

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

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The paper by Geeraert et al. sets out to determine the degradation processes of OC in the Tana River (Kenya), focusing on microbial decomposition. Microbial degradation of DOC was measured in dark incubation experiments, eliminating loss of DOC through photochemical oxidation. It was concluded that bacterial mineralization of labile DOM was significant, contrary to the findings of other studies. The authors restricted this conclusion to mixed C3/C4 environments. The authors further concludes that POC is not a significant source of DOC in the river water.

General comments

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After reading the article I find that the conclusion concerning the significance of microbial degradation in removal of DOC drawn by the authors is supported by their data. However, I find the deduction of POC as a non-significant source of DOC in river water questionable. The authors measured initial fast degradation rates of DOC, and in unfiltered and filtered samples the observed concentration changes were relatively comparable with some differences. However, the concentration representing what the authors refer to as “recalcitrant DOC” is in most cases reached at (before) the second sampling point in the incubation series for both filtered and unfiltered samples. Thus, a possibility could be that any labile DOC released by POC could have been degraded before the second sampling of the incubation series. This would imply a too coarse temporal sampling resolution. Overall, I think that the text have to be developed in order to clarify and strengthen arguments of the article. The authors consistently use relative descriptions when describing observed differences in their experiments (e.g. “slightly more depleted”, “slightly enhanced”, “relatively minor”, etc.). I would suggest that these descriptions is reworked and replaced with numerical measurements. For example (P12768, line 10-12), instead of writing “slightly more depleted” and later report the average difference (0.3 ‰, I would suggest the single use of the latter.

Specific comments

P12762, line 20-23, I suggest a reformulation as “Indeed, only 0.9 PgCyr⁻¹ of the global estimates 1.9 PgCyr⁻¹ (Cole et al., 2007; Regnier et al. 2013) to 2.7 PgCyr⁻¹ (Battin et al., 2009) is delivered to the ocean (Cole et al., 2007; Battin et al., 2009; Regnier et al., 2013).”

P12763, line 16-18; the enrichment of ¹³C of the remaining DOC pool would only occur if the proportion of lignin in the DOC pool decreases, in which case the lignin must be decomposed at a higher rate/preferentially compared to the remaining constituents of the DOC pool (and not simply due to the decomposition of lignin). This is partly inferred from P12763, line 14-15, but should be remarked.

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P12763, line 28, reformulate “broke down” (e.g. degraded)

P12764, line 3, “However” does not fit into the context. I suggest that the authors remove however and introduce a line break.

P12764, line 7, reformulate “.., while it is only, ..” as “while only”

P12764, line 8, introduce a line break.

P12764, line 28; what was time between each of the three campaigns? How were they distributed during the wet seasons in May-June (2013) and in April-May (2014)? From table 1 I see that there is approximately 2 weeks between each sampling date, this should be clarified in the text.

P12765, line 21-25, long sentence; line 25, reformulate “. . . $\delta^{13}\text{C}$ of respectively -27 and -12‰” as “ $\delta^{13}\text{C}$ of -27 and -12‰ respectively”

P12766, line 2-4, reformulate.

P12766, line 5-8, I don't find it relevant to mention maximum discharge as what the authors are implying with this is that there was flooding in 2013 (?). Maybe this can be brought up later in the text when sources of DOC are discussed (P12772, line 4-9). How were the sampling campaigns distributed in time in relation to the flooding?

P12766, line 14-16, I don't know how well H_3PO_4 works as a preservative, but analysis within 4 months of sampling seems quite long. What temperature was the samples stored in?

P12766, line 26-27; were the incubation bottles stirred during the experiment?

P12767, line 25; How did the authors calculate the relative error? Why did the authors choose 50% relative error as a “reason to exclude”? Looking at the supplementary data, I think that more could be said concerning why the slow mineralization rates were measured. The initial DOC concentrations are close to what the authors describe as “recalcitrant DOC”, wherefore the degradable DOC would have been minimum in those

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samples and therefore a slow mineralization rate is calculated.

P12768, line 1, how many series in total were retained?

P12768, line 4, some kind of introduction to the results and discussion section must be given. This is partly due to the first line in section 3.1 (P12768, line 6) where the authors make an immediate distinction between (1) incubation series with relatively limited decomposition of DOC, and (presumably, 2) incubation series with relatively high decomposition of DOC referred to as “all other cases”. What is a relatively limited decomposition of DOC? Which are the all other cases? This should be clarified

P12768, line 8, insert “, there” (“In all other cases, there was a significant. . .”)

P12768, line 9-10, reformulate, e.g. “. . . the final concentration of DOC was systematically ~10% higher in the samples without POC”

P12768, line 14-15, refer to the table/figure where the reader can find the mineralization rates. P12768, line 17, what is meant by “relatively minor”?

P12768, section 3.1; The authors investigated POC as a potential source of DOC in the river, and found that there was a “. . . significant difference between the filtered and unfiltered incubation series, whereby the final concentration of DOC was systematically higher in the samples without POC by ca. 10%.” (P12768, line 8-10). Later on, the authors state that “. . . the unfiltered incubation series is treated as equivalent to the filtered ones for the remainder of the discussion” (P12768, line 18-19) due to a “relatively minor” enhancement of mineralization rates in POC samples (P12768, line 17). I find the use of words contradictory.

P12769, line 5-6 reformulate “This limited decrease in concentration can be related to the low initial concentration which was for all those samples below 2 mg L⁻¹. . .” as “This limited decrease in concentration can be related to the low initial concentration (<2 mg L⁻¹). . .”

P12769, line 7-8, in which series was mineralization observable? (insert reference)

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P12769, line 14-16, the calculated rates of decay of DOC should be “per day” (day⁻¹)

P12769, line 16-20, when comparing the results to the results from Moody et al. 2013, I think it is better to compare absolute instead of relative (percentage) concentration changes. This will be more interesting, and will in part justify the authors claim that DOC < 2 mg L⁻¹ is recalcitrant (if Moody et al. 2013 have similar values).

P12770, line 10. What is meant by a stronger reduction? Is it enhanced decay rates or greater absolute degradation? Reformulate “stronger”. (Same at line 14)

P12770, line 21, reformulate “. . . of -21.2, -23.1, and -24.3‰ for the mineralized, initial and remaining carbon pools for all the observations.” as “. . . of -21.2, -23.1, and -24.3‰ respectively.”

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