

Interactive comment on “Dissolved organic carbon lability and stable isotope shifts during microbial decomposition in a tropical river system” by N. Geeraert et al.

N. Geeraert et al.

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We are grateful for the comments and feedback offered by Referee # 1. Below, we will reply to each of the comments, with the original comments prior to each response.

REF: The topic of this study fits well within the scope of BG and should be of interest for a broad range of readers. Its main strengths are the stable isotope approach used to assess the dynamics of biodegradation of riverine DOC, and the fact that the study was carried out on a river with mixed C3 and C4 sources of DOC. The data appears of excellent quality (S. Bouillon is a recognised specialist in the measurement of ^{13}C signature of DOC), but it would be more convincing if the standard deviations of the

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reported averages would be available in addition to the ranges of values (see specific comments below).

REPLY: We have added the standard deviation along with the average values and the range.

REF: On a less positive note, the quality of the writing should be improved a lot. I understand that English is not the mother tongue of the authors but in several places the text would gain in clarity if reviewed by someone fluent in English. The manuscript is also very short and reports a very small dataset – although I realize that collecting water samples in Kenya is not a simple task. The discussion and conclusions would also have gained from complementary analyses of the bulk chemical composition of the DOC before and after incubation to differentiate between C3 vs. C4 decomposition and differential biochemical decomposition (optical analysis, as proposed by the authors, or FTIR/NMR analysis on freeze-dried residues). While reporting a change in the $\delta^{13}\text{C}$ signature of DOC upon bacterial degradation is novel (most studies assume that biodegradation does not lead to such changes), understanding the reason why the signatures change would have been even more enlightening. I feel that an improved version of this manuscript would be worthy of publication in BG mostly because it would report for the first time (to the best of my knowledge) changes in $\delta^{13}\text{C}$ stable isotope signature upon microbial degradation of DOC originating from mixed C3 and C4 sources. I feel however that more DOC characterization work would have resulted a much stronger paper.

REPLY: These experiments were carried out as a side-project within a larger project to test whether it could explain some of the observations made during the first field campaign. As it was indeed very exploratory, we didn't prepare samples for complementary analyses. However, as a shift in stable C isotope ratios during bacterial degradation was not yet reported before, we found the results sufficiently exciting and novel to share them with the scientific community and to stimulate new ideas or research questions to those working with DOC mineralization.

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Specific Comments

REF: 1. Page 1, lines 21-24: The concluding sentence of the abstract should be reworked; the authors probably mean that the stable isotope signature of total DOC in rivers does not necessarily reflect the relative proportion of C4- and C3-derived DOC in the catchment. REPLY: This sentence has been reworked to make it more understandable.

REF 2. Page 4, line 2: Decomposition mechanisms were not determined in this work – only speculative hypotheses are provided in the discussion section. Reference to the mechanism should be removed since this is the paragraph that describes the work that was performed.

REPLY: We agree with this comment, and since our data do not allow to make supported statements on the mechanisms involved, we removed the reference to the decomposition mechanisms.

REF 3. Page 5, line 28 to page 6, line 2: More details should be given on the DOC-IRMS setup or a reference to published work should be provided.

REPLY: The operating principle of our TOC analyzer and a reference to the original paper where the setup was described (St-Jean, 2003) have been added to the text.

REF 4. Page 7, lines 1-7: The authors should provide a quantitative result for the differences between incubations with and without POC. What is the percent contribution of the POC bacterial pool to total degradation in each sample?

REPLY: We do not see how we should interpret this question.

REF 5. Page 7 line 10, line 11, line 20, line 21, line 23 and line 24 (and everywhere else in the text): Please provide the standard deviation whenever an average is given – giving a range of values is not sufficient.

REPLY: The standard deviations have been added whenever the average of a value is

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given.

REF 6. Page 7, lines 18-24: How do these degradation rates compare with literature values? The authors cite several studies reporting such rates in their introduction.

REPLY: Although not many of the studies reported the exact mineralization rates, we could compare our rates with those of Amon and Benner (1996) which also had similar initial DOC concentrations. The experiments of Moody et al. (2013) started and ended with much higher DOC concentrations, but showed a similar trend as our observations: most of the degradation occurred within 2 days. Those two examples are added with quantitative information.

REF 7. Page 8, line 10 and line 14: Please provide the significance level for the statistical test used here.

REPLY: The significance level for the test was added.

REF 8. Page 8, line 22: What statistical test was carried out to decide whether these two values are outliers? Please explain.

REPLY: While it was based on visual interpretation and the strong improvement in R^2 , the calculation of Cook's distance indicated that only one of them had a distinctively large impact on the regression equation. However, based on the comments of another reviewer, we have decided to use a robust linear regression which is less affected by the outlier.

REF 9. Page 8, lines 27-27: Again, please provide the standard deviation for these averages. Are the differences between these averages significant?

REPLY: We added the standard deviation of the averages and the test (with significance level) which indeed indicated that the three of them are significantly different.

REF 10. Page 9, lines 4-5: Please provide a reference for the heavier $d^{13}C$ signature of carbohydrates.

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REPLY: The reference to Benner et al. (1987) has been added.

REF 11. Page 9, line 24, to page 10, line 10: An alternative reason for the similar reactivity between the upstream and downstream sites could be the photo-activation of a fraction of the non-labile DOC pool (photocleavage of large biochemical into smaller, more bioavailable components. This possibility should be added.

REPLY: We had indeed neglected to mention this option. It has been added in the revised version of our manuscript. In the further discussion of this DOC source, we do argue that this may be unlikely to be an important mechanism due to the limited light penetration depth (high sediment load). Nevertheless, it indeed deserves to be mentioned and explored further.

REF 12. Pages 14-15, Table 1: The column titles should be reformatted.

REPLY: The column headings are well formatted in the online document based on the LaTeX file.

References Amon, R. and Benner, R.: Photochemical and microbial consumption of dissolved organic carbon and dissolved oxygen in the Amazon River system, *Geochim. Cosmochim. Ac.*, 60, 1783–1792, 1996. Benner, R., Fogel, M. L., Sprague, E. K., and Hodson, R. E.: Depletion of ^{13}C in lignin and its implications for stable carbon isotope studies, *Nature*, 329, 708–710, 1987. Moody, C. S., Worrall, F., Evans, C. D., and Jones, T. G.: The rate of loss of dissolved organic carbon (DOC) through a catchment, *J. Hydrol.*, 492, 139–150, 2013. St-Jean, G: Automated quantitative and isotopic (^{13}C) analysis of dissolved inorganic carbon and dissolved organic carbon in continuous-flow using a total organic carbon analyser. *Rapid. Commun. Mass Spectrom.*, 17, 419–428, 2003.

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