and the local catchment area of each lake. This observation is presented as a general pattern for the central Amazon basin and not for some specific lake. Based on what is presented on previous works (e.g.: Victoria et al., 1992; Hedges et al.,1986; Mortillaro et al., 2011; Moreira-Turcq et al., 2013, Moreira et al., 2014) we understand that these conclusions are new insights concerning to this topic.

- Comment #18 (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.

Reply: The bulk parameters did not show any seasonal changes, which implies that these parameters do not change on these time scale (months). On the other hand some biomarkers do show significant change in these periods according to the periodical floods. Based on that we stated that in surface sediment samples, the bulk parameters cannot be applied to observe seasonal changes in the SOM but some biomarkers do so.."

- Comment #19 (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.

Reply #19: It is not correct that crenarchaeol was/is typically used as a marker for soil OM since this compound is predominantly produced in the aquatic environment. Nonetheless, it is true that the crenarchaeol can be produced in various environments. According to the literature and our own results (Figure 5), the concentration of crenarchaeol in the SPOM of the Amazon River is significantly higher than any other compartment. Based on this observation, we interpreted that this compound is mainly produced in the rivers and consequently, this is the main source of crenarchaeol to the SPOM and thus SOM. Thus, we applied the crenarchaeol as an indicator of SPOM. We will modify the sentences which were written ambiguously as follows: P5, L21: The ratio between brGDGTs and crenarchaeol have been applied to quantify the OC proportion originating from soils and aquatic environments (Hopmans et al., 2004; Herfort et al., 2006; Belicka and Harvey, 2009; Smith et al., 2010) and have recently been applied in rivers and floodplain lakes of the Amazon basin (Kim et al., 2012; Zell et al., 2013a; Moreira et al., 2014).

- Comment #20 (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 as-

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sumed/hypothesized?

Reply: The conclusions of the present work are based on the comparison of multiple biomarkers. This comparison consider the contrasts of each biomarker according to the spatiality and the seasonality. Apart of it, the mean values of the sediment samples were compared to other compartments of the floodplains ecosystem (data found in the literature), in order to qualify and to quantify the sources of SOM. The C:V ratio and the crenarchaeol were the only biomarkers which clearly indicate specific sources of SOM (macrophytes and SPOM respectively). Consequently, they were applied to quantify the fraction of these two sources of SOM, according to the end member approach (Eq. 1 and 2). The other biomarkers could also indicate the sources of SOM but not quantitatively. In terms of seasonality (which is linked to the periodic floods), the literature postulates that during the RW and HW the river transfer and deposits its SPOM into the lakes and this process is the main source of SOM. However, the crenarchaeol (the principal indicator of riverine SPOM) does not change seasonally. On the other hand, the $\lambda 8$ and the brGDGTs do change. Both can be transported to the lakes from the river SPOM in the RW season, based on the hydrology of this ecosystem (Bourgoin et al., 2007). In the FW water season, the only possible sources of these compounds are the sorrounding forests (wetland and "terra firme") in the catchment area of each lake. One can speculate that the macrophyte production of lignin and the in situ production of brGDGTs could cause such changes. If one considers this hypothesis, it is expected that the C:V ratio and the IPL brGDGTs would also increase in these same seasons, which was not observed (Figure 4D and Table 3). Thus, we interpret that in the RW season, the riverine SPOM and the soil runoff (as a consequence of the rain) are the causes of the increase in the $\lambda 8$ values. In the FW season, the main source is the receding waters, which transfer the forest OM into the lakes. We will modify the sentences as follows:

P20: "The $\lambda 8$ values showed significantly higher values in the RW and FW seasons (Figs. 4e, g, and 6a) in all lakes. The mean concentrations of brGDGTs also show

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higher values in the FW season (Figs. 4h and 6b) if compared to the HW season. The co-occurrence of these two types of molecules indicate that litter, traced by lignin phenols, and superficial soil, traced by brGDGTs, are preferentially deposited during rising and receding waters. Besides, the seasonal mean values of (Ad:Al)v show remarkably lower values in the RW and FW seasons (Fig. 4f), an inverse pattern if compared to the $\lambda 8$ and brGDGTs. This means that less degraded lignin is present in the surface sediments in the RW and FW seasons. Thus, the increase in the concentrations of the organic compounds is not a consequence of the re-suspension of the sediments, but to the arrival of fresher OM. In the HW and LW seasons, more degraded lignin phenols (higher values of (Ad : Al)v) are present in the sediments concomitant with lower amounts of $\lambda 8$. Consequently, the possible process via which the $\lambda 8$ and brGDGTs can be transferred to the lakes sediments are the connection with the river main stream and with the local catchment area (wetland and "terra firme"). The lignin concentration could also increase as a consequence of the macrophyte communities, while the brGDGTs could increase due to the in situ production. However, the concentration of crenarchaeol, C : V ratio and the IPL brGDGTs do not reveal significant seasonal changes (Table 3 and Fig. 4). Based on these observations, we interpret that these changes in 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 soil and leaf runoff. Previous works postulated that Andean and low land soil material is mainly transferred to the lakes via river main stream, in particular, during the RW and HW seasons and that would be the main source of SOM of the floodplain lakes (e.g., Victoria et al., 1992; Moreira-Turcq et al., 2004; Mortillaro et al., 2011). However, according to our results, the lignin phenols increase their concentration in the RW and the FW seasons. Thus, based on the hydrodynamics of floodplain lakes (Bourgoin et al., 2007) and the comparison between the biomarkers applied in this study, in the RW and FW seasons, these organic molecules are mainly derived from the drainage of local wetlands and "terra firme" soils. This is more evident for the upstream lakes, which are surrounded by flooded forests and by larger flooded area, than for the downstream lakes, which are surrounded mainly by grass vegetation and shrubs. 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 lake and the wetland and non-flooded forests are determinant for the sedimentation of the organic compounds."

- Comment #21 (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?

Reply: The literature postulates that in the RW and in the HW seasons the flux of suspended material goes from the river main stream to the lakes. In FW season the flux goes in the opposite direction, from the flooded forests and lakes into the river. Finally, in the LW season, these interactions are very small (Bourgoin et al., 2007). Consequently, the increasing values in $\lambda 8$ and brGDGTs, could be caused by the intense exchange of material between these compartments. As presented in Reply #20, after comparing these data with other biomarkers, we interpret that, the main cause for such seasonal changes are the interface between the lakes and the local wetland and non-flooded forests.

- Comment #22 (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?

Reply: This statement could not be determined quantitatively. However, the mixing model results showed that the SPOM fraction is 20-30%, which indicates that the majority of the SOM does not come from this source. Further, the increasing values ob-

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served in $\lambda 8$ (RW and FW seasons) and brGDGTs (FW season), and the comparison with other biomarkers, such as the C:V ratio, crenarchaeol and IPL brGDGTs, leads to the interpretation that the interface between the lakes and the local catchment area are determinant for the composition of the SOM. This process occurs in both upstream and downstream lakes since no spatial changes were observed for any biomarker, except the bulk $\delta 13C$.

- Comment #23 (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 macrophytes 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.

Reply: Both wetlands and non-flooded forests (“terra firme”) can be relevant sources of SOM. We interpret that the wetlands are the most important among them, since it is nearer to the lakes and direct linked to them due to the periodic floods. However, the end member estimations are not conclusive about the fraction of each of these two sources to the SOM. We modified sentences as follows: “The vegetation coverage of the wetlands (flooded forests) and “terra firme” (non-floodable forests), in the local catchment area of each floodplain lake, are the most important source of SOM in floodplain lakes of the central Amazon basin. The macrophyte community is also an important source of SOM whereas the river SPOM contributes to a minor fraction of it.”

- Comment #24 (P21, L15): “The sedimentation of OC in the floodplain lakes are linked to the periodical floods.” Hydrology/hydrodynamics are not discussed beyond discharge at OÌĄbidos. 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.

Reply: It has been widely reported in the literature the role of the periodic floods in the biogeochemistry and ecology of the floodplain lakes, in particular to the composition of the SOM (e.g., Hedges et al., 1986; Junk, 1997, Moreira-Turcq et al., 1013). The present work reinforces that the periodic floods are determinant for the sedimentation of OM in the lakes. However, we propose that the interface between the lakes and the local catchment area is more relevant than the interface between the lakes and the river main stream.

- Comment #25 (P21, L19): "Hence, together with wetland vegetation, the hydrodynamics of the flood- plain 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.

Reply: The hydrodynamics of the floodplain lakes is driven by the periodic floods, which is well reported in the literature (e.g., Hedges et al., 1986; Junk, 1997, Moreira-Turcq et al., 1013, Bourgoin et al., 2007). The present work emphasizes that the periodic floods are determinant for the sedimentation of OM in the lakes. However, we propose that the interface between the lakes and the local catchment area is more relevant than the interface between the lakes and the river main stream.

Technical Corrections:

-TC #1 (P4, L7): Capitalize "and"

Reply: We will correct it as follows: "The primary production is performed by the flooded forest, macrophytes, phytoplankton and periphyton (Junk et al., 2010) and further, the organic matter (OM) produced in the floodplain lakes fuels the outgassing CO2 in the river system (Abril et al., 2014). "

-TC #2 (P6, L11): "rivers" or "River" should be added after the river names (e.g. "Tapa-

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joÌĄs River”) P6, L21: Capitalize “River”

Reply: We will correct it as follows: P12, L11: "white water (e.g. Solimões, Madeira and Amazon rivers), black water (e.g. Negro River), and clear water (e.g. Tapajós River)." P21, L3: "However, even in Curuai, where the primary production and the riverine SPOM is admittedly an important source of SOM. . ."

-TC #3 (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?

Reply: The necessary changes will be made in the text as follows: "The four hydrological seasons were targeted during different research cruises with a small research vessel (Fig. 2): in June and July 2009, which covered the High Water (HW) season; in October 2009 covering the Low Water (LW) season, in August 2010 covering the Falling Water (FW) season and in January 2011, which covered the Rising Water (RW) season. In each floodplain lake, sediment samples were collected at three stations in each season." P8, L5: "Total carbon (TC), total nitrogen (TN), and δ13C for the samples obtained during the HW and LW cruises. . ." P8, L9: "Four samples obtained during the FW and RW cruises were analysed. . ..”

-TC #4 (P8, L17): Delete “and”

Reply: We will delete it as follows: "TC (wt.%) correlated very well with TOC (wt. %) with a +0.16 intercept (R2= 0.96; p < 0.001; n = 16)."

-TC #5 (P9, L18): Change “chromatography” to “chromatograph”

Reply: We will change it as follows: "To confirm the identification of each lignin phenol, eight selected samples were analyzed with an Agilent 7890A gas chromatograph coupled to an Agilent 5975C VL MSD mass spectrometer using a selective ion monitoring (SIM) at NIOZ (The Netherlands)."

-TC #6 (P10, L25): What brand/model HPLC-APCI-MS was used? P10, L9: Change “chromatography” to “chromatograph”

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Reply: We will change it as follows: "The core lipids and IPL-derived GDGTs were analyzed using high performance liquid chromatograph-atmospheric pressure positive ion chemical ionization-mass 25 spectrometry (Agilent 1100 series LC/MSD SL, Alltech Prevail Cyano column (150×2.1 mm; 3μm)) in selected ion monitoring (SIM) mode according to Schouten et al. (2007)." P11, L9: "The n-alkanes in the apolar fraction were identified by a Thermo Finnigan Trace DSQ gas chromatograph (GC-MS) and quantified with an HP 6890 GC system."

-TC #7 (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.

Reply: We will report p values in the revised version.

-TC #8 (P11, L4): Change “value” to “values”

Reply: We will change it in the revised version.

-TC #9 (P12, L19): It should be noted that Lambda 8 is the “amount of lignin” normalized to OC.

Reply: We will add this information in the revised version as follows: "No significant changes were observed along the upstream-downstream transect for the mean values of λ8 (i.e. a proxy for the amount of lignin normalized to OC);. . ."

-TC #10 (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.

Reply: The term "Significant" means $p < 0.05$ as described in session 3.6. We will report p values in the revised version.

-TC #11 (P17, L7): Add a space to “samples can” P17, Line 22: “Periodical” should be

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replaced with “periodic” here and elsewhere.

Reply: We will revise the sentence suggested by the referee#1 as follows:"According to this approach (Martinelli el al., 2003), the C : V values of macrophytes and the average values of soil and riverine SPOM sample scan be used to estimate the contribution of macrophyte OM to the SOM." We will also revise the following sentences: P17, L22: ”The periodic floods link the floodplain lakes and the wetland vegetation and soil." P4, L8: "Periodic floods intensify the exchange of organic compounds, nutrients and minerals between rivers, lakes and flooded soils (Junk, 1997).” P21, L15: "The sedimentation of OC in the floodplain lakes are linked to the periodic floods. . ."

-TC #12 (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.

Reply: We will correct it as suggested by the Refefee#1 as follows: "Accordingly, the fraction of C4 plants in the SOM in the upstream lake is only 3 %, but for the downstream lake 22 %."

-TC #13 (P20, L15): Change “manly” to “mainly”

Reply: We will corrected it as suggested by the Refefee#1 as follows: "Since the concentration of crenarchaeol (a marker for SPOM) and the C:V ratio (mainly affected by aquatic macrophytes; see above). . ."

References: Reply: We will add the following references in the revised version.

Schmidt, M.W.I., Torn, M.S., Abiven, S., Dittmar T., Guggenberger G., Janssens I.A., Kleber, M., Kogel-Knabner, I., Lehmann J., Manning, D.A.C., Nannipieri P., Rasse D.P, Weiner S., Trumbore, S.E.: Persistence of soil organic matter as an ecosystem property, Nature, 478, 49-56, 2011.

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Bonnet, M.P., Barroux, G., Martinez, J.M., Syeler, F., Moreira-Turcq, P., Cochonneau, G., Melack, J.M., Boaventura, G., Maurice-Bougoin, L., León, J.G, Roux, E., Calmant, S., Kosuth, P., Guyot, J.L., Seyler, P.: Floodplain hydrology in an Amazon floodplain lake (lago Grande de Curuai), Journal of Hydrology, 349, 18-30, 2008.

Bourgoin, L.M., Bonnet, M.P., Kosuth, P., Cochonneau, G., Moreira-Turcq, P., Guyot, J.L., Vauchel, P., Filizola, N., Seyler, P.: Temporal dynamics of water and sediment exchanges between the Curuai floodplain and the Amazon River, Brazil, Journal of Hydrology, 335, 140-156, 2007

Interactive comment on Biogeosciences Discuss., 12, 8747, 2015.