Biogeosciences Discussions, 2, S1002–S1010, 2005
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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.

A. de Junet et al.

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Reviewer 3

Comment 21: 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. Reply: same as for comment 1&2 by reviewer 2

Comment 22: 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. Reply: the reviewer refers to the unexpected C/N ratio of 21 at 3 meter depth in the water column. Standard deviation for this sample (triplicate analysis in this case) is less than 0.5.

Comment 23: 1) Abstract. A substantial reduction is required (about 1:2 for the Abstract and a sentence on main conclusions should be given (e.g., general picture of OM cycling in the reservoir). Reply: same as for comment 7 by reviewer 1.

Minor concerns: Comment 24: 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. Reply: we kept this information in the revised MS because it is not that long and because the information specific to bacterial biomass (methanotrophs and Chlorobiaceae) is not so classical and refeeing this information is essential in the context of this paper.

Comment 25: I miss a sentence why this particular reservoir has been studied and why it is thought to be representative. Reply: We actually have no definitive idea if Petit Saut is a reservoir representative for the whole tropics. What we know is that all tropical reservoirs have an anoxic and methane-rich hypolimnion like at Petit Saut. In the revised MS, we write: "This site represents the tropical reservoir that is best documented in terms of CO2 and CH4 emissions (Galy-Lacaux et al. 1999; Abril et al. 2005), primary production and phytoplankton communities (Vaquer et al. 1997) as well as bacterial communities (Dumestre et al. 1999; 2001). Previous studies and field observations suggest that many organic matter pools of different origins coexist in such system...". We hope this satisfies the reviewer's comment.

Comment 26: 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? Reply: the oxicline in the

Petit Saut lake is very stable, both spatially and seasonally. In the revised MS, we wrote that its position at 6 meter depth was "a classical situation all over the lake reservoir during the dry season (Richard 1996)"

Comment 27: 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? Reply: same as for comment 3 by reviewer 1. In addition, we did not sample the water column at this site because this sediment sampling aimed to describe the composition of the flooded soils.

Comment 28: Which ratio was used for C/N evaluation (weight or atomic)? Reply: weight (now stated in section 2.2.1 of the revised MS)

Comment 29:Ţ p. 1165 l. 11: Details on water filtering is not needed here as details are given below Reply: this has been removed, as suggested by reviewer 3

Comment 30: p. 1167 l. 20-25: It is not necessary to give these equations as this is a standard procedure. Reply: these equations have been removed, as suggested by reviewer 3

Comment 31: 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. Reply: It is not apropriate to compare the bacteriochlorophyll allomers with the allomers and epimers arising from chlorophyll a. The bacteriochlorophyll allomers correspond to a genuine mixture of natural compounds that have been well documented for Chlorobiaceae (where these compounds are functioning as light-harvesting pigments in the chlorosomes), while the chlorophyll a epimer and allomer eluting just before and after genuine chlorophyll a are artefacts arising during extraction in organic solvents and to our experience can also result from inappropriate conservation (several months at -18°C). The conservation and extraction procedures used for this study have been optimised to minimise the formation of the epimer and allomer (less than 3 % of chlorophyll a) and we have therefore not considered these. In contrast, chlorophyllide a is a degradation product (de-esterified, while maintaining Mg covalently bound in the tetrapyrole structure) arising in the environment through action of the enzym chlorophyllase. We have sometimes observed this in diatom rich communities, but it was not a significant product in these samples. Furthermore, it is not appropriate to sum this product with chlorophyll, while not considering the others (phaephytins and phaeophorbides), because chlorophillide a is not active in photosynthesis.

Comment 32: 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. Reply: we put some effort in shortening this result section by removing some of the numbers in the text. We did not however totally modify this section because, although it seems like an enumeration of facts and numbers, these facts and numbers must be given somewhere in the MS.

Minor concerns:

Comment 33: 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. Reply: The OC/pigment ratio listed in Table 1 actually corresponds to OC content divided by the sum of chlorophylls (Chla + Chlb + BChlc + BChld) 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 . We agree with the fact that in classical aquatic systems, Chl a concentration is used as proxy for autotrophic biomass. However, in the case of Petit Saut, a large part of the autotrophic biomass does not contain Chl a, as it is constituted by Chlorobiaceae.

Comment 34: 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. Reply: it is true that part of the information contained in fig3 is also shown in figure4. However, the information is very condensed in figure 3 and we feel more comfortable discussing the vertical distribution of parameters on figure 4. The MS has been substantially shortened by removing part of the text as suggested by all reviewers.

Comment 35: 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. Reply: we find logical to present the results “vertically” and, latter, to discuss their meaning “horizontally” and vertically because this is how POM flows through the system.

Minor concerns: Comment 36: 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. Reply: same as for comment 9 by Reviewer 2: This section has been removed in the present version.

Comment 37: 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. Reply: we agree with this comment. This has little implication on our conclusion since the high POC/Chl a ratio in the sediments is due to both processes: a high ratio in the source and an increase of the ratio with degradation. In the revised MS, we have added the following sentence “In addition, a faster degradation of Chl a than POC in the sediments can increase this ratio”.

Comment 38: p. 1173 l. 27: < _Chl> should be changed to < _Chla> to make clear that it is the sum of chlorophyll-a + pheophytin-a and not the sum of chlorophyll-a + chlorophyll-b. Reply: This ratio actually corresponded to OC content divided by the sum of chlorophylls (Chla + Chlb + BChlc’s + BChld’s) (see also comment 33)and we have now corrected this ratio description in the revised texte. We believe that it is more appropriate to consider for this case only the chlorophyll forms that represent potentially active cells rather than including all the degradation products. Thus it does not correspond to the sum of chlorophyll-a + pheophytin-a as the reviewer suggested.

Comment 39: 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. Reply: same as for comment 10 by reviewer 2.

Comment 40: p. 1174 l. 1-9: The variation of phytoplanktonic _13C has been explained in the Introduction and should not be repeated in the Discussion. Reply: this has been removed in the revised MS.

Comment 41: 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. Reply: this has been removed in the revised MS

Comment 42: 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. Reply: we partly disagree with this comment because the pigment data show a predominance of chlorobiaceae around the oxicline, which is not reflected by heavier d13C. This section has been rewritten,

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referring more precisely to a previous study by Dumestre et al. 2001.

Comment 43: 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. Reply: this section has been partly removed in the revised MS. At depth 3 meters, the standard deviation on three C/N analysis gave a standard deviation lower than 0.5.

Comment 44: p. 1177 l. 2: How can epiphytic scytonemin settle into the traps at 7 or 20 m water depth in a stable stratified lake? Reply: same as for comment 15 by reviewer 2

Comment 45: p. 1177 l. 5-12: The high _-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 _-carotene even within a few weeks and not only on <geological time scales>. Therefore, it would be more suitable to compare the _-carotene/ _Chla (including the relatively stable pheophytin-a) ratios. However, the authors should be aware that _-carotene sometimes coelute with pheophytin-a and is then overestimated. We agree with the reviewer that phaeophytin a may elute close to or even coelute with beta-carotene. However, on our system we achieve a separation of these compounds by about 0.5 min. In some samples with very high beta-carotene contents we have sometimes observed that the tail of the beta-carotene peak is sometimes contaminated with a minor proportion of pheaophytin a, which we then detect and correct for by careful spectral analyses at the different retention times. Nevertheless, this phenomenon was not important for these samples and we are confident of our quantifications.

Comment 46: 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. We agree with the reviewer that this issue is more complicated and full of nuances than described in the original text. The important point is that we detected neither chlorophyll b nor phaeophytin b in the surface sediments of station 5, 6 and 7, while we detected lutein at station 5 and particularly at station 6 (see Table 1). (Please note that the chromatogramm in Fig. 2 reflecting presence of Chlb and phaeob is actually station 1 and not downstream the reservoir). Thus, lutein, reflects an input from degraded matter most likely originated from the Chlorophyceae flushed out of the reservoir. We have deleted the controversial phrase and modified text as follows: “In addition, the presence of lutein but the absence of Chl b and Phaeo b reveal a contribution of degraded OM, most likely originating from the Chlorophyceae flushed out of the reservoir.”

Comment 47: 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. Reply: same as for comment 25 by reviewer 2. In addition, comparing this tropical system with all its particularities (acid waters, anoxic hypolimnion, light penetration below the oxicline, presence of pelagic chlorobiaceae, etcĚ) with other reservoirs worldwide is an almost infinite work and would anyway remain highly speculative.

Comment 48: 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. Reply: The conclusion has been almost totally rewritten.

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