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Biogeosciences Discussions

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 #3

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This paper is an interesting preliminary study on the origin of the organic carbon, its incorporation into the sediment and transfer along a tropical stream and reservoir. It is, therefore, an important contribution to the general understanding on sources and sinks of atmospheric CO_2. The authors attempted to identify i) the major sources in the distinct river and reservoir compartments, ii) the biogeochemical processes occurring therein, and iii) the vertical (water to sediment) and horizontal (upstream to downstream) fluxes. A well-suited multi-proxy approach was used including carbon isotope composition, C/N ratios and photosynthetic pigments that shows clearly the advantage of combining these biogeochemical markers.

The study was well-done and basically well-written. However, the authors should be aware about the limits of their study (e.g., one month, one year, one season, one site for each compartment, sediment cores from another site than water samples and sediment traps, mean values of duplicates ...) and draw conclusions much more carefully. The manuscript length can be reduced substantially if long speculations are avoided. Some suggestions are given below.

Unfortunately, there is no information on the error of the method or about the variance between the duplicates. Hence, one cannot estimate if the difference between mean values of distinct sample sites is greater than the difference between duplicates, and if variability is not just due to the standard error of the method.

Specific comments:

1) Abstract

A substantial reduction is required (about $\frac{1}{2}$ for the Abstract and a sentence on main conclusions should be given (e.g., general picture of OM cycling in the reservoir).

2) Introduction

The Introduction is well written and sets the scene well with a good summary of the state-of-the-art. The Introduction also clearly provides the issue of the study.

Minor concerns:

- p. 1162 par.1: Details, on what information the carbon isotopic composition may provide, could be reduced because most Biogeoscience readers will be familiar with such studies.
- I miss a sentence why this particular reservoir has been studied and why it is thought to be representative.

3) Material and Methods

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The Materials and Methods were generally well suited and well described. Nonetheless, the selection of some sampling sites and extrapolation of some facts remains unclear. For example: was the reservoir oxicline determined only at Station 4 and was this representative for the whole reservoir? And why the littoral zone was chosen for coring if its sedimentation is very different from the centre of the reservoir? And why no SPM (at least in surface water) was analysed at that site?

Minor concerns:

- Which ratio was used for C/N evaluation (weight or atomic)?
- p. 1165 l. 11: Details on water filtering is not needed here as details are given below
- p. 1167 l. 20-25: It is not necessary to give these equations as this is a standard procedure.
- p. 1168 l. 7-17: In my opinion, it would be sufficient for the present study to state that all bacteriochlorophyll allomers were summed. It's not the first study summarizing them. In contrast, it would be important to know if all chlorophyll-a allomers and chlorophyllides were summed in <Chl a> as well.

4) Results

The information in the Results is important but rather difficult to assess due to the huge amount of data the authors gathered. I would suggest shortening it considerably by removing most of the numbers and refer instead to the well prepared Table and Figures. As it stands it seems more like an enumeration of facts and numbers and that's a pity.

Minor concerns:

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- p. 1169 l. 17: What is the meaning of total pigment concentration? This is definitively not a measure for autotrophic biomass or productivity, particularly as the pigments used were obviously selected (cf. chromatogram in Fig. 2). I would prefer to have chlorophyll-a concentration as standard measure of autotrophic biomass and productivity instead of <total pigment>. The same is true in the Figures 3 and 5.

- All information given in Figure 4 is also given in Figure 3 (oxycline can be shown in Figure 3). Thus, Figure 4 could be deleted to shorten the manuscript.

5) Discussion

The Discussion is well organized, easy to read and of great interest. However, Results were presented "vertically" (Water – Traps – Sediment), but Discussion is presented "horizontally" (River – Reservoir – Estuary). Although both structures have their advantage, one structure should be kept throughout and follow the central thread. Parts of the Discussion and the Figures 8–10 could then easily be presented in the Results and would certainly make the Results easier to read and understand.

Minor concerns:

- p. 1172 par. 1: There is no need to introduce the Discussion by what will be done. That paragraph should be moved to the Introduction.

- p. 1173 l. 17 and l. 27: It is difficult to compare POC/Chl a ratios in the water column and in the sediment to prove terrestrial or lacustrine origin, because chlorophylls undergo much stronger degradations in the sediment than POC.

- p. 1173 l. 27: $< \Sigma$Chl$>$ should be changed to $< \Sigma$Chla$>$ to make clear that it is the sum of chlorophyll-a + pheophytin-a and not the sum of chlorophyll-a + chlorophyll-b.

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- 1173 l. 26-27: <few amounts> of fucoxanthin that reveals <a small contribution of diatoms> seems not to be justified as the concentration of fucoxanthin is as high as the concentration of lutein. What's about chlorophyll-c? Generally, fucoxanthin is a good marker for diatoms avoiding microscopic evaluation, but it is impossible to use fucoxanthin to discern pelagic from benthic communities.
- p. 1174 l. 1-9: The variation of phytoplanktonic δ13C has been explained in the Introduction and should not be repeated in the Discussion.
- p. 1174 l. 18-26: The information of the bacterial chlorophyll allomers that was not mentioned in the Results is of secondary interest for the purpose of this paper and can be omitted.
- p. 1175 par. 1: The discussion about the contribution of methanotrophic bacteria counterbalancing the isotopically heavier Chlorobioaceae is much too long and not fully supported by the presented data.
- p. 1175 l. 14–p. 1176 l. 6: The discussion about the impact of TEP to the unusual C/N ratio should be shortened considerably as it is highly uncertain and a single value that even might not be representative for the reservoir. Moreover, this ratio is a mean value of 2 samples and no information on the variance between these two samples is given. So a critical reader might suppose that the ratio in one sample was within the "normal" range while the ratio in the duplicate was very high leading to a very high mean ratio...
- p. 1177 l. 2: How can epiphytic scytonemin settle into the traps at 7 or 20 m water depth in a stable stratified lake?
- p. 1177 l. 5-12: The high β-carotene/Chl a ratio in the sedimentary material is certainly due to the much stronger degradation of chlorophyll-a than that of the relatively stable β-carotene even within a few weeks and not only on <geological

time scales>. Therefore, it would be more suitable to compare the β-carotene/ ΣChla (including the relatively stable pheophytin-a) ratios. However, the authors should be aware that β-carotene sometimes coelute with pheophytin-a and is then overestimated.

- p. 1179 l. 19-20: The statement that <the presence of lutein but absence of Chl b reveal the contribution of OM derived from partially degraded Chlorophyceae> because <in sediments Chl b is degraded much faster than lutein> is highly speculative, as there are also numerous studies showing that chlorophyll-b is more stable than lutein in other sedimentary environments and because the study of Bianchi et al. was not designed to show faster or slower degradation of chlorophyll-b and lutein. Also, it is unclear why the relatively stable pheophytin-b was not considered in this study. The chromatogram shown in Figure 2a indicates the presence of pheophytin-b.

- It would be of great interest in my opinion, to know, in which way the processes shown for this tropical reservoir vary or are supposed to vary from reservoirs in temperate or cold regions. Hence, if the conclusions and quantifications in Petit Saut can be applied to other reservoirs.

6) Conclusion

The Conclusions are concise and useful. However, due to the fact that only one month of one year and selected sites in a big complex system were studied, and this without statistical verification, the conclusions should be drawn much more carefully. Conclusions were expressed as facts that need some further improvements; however, in my opinion, conclusions should be presented as assumptions, although important ones, which need to be proven.

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