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Interactive Comment

Interactive comment on "Sources and transfers of particulate organic matter in a tropical reservoir (Petit Saut, French Guiana): a multi-tracers analysis using δ^{13}C, C/N ratio and pigments" *by* A. de Junet et al.

Anonymous Referee #2

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General comments:

The authors present a multi-parameter geochemical approach to characterize the source and transfer of organic matter in a tropical reservoir. This is a nice study, employing an impressive suite of geochemical measurements and providing the reader with a good background on the potential and use of those parameters, particularly when used in combination. The topic of the manuscript clearly fits the scope of Biogesciences and is of relevance: Understanding the source and cycling of organic matter in reservoirs is imperative for the understanding of CO_2 exchange with the atmosphere (heterotrophy vs. autotrophy) and the overall primary productivity in this tropical environment. The investigated reservoir, with various OM sources, seems to represent a suitable environment, in which to conduct such a multi-tracer study, and calibrate the factors that control bulk geochemical signals within sediments. Nevertheless, I felt that this work only modestly enhances our present understanding of OM sources and biogeochemical cycling in this reservoir and that the attribution of geochemical signatures to OM sources is quite speculative. I had the impression that the data did not allow such conclusive evidence as to the source and decomposition of OM in the reservoir as the authors state, and I was missing a more cautious interpretation of the data. Often, the authors picked what fit from the literature but ignored other possibilities.

The most evident shortcoming of the paper is that it lacks any constraints on temporal changes in sources and the geochemical and isotopic composition of organic matter into and out of the reservoir. The fact that system-internal processes like variable C-isotope fractionation as a function of pCO_2 or the switch by phytoplankton from CO_2 to bicarbonate uptake can produce large changes in the C-isotope signature of both suspended and sinking organic material has been almost completely ignored. Temporal variations in the d13C of OM up to 15 could be attributed to reservoir effects of C uptake in many other freshwater ecosystems. Clearly, seasonality effects do not play a big role in tropical ecosystems, but there is no doubt that phytoplankton blooms also occur in tropical freshwater environments.

I understand that one could always do more and I acknowledge that the presented data set what is already quite impressive. Yet, the fact that this data set may not be representative needs to be highlighted, and potential implications of seasonal variations in isotopic signatures need to be discussed.

I got the impression that the sampling was not well planned. Why taking only one core in the littoral zone? Why sampling only two biofilms? Are they representative for the total biofilm biomass in the reservoir? What is the contribution to the total biomass anyway? Again, it is difficult to constrain the water column biogeochemistry using a single water column profile. The system is likely to change spatially and temporally.

The introduction was very promising and well-written, raising high hopes for the rest of the paper. Yet, a significant amount of shortcomings and inadequacies is present in the sections that follow, and there, the manuscript is rather poorly written, with numerous grammatical errors.

In detail:

The abstract is quite long and should not represent a condensed *Results* section only. I was missing any statements as to the significance and implications of the findings.

p. 1165, l.18: Why did the authors retrieve a sediment core in the littoral zone? This clearly is not the location that is representative for the general sedimentation conditions in the reservoir.

p.1172, l. 23: “The combination of three kinds tracers.....allows to describe the major patterns of OM origin”. This is not an acceptable way to start a discussion. I do not even think the first sentence is true. But if it were, it should be part of a conclusion, at which the authors may arrive after thorough discussion of their data.

p.1173, p.23: Is there any indication for the diatoms being benthic rather than pelagic? Wouldn’t pelagic diatoms be the first guess?

In general, the authors may want to look at their SPM and trap samples using a microscope. Pigments concentration determinations a good complementary tool, but the easiest way to detect algal material in recent sediments is to have a detailed look at the samples.

p.1175, first paragraph: A C/N ratio of 10–12 is extremely high for sample that contains a large amount of bacterial biomass. Similarly, the d13C indicates “regular" phytoplankton. Methylotrophic bacteria can indeed explain the lower d13C, but one would expect a minimum in d13C right at the oxycline. Here methylotrophic biomass should peak because bacteria have excess to both O_2 from above and methane from below the oxycline.

Is there a biomarker for methylotrophic bacteria? Hopanoids?

p.1175, second paragraph: The authors argue that the at 3 m water depth the high C/N ratios can be attributed to stoichiometrically “unusual" phytoplankton rather than to the input of terrestrial plants. What about the TOC/pigment ratio? It is much higher than typical for phytoplankton, and suggests a terrestrial origin.

The discussion of TEP is highly speculative, and the argumentation is weak. First, most environments in the ocean are N limited but algal exudates do not play a large role in the export production, at least we do not know much about it. Second, I doubt that this environment is N-limited. Are nitrate and ammonium concentration available?

Section 4.2: Often it is not clear when the authors write about actual sediments, settling or suspended particles. For example on p. 1176, l. 23, what is meant with “sedimentary source"?

The surface sediment signal represents a signal that may integrate one year or so of sedimentation, whereas the sediment trap material represents sedimentation only during a minor portion of the year. Thus it is difficult to compare the d13C in trap material and in sediments, and to infer preferential sedimentation or degradation as plausible explanations for the observed difference in d13C.

p.1177, first paragraph: Why does the presence of Scytonemin necessarily indicate the presence epiphytic biofilms in all sediment traps? Are there no other sources of Scytonemin? Could the cyanobacterial biomass not be derived from the water column? Also, I would imagine that OM in biofilms is rather immobile.

p. 1177, second paragraph: Explaining the variation in C/N and d13C in the sediment core with variations in the source is too simple. What about possible effects due to changes in productivity or stoichiometric and isotope alteration during early diagenesis?

p.1177, l. 26: “result of complex biological and chemical mechanism" can mean everything. Be more specific.

Section 4.3, first paragraph: Is there any other evidence than the low d13C that indicates the contribution of methylotrophic bacteria to the biofilm biomass? Can methylotrophic bacteria be expected in the biofilms? Has this been observed elsewhere? This seems to be an interesting aspect, but complementing evidence (e.g., biomarkers) would be desirable.

Section 4.4: The authors sampled the main input and the output, yet I was missing a more comprehensive discussion on OM balances and budgets. Most of the autochthonous material is remineralized, but a large amount is transported laterally and then downstream. Is the reservoir a net sink for OM that enters the reservoir? A graph with fluxes of Corg and DIC may help, including data by Abril et al. (2005). But again, settling, import and export fluxes only represent a snapshot in time and may not be representative for the annual average fluxes.

p. 1181 : I do not agree that the observations highlight the importance of TEP. Parts of the conclusion (l. 19–20) suggest that the discussion on TEP has been a major component of the article. Yet, the later have barely been investigated and microscopic and geochemical evidence elucidating their existence and mechanisms that lead to the accumulation of TEP does not exist or is rather vague.

Minor points:

p.1165, l.11: not porosity but pore size

p.1166, l.15: “analyses...on duplicate samples"

p.1167, l.5: “were spun"

p.1170, l.18: “showed a maximum"

Throughout the text: clearly differentiate between suspended, sinking and sedimented particles. Distinguish between POC and POC concentrations (e.g., not “POC decreased" but “POC concentrations decreased"

p.1171, l.17: “contained very few pigments"

p.1171, l.25: “trunks"

p.1172, l.4: “with traces of scytonemin...."

p.1176, l.12 “settling through the water column"

p.1177, l.1: protect what?

p.1177, l.11: I would not call Zuellig’s records of a couple of hundred years “geological".

p.1179, l.22 “lacustrine"

Interactive comment on Biogeosciences Discussions, 2, 1159, 2005.

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