

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

A. de Junet et al.

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Reviewer 2

Comment 1. 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.

Reply : In the revised MS, we have more carefully considered the possibilities of tem-

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poral changes in the geochemical signature of POM, where appropriate (in particular in section 4.2). As we argue in the paper, our identifications of sources is more reliable by using the different tracers in combination. Furthermore, we confront our information with literature data on the limnology of the reservoir realise that this tropical reservoir is the best documented so far. Collectively, this allowed us to identify sources of OM in a reliable way and to draw a first picture of OM cycling. Nevertheless, we recognise that a more detailed and quantified understanding of OM cycling requires the study of seasonality, because of differences between dry and wet seasons (see Conclusion).

Comment 2. 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 pCO₂ or the switch by phytoplankton from CO₂ 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 δ¹³C 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.

Reply: this comment is considered in section 4.2: “In addition, the signature of POM in the water column may have changed during the traps deployment which lasted 48 days. This could explain part of the differences in the signature of planktonic and traps material. However, drastic changes in the isotopic composition of phytoplankton are probably limited in these low alkalinity (~0.1mmol.kg⁻¹), acid waters (pH~5-6), where pCO₂ is always very high (Abril et al. 2005) and where switches by phytoplankton from

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CO₂ to bicarbonate utilisation can be excluded”.

Comment 3: I got the impression that the sampling was not well planned. Why taking only one core in the littoral zone? Reply: as already stated in the previous version of the MS (P1165, L14-15): “At deep stations in the reservoir, the presence of trunks and branches at the bottom precludes the use of box cores” On that ground, we have considered that information coming from a core in the littoral zone could still be helpful. In particular, the deeper part of the core, which has the geochemical characteristics of the soil flooded 10 years before, is presumably similar to what occurs in the deep part of the lake. Potential differences between the littoral and the deep zones of the lake is discussed in section 4.2, last paragraph “(1) the soil flooded 10 years before, clearly identified by the $\delta^{13}\text{C}$ and C/N composition of the core sampled in the littoral zone at station 3, but most probably also present in the deeper zone of the reservoir where sediment core could not be sampled”

Comment 4: 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? Reply: Biofilms are by far not the central part of our study. Based on field observations, two different kinds of biofilms are found at Petit Saut: epiphytic (on dead tree trunks) green biofilms in the euphotic area of the lake reservoir and epilithic (on rocks) red/brown biofilms in the river downstream of the dam. For our study of the whole Petit Saut system, we have sampled these two kind of biofilms, although we did not investigate their representativity and we don't know their contribution to the total biomass.

Comment 5: 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. Reply: same as for comments 1&2.

Comment 6: 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,

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with numerous grammatical errors. Reply: we have significantly modified our MS in order to improve the discussion section.

In detail: Comment 7: 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. Reply: the abstract has been totally rewritten in consequence.

Comment 8: 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. Reply: same as for comment 3.

Comment 9: 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. Reply: This section that started the discussion in the present version of the MS has been removed in the present version.

Comment 10: 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. Reply: fucoxanthin was found only at the surface sediment of stations 1 and 3, as well as in the epilithic biofilm downstream of the dam (station 5), but was NEVER found in the water column (except in the estuary). As already stated in the first version of the MS, microscopic observations by Vaquer et al. (1997) confirm the predominance of Chlorophyceae and the absence of diatoms in the water column of the Petit Saut lake. Furthermore, pelagic diatoms are unlikely to develop in quiet (low turbulent) water columns of oligotrophic reservoirs because they will sink (high density). Such environments have been reported to favour small Chlorophyceae (Reynolds, 1997) as was indeed confirmed by

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microscopic observations (Vaquer et al., 1997). Thus, pelagic diatoms is not a first good guess, although we agree with the reviewer that in principle it is possible to check trap samples for presence of diatom frustules by microscopy.

Comment 11: 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. Methylophilic bacteria can indeed explain the lower d13C, but one would expect a minimum in d13C right at the oxycline. Here methylophilic biomass should peak because bacteria have excess to both O₂ from above and methane from below the oxycline. Is there a biomarker for methylophilic bacteria? Hopanoids? Reply: we have completely re-written the last paragraph of section 4.1 in order to satisfy to these comments. We are confident with our interpretation that methanotrophs and chlorobiaceae outcompete for the d13C signature of POM. We would like to point out that Petit Saut is precisely a system where the concept of “regular” phytoplankton cannot be applied, because physico-chemical conditions (acid, low alkalinity waters with an anoxic, methane rich hypolimnion, etc) are by far not “regular” and the planktonic microbial population reflects this non regular character. Indeed, isotopic data alone and interpreted superficially, could have led to the conclusion of “regular” phytoplankton. However, when combined with pigment data, isotopic data can be interpreted in a very different way and this is where most of the originality of our paper comes from, at least for the water column part. The phospholipids fatty acid 18:1n-7 is a specific biomarker for the type II methanotrophs; they have been measured by Dumestre et al. (2001) and we refer to this study in our revised MS.

Comment 12a: 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. Comment 12b: 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

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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? Reply: First of all, the OC/pigment ratio listed in Table 1 actually corresponds to OC content divided by the sum of chlorophylls (Chla + Chlb + BChlc's + BChld's) and we have now corrected this ratio description in the revised Table. In the epilimnion (3 m depth) this ratio can be compared to typical OC/Chla ratio's for phytoplankton, while in the hypolimnion and at the oxic anoxic interface this approach allows us to consider the impact of anoxygenic phototrophs on OC/chlorophyll ratios (see also reply to comments by reviewers 3 and 4). The two comments (12a and 12b) are linked, since OC/(sum of chlorophylls) and C/N ratio are affected the same way by the OM source. We have suggested that extracellular polymers may explain the high high C/N and OC/(sum of chlorophylls) ratio at 3 meters depth. While this suggestion is supported by the description of slime surrounding Chlorophyceae cells and by what can be expected in environments characterised by high light and low nutrients (oligotrophic) we agree that the indication for TEP is still circumstantial and not sufficiently documented in our study. We have modified the text in consequence. In the last paragraph of section 4.1, we have removed most of the consideration about TEP as they were found too speculative by all 3 reviewers. We find, however, that the idea of a terrestrial carbon source at the center of the lake is even more speculative. How could this source affect the water column only at the 3 meter depth and not below? At 6 and 7 meter depths, among other tracers, the OC/(sum of chlorophylls) (mostly influenced by BChl c&d) is 8 to 16 and indicates that there is no room for a contribution of terrestrial material. At 3 meter, the material collected on the filters was very homogeneous, white-colored and with a creamy texture and no observable trace of plant debris. We are conscious such empirical argumentation is insufficient but we still believe in the presence of phytoplanktonic exudates that increase the C/N ratio at this particular depth (again, Petit Saut is by far not a "regular" system). Because further investigation is needed to conclude on this particular point, we leave the question open in the revised version of the MS (last paragraph of section 4.1): "In parallel, the mechanism leading to such

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a high C/N ratio in planktonic material is unknown. Exudation and coagulation of exopolimeric substances enriched in carbon compared to nitrogen, could be one process, similarly to what has been shown for transparent exopolymeric particules (TEP) in the ocean (Mari et al., 2001). If such process occurs, it could also explain the relatively high OC/pigment ratio found at this depth. This needs further investigation based on microscopic observations and elemental analysis on more samples.”.

Comment 13: 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”? Reply: in the revised MS (section 4.2) we use the term “settling material”, as suggested by the reviewer.

Comment 14: 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 $\delta^{13}\text{C}$ in trap material and in sediments, and to infer preferential sedimentation or degradation as plausible explanations for the observed difference in $\delta^{13}\text{C}$. Reply: we agree with this comment. In the present version, we clearly state this fact: “The material settled at the lake bottom at station 4 and collected with the peristaltic pump (called surface sediment) followed the same trend as the trap material in the $\delta^{13}\text{C}$ -C/N diagram (Figure 8), but contained very little pigments; this material probably integrates a longer period of sedimentation than the traps material, which can lead to an almost complete degradation of pigments, beside a C/N and isotopic signature close to the trap material”. Again, pigments in the traps unequivocally demonstrate the predominance of the planktonic source, whereas C/N and $\delta^{13}\text{C}$ alone would not. Our approach here is thus to discuss how we can explain the isotopic signal, owing to what we show from pigment data.

Comment 15: 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

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water column? Also, I would imagine that OM in biofilms is rather immobile. Reply: As stated in the first version of the MS, Scytonemin was found only in the epiphytic biofilm and in the 3 traps. Never in the water column, where the plankton is dominated by Chlorobiaceae and Chlorophyceae. As also noted by Rev 4, a mechanism for how biofilm enter the traps is proposed in the revised MS (same section as above). Please note that biofilms are generally exposed to sloughing and that parts of biofilm may thus detach and become transported in the water column where these may sediment and be collected in traps. In addition, scytonemin is a pigment that is typically extracellular and included in the sheaths of cyanobacteria, which are characteristic for benthic species. Moreover, cyanobacteria were not detected in the water column and the text has now been adapted: M/S page 20: “As is typical for tropical aquatic environments, the relatively stable (low turbulence) oligotrophic and acidic (pH 5-6) epilimnetic water column is characterized by an assemblage of small Chlorophyceae and is not favorable for the development of pelagic diatom or cyanobacterial communities (Reynolds, 1997). “

Comment 16: 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? Reply: change in aquatic productivity has likely no effect at shallow, littoral station. In the revised version of the MS, we evoke the possibility for a modification in the isotopic signal due to diagenesis: “Although the high concentrations of pheophytin a and b at 6cm depth in the core reveals an intense degradation of this material, its C/N and isotopic signatures remain close to those in the adjacent forest soil”

Comment 17: p.1177, l. 26: “result of complex biological and chemical mechanism” can mean everything. Be more specific. Reply: the two major mechanisms are now specified in the revised MS, although there is still some controversy in the literature on which predominates.

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Comment 18: Section 4.3, first paragraph: Is there any other evidence than the low $\delta^{13}\text{C}$ 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. Reply: no biomarker analysis (eg phospholipids fatty acids) are available for this biofilm. However, the fact that this biofilm is located in an area where methane oxidation is extremely intense is known for a long time (eg Abril et al. 2005), also, we have added in the revised manuscript that “the ability of methanotrophs to grow in photosynthetic biofilms is known for a long time (Arcangeli and Arvin 1997).”

Comment 19: 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. Reply: Today, we don't have enough data to assess a complete organic carbon budget of the Petit Saut system. In particular, the input term is poorly quantified so we don't know if the reservoir is a sink or a source of POM. What we have done here is only comparing the order of magnitude of two organic fluxes (settling in the lake and passing through the turbines) quantified in the present study for the sampled period, to CO_2 fluxes measured simultaneously. Because of all the reviewers' remarks concerning the temporal representativity of our samples, we find more logical not to go further in such mass balance budget.

Comment 20: 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, they have barely been investigated and microscopic and geochemical evidence elucidating their existence and mechanisms that

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lead to the accumulation of TEP does not exist or is rather vague. Reply: considerations about TEP have been removed from the conclusion, which has been re-written.

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" Reply: all these minor points have been taken into account in the revised MS.

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