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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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The authors assert that sedimentation in floodplain lakes in the Amazon basin is an important yet under-constrained process in the regional carbon cycle, and attempt to use bulk geochemical parameters and biomarker measurements to better constrain sedimentary organic matter sources both spatially and temporally. The authors make two main conclusions: (1) that higher lignin and brGDGT concentrations, higher S:V ratio, and lower (Ad:Al)v during raising and falling water seasons indicate increased contribution by litter and surficial soils during this time, and (2) that enrichment in bulk and n-alkane δ13C indicate a downstream increase in C4 macrophyte contribution to SOM across all seasons. They speculate that hydrological dynamics are the likely cause for such differences, although no hydrology data other than water level at Óbidos

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are given.

Overall, the authors present a robust set of bulk and biomarker data from 5 floodplain lakes across an entire hydrologic cycle, with the goal of describing how sources of OC vary spatially and temporally – a very laudable endaevor. However, the manuscript is hindered by confusion about biomarker sources and their role as tracers for specific end members. Additionally, the results and discussion are contradicting at times, especially regarding the grouping of all lakes at all times into a single mixing model while the data indicate spatiotemporal changes in bulk and biomarker values. Lastly, the mixing model needs to be revised in order to be properly constrained and address nonlinearity, and justification for the choice of end-member values (especially "other") must be given. As presented currently, these results cannot be interpreted beyond qualitative inference. As such, I believe major and substantial revisions are needed before the manuscript can be published in Biogeosciences. My main comments are outlined bellow.

General points and questions:

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

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

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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 $\sim$0.8‰ 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.

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. For example: âĂć Page 8758, Line 3: “. . . lower mean value (Table 2) in the downstream Lake Curuai,” should refer to Table 3 and Lake Canaçari. âĂć 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. âĂć Page 8758, Line 11: “Riverbank sediments” is referred to as “Wetland Soils” in Table 2. âĂć Page 8758, Line 12: Table 3 should read Table 2. âĂć Page 8758, Line 15: “The C3 macrophytes . . . δ13C values of -30‰” The range reported in Table 2 is -30‰ to -32‰âĂć Page 8758, Line 23: Fig. 3g should read Fig. 4g âĂć 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. âĂć 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. âĂć 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 Canaçari (115 $\pm$ 57 μg gOC-1)

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when compared to Janauaca (34 $\pm$ 33 μg gOC-1)," are an order of magnitude higher than the values reported in Table 3. âĂć Page 8759, Line 26: ". . . mean values varied between 5 $\pm$ 4. . ." while mean value reported in Table 3 is 4. âĂć 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. 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.

4. Interpretation of end members: One major concern is the inconsistency in interpretation 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). 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. Additionally, brGDGTs are said to track soil OM (p8749, line 10) as well as in situ 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.

5. Mixing model: The linear mixing model approach used here is under-constrained 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

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values) between these end members, resulting in a C:V of the mixture of ∼1.6 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. 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. 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. 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.

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