

Biogeosciences Discuss., author comment AC1
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Reply on RC1

Thibault Lambert et al.

Author comment on "Enhanced bioavailability of dissolved organic matter (DOM) in human-disturbed streams in Alpine fluvial networks" by Thibault Lambert et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-131-AC1>, 2021

RC1: 'Comment on bg-2021-131', Anonymous Referee #1

The paper is organized and well-written, with a clear analysis of the data. Overall, the findings agree with the literature and advance our understanding of the impacts of land use on DOM composition and lability in watersheds.

Reply *We thank the reviewer for her/his positive evaluation of the manuscript.*

My only concern is that "terrestrial" PARAFAC components are stable, which led the authors to suggest that terrestrial material is not being significantly consumed/altere (e.g., lines 407-412 of the manuscript). These components can be produced by microbes, so using these as an exclusive metric of whether terrestrial material is being consumed isn't ideal. At the least, the authors should consider literature showing alteration of terrestrial material by microbes and "maintenance" of these PARAFAC components by microbes, e.g., microbes contributing to these components so that net change is zero, while real change is happening. Due to the diversity of compounds contributing to a given optical signature, a more detailed molecular analysis would be needed to more definitively state whether the terrestrial material is being consumed or not. Thus, I would emphasize more caution when noting the degree to which terrestrial material is being altered and how that relates to land use. These are interesting and encouraging results that help illustrate these dynamics across watersheds but the full extent of terrestrial DOM change isn't fully captured with the data in hand.

Reply *Although reviewer#1 points to a relevant limitation of PARAFAC that we underestimated in the study, we would like to emphasize that we don't suggest that terrestrial DOM is not significantly altered in our study sites. Indeed, LTRC pools were related to the degradation of terrestrial DOM (lines 376-378) and was similar in size to STRC pools. Yet, LTRC pools did not vary across land uses (Figure 5). That being said, we were unable to link LTRC to changes in DOM composition as the terrestrial PARAFAC components remained stable during experiments (lines 372-375), leading us to suggest that aromatic molecules were not degraded by bacterial communities (lines 378-379). As pointed by the reviewer#1, this lack of variation does not necessary mean that terrestrial PARAFAC components were not altered, as bacterial communities are able to both consume and produce humic-like components commonly associated with terrestrial DOM. Thus, we agree that we cannot exclude some alteration of aromatic molecules during degradation processes that would have not been captured by optical measurements.*

In the discussion lines 407-412, we suggested that only a minor fraction of terrestrial DOM is degraded in inland waters. However, as pointed by a comment of the reviewer#1 below, our conclusion was exaggerated as we collected samples on a limited spatial coverage. A more detailed answer is provided to this specific comment below.

To address the first comment, we suggest modifying the lines 370-380:

"On the contrary, the similar sizes of LTRC pools between agro-urban and forest-grassland streams (Figure 5D) suggest that the bacterial degradation of terrestrial DOM was not influenced by human land uses despite higher content in inorganic nutrients, higher bacterial activity, and freshly produced autochthonous DOM in agro-urban streams. LTRC was indeed positively correlated with initial F_{Max} values of C1-C4 components but not with the protein-like components (data not shown), implying that this C originated from terrestrial inputs. However, terrestrial (C2-C4) and photoproducted (C1) components showed no significant trends during incubations (Figure 6) despite the ability of bacterial communities to degrade complex aromatic molecules (Catalán et al., 2017; Fasching et al., 2014; Logue et al., 2016), While the stability of the C1 component during bioassays is consistent with findings that photoproducted molecules are resistant to further bacterial degradation (Tranvik et al., 2001), the lack of variation of terrestrial components may result from a net balance between bacterial consumption and production of molecules contributing to the humic-like signature (Amaral et al., 2016; Guillemette and del Giorgio, 2012). It is therefore possible that alteration in the composition of terrestrial DOM upon bacterial activity may have not been captured by optical measurements. Addressing this point would require the characterization of DOM at the molecular level (e.g., Kim et al., 2006)."

Some of the specific comments refer to personification of materials, e.g. "whose" when referring to DOM or "their" when referring to carbon.

Reply *We will pay attention to this.*

Specific Comments

Line 21: "whose the size increased with human disturbance"

I suggest changing this to "with relative contribution to the total DOM pool increasing with human disturbance."

Reply *Done.*

Line 29: "determine" should be "determined"

Reply *Done.*

Line 35: "their travel"

I suggest changing this to "transit"

Reply *Done.*

Line 49: "proportion" should be "proportions"

Reply *Done.*

Line 60: "amount" should be "amounts"

Reply Done.

Line 72: "results" should be "result"

Reply Done.

Line 106: Remove "the" before Lake Geneva

Reply Done.

Line 115: "forests" should be "trees"

Reply Done.

Line 169: "weights" should be "weight" in the sub-title

Reply Done.

Line 223: May be better to refer to the Fluorolog-3 as a spectrofluorometer.

Reply Done.

Line 233: "prior the" should be "prior to the"

Reply Done.

Line 234: "A eight components" should be "An eight component"

Reply Done.

Line 285: "component" should be "components"

Reply Done.

Line 340: "recently DOM produced" should be "DOM recently produced"

Reply Done.

Line 344: I suggest citing Harfmann et al. 2019 JGR: Biogeosciences here to support this observation

Reply This reference will be added.

Line 349: "investigate deeper" should be "more deeply investigate"

Reply Done.

Line 350: "point" should be "suggest"

Reply Done.

Line 351: "amount" should be "amounts"

Reply Done.

Line 368: "another" should be "additional"

Reply Done.

Line 371: “despites” should be “despite”

Reply Done.

Line 375: The similar size of STRC and LTRC pools is intriguing – is there any evidence that the LTRC pool is related to the STRC?

Reply *A similar size of STRC and LTRC was also reported in Soares et al. 2019 (note that in their paper the LTRC corresponded to a medium-term bio-reactive DOC, defined as the amount of DOC lost between day 7 to day 23). Both STRC and LTRC pools are significantly correlated with DOC concentrations, leading to a positive but weak relationship between STRC and LTRC pools. Moreover, STRC related to protein-like components while LTRC related only to terrestrial humic-like components and there was no relationship between the decay constant k and LTRC. Therefore, we think that the positive relationship between the STRC and LTRC pools is not causal. It may simply reflect higher amount of bioavailable DOM with increasing DOC concentrations due to human disturbance that affects both primary production and export of terrestrial material (lines 349-353), and imply that each pool is driven by specific drivers. This lack of relationship between STRC and LTRC has been previously reported in other freshwater ecosystems (see suggestions for modification below).*

We suggest modifying the manuscript as follow;

In the result section 3.2, line 311:

“Both STRC and LTRC pools were positively correlated with DOC concentrations (Pearson $r = 0.79$, $p < 0.0001$ and Pearson $r = 0.68$, $p = 0.0013$, respectively), leading to a positive but weak relationship between the STRC and LTRC pools (Person $r = 0.49$, $p = 0.03$). Additionally, STRC was related to protein-like components. IN contrast, LTRC was related only to terrestrial humic-like components and there was no relationship between the decay constant k and LTRC.”

And in the discussion, at the end of the section 4.1:

“Similarly to what has been reported in Swedish inland waters (Soares et al., 2019), STRC and LTRC pools were similar in size although the total amount of BDOM was higher in Alpine freshwaters compared to DOM-rich waters of Sweden. Despite the fact that both pools increased with increasing DOC concentrations, we did not find evidence of interaction between STRC and LTRC. The positive relationship between STRC and LTRC likely reflected a greater amount of bioavailable DOM as human disturbance increased, as the latter enhanced both primary production and terrestrial export. Moreover, there was no relationship between the proportion of C consumed over short- and long-terms, and each pool related to specific DOM fractions. Similar observations were reported in Swedish rivers (Soares et al., 2019), in southern Québec (Guillemette and del Giorgio, 2011), or also in the Hudson River (del Giorgio and Pace, 2008). Overall, our findings are in good agreement with the idea that STRC is sustained by phytoplankton growth, whereas the consumption of DOC at longer timescale is rather related to terrestrial inputs of DOM.”

Soares et al. 2019 Scientific Reports noted the role of residence time in long-term bioavailability. The discussion could be expanded a bit to consider this with the current dataset.

Reply *Unfortunately, we don't have estimation of water residence time (WRT) in our study sites, nor the hydrological data required to calculate it. Moreover, given the small*

size and limited range of variation of drainage areas in our study, it is unlikely that we could find relationship between STRC and/or LTRC and WRT as in Soares et al. 2019 where WRT ranges from < 1 day to 100 days (drainage areas from 450 to 47000 km²). We agree with the reviewer#1 that the role of WRT in controlling the different facet of BDOM should deserve more investigation, but this requires to collect samples at a larger scale than the one of our study.

Line 374: "amount of DOC was" should be "amounts of DOC were"

Reply Done.

Line 405: "closed" should be "close"

Reply Done.

Lines 407-412: I'm not sure I agree with this. It stands to reason that inland waters are well-suited to degrade terrestrial DOM, as that is a primary input to these systems and heterotrophic bacteria have arguably adapted to utilize this carbon source.

Reply We recognized that our conclusions are exaggerated here, for several reasons. First, our sampling sites encompass mainly small streams with short WTR. Thus, our estimation of 20% of terrestrial DOM consumed is only applicable in the upper part of the basin upstream Lake Geneva and should be not extrapolated to a larger scale. Second, as mentioned by reviewer#2, we didn't include in our experiments the effect of light and performed our campaigns during the wet season only. Therefore, given the limited spatial extent and the experimental setup, we cannot guarantee our estimation to be conservative in space and time. A proper estimation of DOC loss in the basin would have required more work/other approaches and was beyond the scope of the study. In order to take into consideration this comment and other from reviewer#2, we suggest removing the lines 404-412 from the discussion and modifying the paragraph as follow:

"Our study further shows that the bacterial degradation of terrestrial DOM was not related to land use. Moreover, the lack of relationship between LTRC and BR falls in line with experimental studies showing that, in presence of algal derived DOM, terrestrial DOM is preferentially incorporated into bacterial biomass while autochthonous DOM is allocated to respiration (Guillemette et al., 2016)."

Line 408: "timescale" should be "timescales"

Reply Done.

Line 409: "entered in the lake Geneva" should be "in Lake Geneva"

Reply Done.

Line 409: Remove "the" before water residence time

Reply Done.

Line 418: Add "the" before fluvial continuum

Reply Done.

Line 425: I would reference the findings from line 344 and citation mentioned there; this finding is largely dependent on optics which always benefits from links to other literature.

Reply Done. Although we agree with the reviewer that PARAFAC components are often classified according to previous work, a bacterial origin for the C5 component is supported here by the fact that this compound shows an increase in fluorescence intensity during incubation.

Line 429: "amount" should be "amounts"

Reply Done.

Line 439: "effect" should be "effects"

Reply Done.

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