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Biogeosciences Discussions

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

Anonymous Referee #2

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Review of "Dissolved organic carbon lability and stable isotope shifts during microbial decomposition in a tropical river system". Please note that to allow an unbiased evaluation, I have performed this review without looking at the other previously published reviewer comments.

Summary: The study presents an evaluation of changes of dissolved organic carbon concentrations and their associated d13C signatures in dark incubations of stream water from the Tana River, Kenya. The results show substantial decreases in [DOC] within the first 24 to 48 hours, suggesting strong microbial activity with up to 50% of the DOC being respired. These changes are paralleled by changes in d13C signatures

towards heavier isotopes in the remaining aquatic solution. The authors then discuss potential links between the differences in C3 and C4 landcover data and the derived results for stream DOC.

The manuscript is overall well written and referenced. It lays within the focus of the journal Biogeosciences and will likely be of interest for the readership. Overall my evaluation results in the support of the manuscript with the recommendation to consider the manuscript for publication after the comments below have been addressed.

Major comments and remarks:

- At current, I found part of the results and discussion section ('removal mechanism and origin of DOC') not to be well based on the data provided. This concerns primarily the discussion of the C3/C4 vegetation differences and how these would affect the lability of DOC. I believe that the authors need to provide more evidence here, or to cut back on their interpretation and conclusion. In this context, I wonder if there is at all any data that objectively suggests any type of relationship between stream dDOC, d13C and the C3/C4 landcover data?

- Statistical methods are explained within the combined results and discussion section. I strongly suggest describing these in the methods section. At current, there are results of the stats presented, without the reader knowing what stats methods were actually used.

- I generally prefer to have separate results and discussion sections. However, I acknowledge that this may only be my personal preference and the authors have prepared the manuscript now in the given format. Therefore, I will leave it up to the editor to decide, whether separating results and discussion is feasible and will increase the quality of the manuscript.

Minor comments:

Introduction: I found the introduction to be very well written. However, there was one

detail:

P12764, L4: I was a surprised that it is stated that 'microbial consumption can take place in the entire water column'. Whereas this statement is, as such, true, there is no mentioning of the important role of the benthic system, which may also host microbial biofilms that can greatly enhance heterotroph activity. I suggest to add a sentence or two on this topic. A reference could be (Battin et al. 2003), but there are other good ones as well.

Methods:

See previous comment on statistical methods. Otherwise they read okej.

P12765, L14-18. It may help the reader to understand, which parts of the catchment can be considered humid and which arid (or semi-arid). This aspect may be also important for the question of how the landscape contributes to stream DOC.

L21: interesting approach this mixing model for the landscape C3/C4 proportions. Maybe it would help to guide the reader to why the authors apply this model. A sentence like the following could be added: "To investigate the possible effect of vegetation cover on DOC isotopic composition. . . we estimated C3/C4 vegetation coverage". However, before writing this, the authors may need to clarify the necessity of this vegetation cover data for the study for themselves.

L25: Interestingly these d13C numbers are pretty close to those named as 'typical numbers' for d13C numbers of CO2 in soil (-23‰ and -9‰ for C3 and C4 plants, respectively) named by Clark and Fritz (1997). May be worth to note this somewhere here.

Results&Discussion:

P12770, L5: please reconsider the presentation of statistical results. Were all assumptions for a t-test (normality, homoscedasticity) met here?

L25: as not all readers may be so familiar with the selective photochemical oxidation, I suggest adding the reference that the authors cite in the introduction.

P12771, L2: The idea of selective decomposition is truly interesting. Maybe the statement that 'isotopically heavier carbohydrates were preferentially decomposed' could be evaluated and discussed a bit more. Also, it is unclear to me, based on which data the authors come to this conclusion. Please provide more detail.

In some older literature one can read that 'bacteria prefer to metabolize the isotopically light organics and oxidizers [. . .]' (Clark and Fritz 1997), as it is easier to break 12C-H bonds than 13C-H (or C-2H). This is generally assumed to cause the opposite effect as the one described above. So here is truly an interesting aspect to explore. But first, the reader needs some more evidence for a relationship of a DOC source and the C3/C4 story.

P12772, L1: Even if I have not been to the Tana River, I am not sure these are all the potential sources of DOC to this system. You may also consider i) additions of leaf litter from riparian vegetation that can enhance POC, but also DOC for example through leaching or ii) any human activities, such as sewer inflows that may also contain organic matter. On the contrary, groundwater appears to me like an unlikely source of DOC to the river, as this is commonly considered to be low or very low in DOC, but often high in pCO2. Also, this point comes back to my first main comment.

Figure 2 and 3: They appear a bit redundant, as they show almost the same thing. I wonder if these could be combined or if one of them could be removed(?).

Figure 4: First part of caption reads strange. It's the percentage of change of the initial. . .

Figure 5 and associated results (p12770, L12-16): I believe this is a typical example, where the use of a simple regression based on least squares fitting is not a good choice. The authors acknowledge this, as they present two such regression models.

However, two regressions don't make much sense here. Instead the authors should reconsider their approach and use one of the commonly used 'robust regressions' to account for the two possible outliers.

References:

Battin, T. J., L. A. Kaplan, J. Denis Newbold, and C. M. E. Hansen. 2003. Contributions of microbial biofilms to ecosystem processes in stream mesocosms. Nature 426: 439-442.

Clark, I. D., and P. Fritz. 1997. Environmetal Isotopes in Hydrogeology. Lewis Publishers.

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