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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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- Comment #1: Spatial heterogeneity within each lake: All lakes (except for Mirituba) are described as receiving water and sediments from multiple sources (i.e. white waters, black waters, clear waters), yet only 2-3 samples were collected for each lake in each season. There is no discussion as to the spatial variability of SOM within each lake for a given season, although this could have large impacts on the observed seasonal variability. For example, Moreira-Turcq et al., 2004 state that, "[sediment] fluxes were highly variable in space [within the Curuai lake], precluding extrapolation from a few measurements to a single value for the whole vaÌĄrzea." This heterogeneity was

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also reflected in %OC, C/N ratio, mineralogy, etc. The role of spatial heterogeneity within each lake should be addressed here.

Reply: The present work does not intend to evaluate the heterogeneity of each lake. For such, an effort, it would be necessary to examine a higher number of samples and a more detailed description of each lake, in terms of hydrological and physic-chemical characteristics, as pointed out by referee #2. Instead, our aim was to understand the seasonal and spatial variations with respect to the sedimentary organic carbon in the floodplain lakes of the central Amazon basin. To this end, we selected three sampling sites in each of the five major floodplain lakes during four hydrological seasons. The selected sites were the most distinct regions of each lake: near the connecting channel, in the middle and near the floodable forests. As a result, we have approximately twelve samples for each lake for characterizing the spatiality along the transect from upstream to downstream, and we have approximately fifteen samples per season for characterizing the seasonality. In total, 57 sediment samples were analyzed in this study. This gives us a robust sampling set to investigate changes in spatiality and seasonality and to compare the results from the sediment samples with other compartments of the ecosystem and sources of OM, based on the statistics. In order to illustrate the variability in our data we presented the error bars in Figure 4 and all mean values used in the variance analyses (ANOVA) were presented in box plots. In the revised version we will make the aim of this study clearer.

- Comments #2: Sample collection and analysis: Naming conventions are not consistent throughout the manuscript. For example, the authors state that, “four riverbank sediments ... were also collected during the LW season,” (p8753, line 24-25) however these samples are also referred to as “wetland soils” (e.g. in Table 2). Bulk carbon % and δ13C values reported are for raw samples – i.e. not decarbonated – although a subset of decarbonated samples resulted in similar carbon content with an offset of 0.16% (δ13C not compared). Assuming this 0.16% is inorganic carbon, this could explain âĹij0.8‰ of the observed downstream δ13C SOM enrichment. This is not a large

difference (12% of the total observed), but should be addressed explicitly. Similarly, δ13C should therefore not be referred to as δ13Corg throughout the manuscript. For n-alkane quantification, peak areas should be calibrated against an external standard, with an internal standard only used for calculating extraction recovery. More detail should be given for GC-IRMS methods, such as column used, standard reproducibility, calibration method (i.e. using pulses of CO2 with known δ13C?), etc.

Reply: We thank the referee for spotting the inconsistencies of sample names and will correct them in the revised version of our manuscript. We will also provide more detailed description of the GC-IRMS method in the revised version. However, it is not clear for us why we should use an external standard if we use the internal standard, which is more common exercise in our field, for the quantification.

- Comment #3: Reporting of results: Significant inconsistency exists between the results reported in the Results section of the main text and Tables 2-3, and tables / figures are mislabeled throughout the main text.

Reply: Thanks for spotting such mistakes. We will go through the text, figures and tables carefully and correct such mistakes thoroughly.

For example: Page 8758, Line 3: ". . . lower mean value (Table 2) in the downstream Lake Curuai," should refer to Table 3 and Lake Canacl̀ğari.

Reply: This will be corrected in the revised version as follows: "The TOC content was the lowest in the downstream lake Curuai (2.0 $\pm$ 0.6 wt. %) and the highest in lake Cabaliana (3.3 $\pm$ 0.8 wt. %) (Fig. 3a, Table 3)." ....

Page 8758, Line 6-7: "The lowest mean value was found in Curai (10 $\pm$ 1) and the highest one in lake Mirituba (11 $\pm$ 2)." These values are statistically identical.

Reply: This will be corrected in the revised version as follows: The sentence: "The lowest mean value was found in Curuai (10 $\pm$ 1) and the highest one in Mirituba (11 $\pm$ 2)" will be deleted..

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Page 8758, Line 11: “Riverbank sediments” is referred to as “Wetland Soils” in Table 2. aÌĆA ÌĘ c ÌĄ Page 8758, Line 12: Table 3 should read Table 2.

Reply: This will be corrected in the revised version as follows: ”The δ13Corg values in soils and wetland soils varied between -29 and -19 ‰ (n = 7). . .”

Page 8758, Line 15: “The C3 macrophytes ... δ13C values of -30‰ ÌĞ” The range reported in Table 2 is - 30‰ to -32‰

Reply: This will be corrected in the revised version as follows: ”The C3 macrophytes (Eleocharis sp. and Pistia stratiotes) had δ13Corg values between -30‰ and -33‰ and values of C : N ratio between 15 and 24 (Table 2). . . .”

Page 8758, Line 23: Fig. 3g should read Fig. 4g

Reply: This will be corrected in the revised version as follows: ”. . .compared to the HW (23$\pm$9 mg goc-1) and LW (29$\pm$12 mg goc-1) seasons (Fig. 4g). . . .”

Page 8758, Line 25 – Page 8759, Line 1: “The values of S:V ratio did not show significant spatial differences either but higher mean values in the RW season (1.1 $\pm$ 0.1) and in the FW season (1.2 $\pm$ 0.2) were observed in comparison to that of the LW season (0.9 $\pm$ 0.1).” These values are statistically identical.

Reply: This will be corrected in the revised version:

Page 8759, Line 4-11: All numbers reported here are inconsistent with the values reported in Table 2. Again, “riverbank and wetland soils” is referred to only as “Wetland Soil” in Table 2.

Reply: This will be corrected in the revised version as follows: ”For the C3 macrophytes, λ8 values varied between 26–67 mg g-1 and between 48–94 mg g-1 for the C4 macrophyte samples. The S : V ratio varied between 0.6 and 0.9 for C3 macrophytes and between 0.4 and 0.7 for the C4 macrophyte. The range of C : V ratio was 0.4 to 3.7 for the C3 macrophytes and 1.7 to 4.0 for the C4 macrophytes. The (Ad : Al)v ratio

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varied between 0.2 and 0.8 for all macrophyte samples (Table 3). For the "Terra Firme" soil and wetland soil samples, the λ8 values varied between 9 and 88 mg g-1. The S:V ratio varied between 0.5 and 1.1, the C : V ratio varied between 0.2 and 0.5, and the (Ad : Al)v ratio varied between 0.6 and 1.5". . ..

Page 8759, Line 14-17: Unclear whether this is referring to core GDGTs, IPL GDGTs, or both. Values of crenarchaeol reported in the main text, ". . .higher in Canacìğari (115 $\pm$ 57 μg gOC-1) when compared to Janauaca (34 $\pm$ 33 μg gOC-1)," are an order of magnitude higher than the values reported in Table 3.

Reply: This will be corrected in the revised version as follows: BrGDGT refers to the CL fraction and IPL brGDGts to the IPL fraction as presented in Table 3.

Page 8759, Line 26: ". . . mean values varied between 5 $\pm$ 4. . ." while mean value reported in Table 3 is 4.

Reply: This will be corrected in the revised version as follows: "The mean values varied between 4 $\pm$ 4 and 10 $\pm$ 6 μg g-1 in the HW and LW seasons, respectively." . . ..

Page 8760, Line 5-12: n-alkane results reported in Table 6 although Tables 4 and 5 have not been introduced yet. Tables should therefore be re-arranged for clarity.

Reply: We will rearrange the Tables according to the appearance in the text in the revised version as follows: ". . .and the C : N ratio values varied between 6 and 16 (n = 7; Table 2)." . . ..

Additionally, only average values are reported in Tables 2 and 3, while uncertainty is reported and interpreted in the main text. Analytical uncertainty should be reported in Table 2, and standard deviations about the mean values should be reported in Table 3.

Reply: We will report the uncertainties in Table 2 and 3 in the revised version as follows: "The mean concentrations of crenarchaeol were higher in Canaçari (12 $\pm$ 6 μg g-1) when compared to Janauaca (4 $\pm$ 3 μg g-1)."

P8760 L.5-12: Rearrangement of table numbering: Reply: We will rearrange the table numbers as follow: Table 6 will become Table 4 Table 4 will become Table 5 Table 5 will become Table 6

Other changes in the text: Reply: We will correct them as follow: P8761 L.12 - ". . .those of the SOM of the floodplain lakes in Fig. 5 and Table 5." P8763 L.10 - ". . .the SOM (Eq. 1–3, Table 6)." P8763 L.18 - ". . .the values of the other possible sources (Table 6)" P8765 L.5 - "The results (Table 4) show that the long-chain n-alkanes δ13C signature. . ." P8765 L.13 - ". . .sediments of Janauaca and Curuai are listed in Table 4." Table 2 - Analytical error will be reported Table 3 - Standard deviation will be reported

- Comment #4: Interpretation of end members: One major concern is the inconsistency in interpre-tation of end members and the biomarkers used to infer them. For example, lignin is referred to both as a "recalcitrant organic macromolecule" as well as a "relevant source for the outgassing of CO2 from the Amazon River," implying that lignin is labile (p8751, line 15-20).

Reply: Although lignin is generally considered more recalcitrant in comparison to other organic compounds, recent studies also showed that this compounds can be degraded during the transport (Schmidt et al., 2011 Nature). Therefore, it is not inconsistency to interpret our data in this regard. However, we also see that the link between theses point is not well described in the current version and thus we will incorporate this point made by the referee in the revised version.

Discussion of lignin parameters (p8761, line 9 – p8762, line 4) does not discuss the fact that mixing of C:V, S:V and (Ad:Al)v is highly nonlinear between sources due to their variable λ8 values.

Reply: We understand that this is a very pertinent comment about the nonlinearity of the lignin phenols in this approach. It will be properly discussed in the revised version.

Additionally, brGDGTs are said to track soil OM (p8749, line 10) as well as in situ

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production (p8762, line 10-13), while the authors state that, “riverine SPOM is the only possible OM source to explain a substantially increased concentration of crenarchaeol, in the SOM of the floodplain lakes if compared to other sources” (p8762, line 13-15). However, crenarchaeol is then used “as an (indirect) indicator of aquatic primary production.” (p8762, line 20). Riverine SPOM is itself a complicated mixture of OC with highly variable contribution by phytoplankton production depending on the type of river (i.e. white, black, clear) and the water stage (e.g. Kim et al., 2012 GCA). Therefore, the simplification that crenarchaeol tracks riverine SPOM contribution used here should be refined.

Reply: In general, brGDGTs are mainly produced in soils, while crenarchaeol is predominantly produced in aquatic environments such as lakes and rivers. And thus brGDGTs can be used to trace soil OM input from land to the aquatic environment and crenarchaoel as an indicator for the aquatic production. Although this fact can be complicated since it turned out that brGDGTs are also produced in the aquatic environments and crenarchaeol in soils, it has been shown that detailed studies in a given area can give us detailed information from where these compounds are mainly originated and thus we can use them to trace the source of sedimentary OM. We will make this point clearer and try to avoid any inconsistency in the text in the revised version.

- Comment #5: Mixing model: The linear mixing model approach used here is underconstrained and nonlinear, and therefore invalid as presented. As an example of nonlinearity, a mixture of 50% macrophyte-derived OC and 50% riverine SPOM-derived OC will bias toward the macrophyte end-member due to the contrasted lignin concentrations (λ8 values) between these end members, resulting in a C:V of the mixture of âĹij1.6 rather than 0.75 if mixing was linear. Additionally, this model is inherently a 3 end-member mixing (rather than 2 as stated): macrophyte, aquatic, and “other” (also referred to as wetlands?). Thus, determining Fmacrophyte and Faquatic independently and solving for Fwetlands by difference is invalid, for example due to the influence of C:Vaquatic to the total C:VSOM which is not incorporated into the model as presented.

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Instead, this should be simultaneously solved as a system of 3 equations with 3 unknowns. Lastly, it is unclear how the authors chose end-member values for the "other" source or how they determined the uncertainty in the resulting fractional contributions.

Reply: To make possible the estimations of each fraction of SOM based on the end member approach we grouped the principal sources of SOM in Macrophytes, SPOM (riverine and lacustrine) and the surrounding forests (wetlands and "terra firme" soils, leaf, grass and wood). The calculation used only the biomarkers which were characteristics of one specific source. In this sense, the fraction of the calculated source was distinct from any other sources. For example, the C:V ratio indicates macrophytes, since its concentration in macrophyte samples are higher than any other source. Thus, C:V in the riverine SPOM does not affect the results. The same is expected for the calculations of SPOM based on the crenarchaeol. There is no specific biomarker to calculate the forest fraction. Thus, it was estimated based on the results of the two other fractions as follows:

Fmacrophyte + FSPOM + Fforest = SPOM (Eq.1)

FSPOM = [(CrenSOM - Cren(forest+macrophye)) / (CrenSPOM - Cren(forest+macrophyte)] x 100 (Eq.2)

Fmacrophyte = [(C:VSOM - C:V(SPOM+forest)) / (C:Vmacrophyte - C:V(SPOM+forest))] x 100 (Eq.3)

The values of Cren(forest+macrophyte) and C:V(SPOM+forest) was determined on the average value of the respective biomarker in these sources. The data used to calculate these averages are presented on Table 4 and the averages on Table 5 as "OCother" (this will also be modified in the final version) . Finally, we understand that present version of the formulas and the text in the manuscript were not clear about our mathematical background and our interpretation, thus we propose to do the necessary changes as it has been exemplified it the responses to referee #1 and #2.

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The necessary modifications in the text will be done in the text as follows: “In Eqs. (2) and (3), the FSPOM and Fmacrophytes represent the estimated fractional abundance in SOM of macrophytes and SPOM, respectively. C:VSOM and CrenSOM are the average values of each parameter found in the sediment samples, C:Vmacrophytes and CrenSPOM are the values of the source of the respective parameter and C:V(SPOM+forest) and Cren(forest+macrophyte) are the values of the other possible sources (Table 5). These calculations indicate that 20–30 % of the SOM is derived from macrophytes and 20–30 % from. Consequently, the remaining 40–60 % of the SOM might be derived from the wetlands and non- flooded forests (Eq. 1). The periodical 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.”

The authors dedicate most of the results and discussion section to presenting data which show differences between lakes or seasons, i.e. increasing δ13C and decreasing %OC downstream, lower λ8 during LW and HW, lower brGDGTs during HW. However, none of these differences are incorporated into the mixing model presented here. There is no justification given for grouping all locations and seasons into a single mixing model despite their disparate bulk and biomarker values. In fact, this is contradictory to the observed downstream increase in bulk and n-alkane δ13C.

Reply: Even considering the spatial and seasonal variability for some biomarkers, which is not the case for crenarchaeol and C:V, the mixing model intends to compare the different compartments of the ecosystem and sources of SOM with the sediments. In this case, the seasonality and the spatiality should not be taken into account.

Additionally, the statement that, “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,” (p8764, line 7-10) is highly speculative and requires justification.

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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.

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