Dear Dr. Abril, dear reviewers,

thank you for your effort and your constructive reviews. These suggestions helped to significantly improve the manuscript in terms of clarity and compatibility with the readers of Biogeosciences. We changed and adapted large parts, which are described in detail in the point-wise answers to the reviewers' comments below.

Line numbers refer to the submitted manuscript with tracked changes.

Reviewer 1

General Comments: The manuscript titled "Complex interactions of in-stream DOM and nutrient spiralling unravelled by Bayesian regression analysis" compares instream uptake of DOM differing in source/quality which has been relatively understudied in the literature. The authors pose an interesting research question well within the scope of biogeochemistry. A secondary objective of the manuscript was to refine a statistical model to estimate nutrient uptake that can provide estimates of uncertainty, account for nonlinearity, and allow for the addition of different nutrient fractionations (DOM optical properties here)to examine differences in uptake. The INSBIRE model appears robust and useful, but its mathematical/statistical evaluation is outside of my expertise. I appreciate the value in the author's research, but my opinion is that this manuscript needs major revisions before publication here or elsewhere.

1. My major concern is the lack of independence in the study design. Nutrient/DOC leachate additions were added to the same stream reach several times per week. Subsidies from these additions are going to stimulate periphyton communities increasing their metabolic activity and biomass. More metabolic activity/biomass of bacteria and algae is going to result in faster uptake velocities of DOC and nutrients (e.g., SRP). Thus, I do not think it is entirely fair to compare uptake rates of different leachates between multiple additions that occurred over a few weeks as periphyton communities would have had ample time to use these resource subsidies to increase production and potentially alter subsequent nutrient uptake measurements. Commentary is needed in the methods to justify the study design and in the discussion to address the potential effects of repeated resource subsidies on nutrient uptake rates.

> We fully understand the reviewer's concern about independence as this was also our concern in planning the experimental design. However, as the environment also changes naturally (e.g. discharge, temperature), different additions cannot be compared if the interval between them is too long. Thus, we decided for a compromise concerning the length of the intervals between the different samplings based on our long-term experiences in nutrient additions experiments. Independence effects were reduced to a minimum through the following considerations and were also checked regularly during the entire experiment:
> a) The added material did not induce unnatural concentrations in the stream, but created peaks equal to or below local rain events. As the additions were within the range of the natural variability of the stream, we do not expect any stimulation of biofilm growth through the additions. Biofilm samplings after each addition as well as between additions supported this assumption by showing no systematic change in enzymatic activities over the course of the experiment.
> b) Additions were limited to a maximum of two times per week with an interval of at least 48 h between two consecutive samplings, allowing the system enough time to recover. We also observed no systematic change in uptake rates over the course of the experiment, supporting again the assumption that the additions did not stimulate biofilm communities and their metabolism. As we could not identify a stimulation from additional P-PO4 on the DOM uptake, we conclude that the additional P-PO4 had no significant impact on the

metabolic processes.

c) Regarding the natural environmental changes, we were lucky to accomplish all our experiments during a period of stable weather conditions.

We added the above mentioned information in the method section and also shortly discuss potential effects of repeated additions as suggested by the reviewer. (Lines 210-217)

2. While I see the value in the INSBIRE model, I think there is too much commentary on it in the manuscript for this type of journal. I think these sections could be simplified to improve flow and keep the focus of the manuscript on DOM dynamics – as was outlined by the research questions in the introduction. Much of the detailed information could be added to a supplement for interested readers and anyone wanting to use the INSBIRE model in their own research.

The section about the methodological approach to INSBIRE was mostly rewritten to make it easier to follow from a biogeochemical perspective (lines 302-464). We pronounce the differences to commonly applied analyses within the nutrient spiralling concept and explain the fundamentally important aspects in the manuscript. Most of the mathematical details (lines 353-427) were moved to section S1 in the supplement material.

a. The use of probability distributions to describe uncertainty is a valuable aspect of Bayesian modelling and INSBIRE. However – while visually appealing – Figures 5and 6 are hard for the reader to interpret (e.g., how much do they overlap). The use of numerical 95% or 90% credible intervals instead of these distributions would be beneficial to the reader as the degree of overlap can be readily ascertained.

Thank you for this suggestion. We have tried several options and decided to keep the graphs as they can provide complementing information (e.g. mixtures of distributions become visible through shoulders in the curves) but to add a table with Bayes factors of the probability (Lines 525ff, 535ff). By that, we can make the difference between uptake velocities easy to understand without overloading the graph. We consider this approach beneficial for the understanding.

3. This manuscript would greatly benefit from a thorough editorial review to improve sentence structure, clarity, and flow. Some of the more complex sections in the methods, results, and discussion were hard to follow making it difficult to understand what was done and provide a comprehensive academic review of the manuscript. I have included some technical corrections below, but level of editing needed is beyond my capacity as a reviewer and my editorial comments are not complete.

We improved the language and the structure of the manuscript significantly. Specifically, we revised the phrasing, paragraph and sentence structure, and moved more detailed information on the model to the supplement material. We furthermore structured the discussion more clearly according to the research questions. A native speaker rechecked the writing to increase the clarity of presentation.

a. Paragraphs in the discussion and introduction would benefit from clear introduction and conclusion sentences to define the topic in the body. Clarity in structure would help the reader follow along.

We appreciate the comment of the reviewer and revised the manuscript accordingly.

b. Avoid starting sentences with "it", "these", "this", "they" ect. especially when multiple subjects are being referred to. Best to be specific and clear to help the reader follow along.

We appreciate the comment of the reviewer and revised the manuscript accordingly.

Specific/Technical Comments:

Introduction

1. The content of the introduction is good, but a thorough editorial review is needed to help the reader follow along and increase the connection between the content/concepts introduced.

We revised the manuscript accordingly and provided clear connections between ideas and topics addressed wherever necessary to improve the structure and help readers follow our concepts.

a. Could add a bit more information on DOM composition e.g., mixture of labile and recalcitrant compounds to the paragraph on lines 56-69.

We added more information on DOM composition as suggested (lines 75-81).

2. A better definition of dampening/stimulating effects is needed, and I do not think this is the most appropriate term. Nutrient uptake by stream communities has an upper limit simply due to scaled up enzyme kinetics. Once that uptake rate is reached, it will not increase any further even with increased nutrient concentrations. Your non-linear models are entirely appropriate, but the way that "dampening effects" are defined (line 80: "dampening effects of nutrient concentration on the uptake efficiency") and referred to throughout the manuscript is not clear to the reader. The lack of clarity here continues in the discussion in the paragraph on "dampening effects".

We appreciate the comment of the reviewer. The term "dampening" was completely removed from the manuscript. Instead, we elaborated the "efficiency loss" model (decreased uptake with increasing concentration of the same component) as well as positive and negative interactions (decreased/increased uptake with increasing concentrations of another component) to clarify this issue.

Line 42: sentence is vague and could likely be connected to the following sentence to add context

This was solved by rewriting the introduction.

Line 43: relate – is related to; stream and rivers are synonyms maybe differentiate with more context e.g., headwater streams, large rivers

We differentiated between headwater streams and larger rivers accurately throughout the manuscript.

Line 45-46: duplicate references

Duplicate references were corrected.

Line 46-47: DOM influences the toxicity of pesticides has no context and does not relate to your research question, consider removing – also duplicate references

This sentence only lists a few of the various effects of DOM on important ecosystem processes, highlighting the key role of DOM for the aquatic ecosystem. Due to the restructuring of the introduction, we think that mentioning this within a list of other effects will not confuse people.

Line 48: "this capacity" – what capacity? Can link to the previous sentence to add context

The misunderstanding was solved by rewriting the introduction.

Line 61: is produced – are produced

This was solved by rewriting the introduction.

Methods

1. Section 2.1 additional commentary – a sentence or two – on the Hydrological OpenAir Laboratory would be better than just a citation.

We added the methodological approach and the aim of the HOAL. A link to the website was added for readers interested in past and current work. (lines 168-172)

2. Section 2.2 see general comment on experimental design. Increased justification is needed.

See comment general part 1.

3. I think sections 2.6 and 2.7 need to be simplified and organized based on how you presented the research questions in the introduction to make the methods easier to follow. Two models were used to look at 1) random effects of DOM quality and 2)interactions? Clear definition of variables in the interaction models would be helpful.

Section 2.6 and 2.7 were combined and fundamentally changed. Most of former section 2.7 was moved to the supplement material. We used the structure of the research questions to guide the reader through INSBIRE and provided necessary information, leaving the mathematical details for the interested reader in the supplements.

a. For interactions did you only used the power function with your independent variables? Did you test different functions or just use the power one? I guess the choice of the power function was because it was used in previous studies. I think an exponential function or a logistic function, if you want to go one step further, is more ecologically appropriate.

The choice of a suitable function is indeed a difficult part in the analysis and we agree with the reviewer, that the power function is ecologically questionable. Besides the power function, we tested a linear function, a Michaelis-Menten type function, an exponential function and an asymptotic regression function. With some background knowledge, all of those (and more) functions can be used within the INSBIRE approach. We decided to use the power function because those models showed the highest Bayes factors for most additions. The big advantage of the power function is that there is only one parameter to fit, which makes it less prone to over-fitting (discussed in e.g. McElreath, 2020, chapter 7). Also, in our experiment, concentrations hardly reached uptake limits. In such cases, uptake rate curves often exhibit a power function (such as e.g. the efficiency loss model described by numerous other authors), probably representing the lower part of a saturation model within a concentration range below saturation and thus naturally met in the system.

b. I am confused by the addition of wetted width as an interaction. Sure, more surface area might equal more retention of DOM, but all measurements occurred in the same stream with little difference in discharge.

> As our stream was rather small, even small changes in discharge may create quite large differences in the wetted width and thus in the reactive surface area. In fact, the wetted width of our stream ranged between 2.6 and 7.2, so the difference is a factor of 2.8. Thus, wetted width is an important parameter to analyse differences between sampling dates not due to source effects and our results actually show different responses of the various DOM components to changes in wetted width.

Throughout: units for liters should be L

> L was used for litres throughout the manuscript.

Results

1. The result section had many method-like statements in it. Some of these statements provided information that was not present in the methods. A clear methods section that follows the research questions presented in the introduction would help to keep the results section as a summary of what was found.

> We clearly distinguished between methods and results and structured the revised manuscript accordingly (lines 467-468, 508-510, 553-555, 567-568).

2. Results could be better structured around the 3 research questions from the introduction.

> We restructured the results section in alignment with the research questions.

Discussion

1. Discussion could be better structured around the 3 research questions from the introduction

> We restructured the discussion section in alignment with the research questions.

2. Stronger introduction sentences are needed set up what is going to be discussed in each paragraph.

> Large parts of the discussion section were heavily edited and we kept this suggestion in mind during the process.

3. "Interactions among different DOM components, which indicate transformations of one substance into another during DOM processing" – an interaction means that the relationship between uptake and concentration of one component is dependent on the concentration of another component. For example, more SRP leads to a more rapid increase in the uptake of a component relative to concentration. The interaction models were difficult to understand, but the above conclusion does not make sense. If DOM was being broken down into different components, the uptake of one component would be positively associated with the concentration of a degradation product. Since all leachate additions differed in composition the concentration of individual components is confounded. Correlations among net uptake of the different components may be better suited to address this.

Correct, two components within the same degradation or production process are not interacting; thanks to the reviewer's comment, we have realized that the original phrasing of this statement was unfortunate and much too short to describe the full complexity of this idea. What we actually meant was: Mathematical interactions between two different DOM components given by the model may not necessarily come from (ecological) interactions, such as in the SRP example described by the reviewer (where one component affects the uptake of the other component); they may originate from a degradation or production process of DOM, in which both components are involved at different stages, so that one component is an intermediate product of the other. Unfortunately, we cannot check whether this assumption is true with the applied method of spectroscopic analyses; however, several other authors have found indications that especially DOM components, which increase during the DOM uptake, may actually be degradation products of other (decreasing) DOM components.
Because of the complexity of the topic, we think, that correlations might not be able to reveal subtle relations (that might be covered by hydrology e.g.) and therefore used a multivariate regression approach.
We clarified passages of the manuscript accordingly and explained and used exact terms for mathematical interactions, ecological interactions, and dependent components occurring at different stages of the same process.

Line 403-405: these two sentences contradict each other. Provide more context.

What we meant was that previous studies have shown different uptake velocities for different DOM sources like our study, but the number of such studies is too low and the used leachates and studied systems too diverse to draw any general conclusions about the uptake of different natural and anthropogenic DOM sources (or component mixtures) yet.
Through the rewriting of the discussion section, especially, the more precise analysis of the differences of v_fs, we could solve this contradiction.

Line 403-420: This paragraph goes back and forth comparing uptake within your study and between your study and others. I suggest sticking with the latter and write a new paragraph for the between leachate differences you observed in your study. This new paragraph will also help transition into the next paragraph.

The two topics were clearly separated.

Line 414: dung allied – manure added

This was solved by rewriting the discussion.

Line 421: relation – relationship

This was solved by a rewritten discussion.

Line 432: should this be Figure 6?

We corrected this cross reference.

Section 4.2: first paragraph states that there was no major difference in DOM bioavailability indicated by the broad overlap of parameter ranges, but the second paragraph discusses differences in uptake of different DOM components. The two paragraphs contradict each other.

From the reviewer’s comments, we saw a need in better expressing the differences between the uptake velocities of different fluorophores. We solved the mentioned contradiction by quantifying the probability of a difference with the Bayes factor and a clearer description (lines 525ff, 535ff).

Reviewer 2

In their manuscript entitled “Complex interactions of in-stream DOM and nutrient spiralling unravelled by Bayesian regression analysis”, Pucher et al. investigate the inter-actions between DOM and nutrient uptakes in a small stream based on an experimental setup. They used five different leachates having contrasting DOM properties based on optical measurements (PARAFAC), added these leachates into the stream and then measured DOC and nutrient concentrations and DOM properties along a 215 m reach. For interpreting their data and calculate uptake velocities, they proposed a new approach based on the spiralling concept and called Interactions in Nutrient Spirals using BayesIan REgression (INSBIRE). The topic is of great interest, however the manuscript is hard to follow for readers that are not familiar with this type of approach. Furthermore, some clarification are required regarding the relevance of INSBIRE. Indeed, it seems to me that the model requires a lot of parametrization that is subjective (e.g. lines 365-378 or lines 390-400). It is like turning buttons to fit each data individually without clear ideas about the processes behind. It is therefore hard to interpret the data, as recognized by the authors (line 454 for instance) that finally can only make hypothesis on the processes occurring (e.g. lines 482-487). It doesn’t seem to me that INSBIRE allows finally to investigate or quantify properly the interactions between DOM and nutrients uptakes, and I was also wondering to which extent this approach could be used by other researcher and/or in other study sites.

We thank the reviewer for his/her comments on the manuscript. From the comments, we see a need for text improvements and a shift in focus on the biogeochemical processes. However, we disagree with the reviewer’s comment that the used INSBIRE approach is done by "turning buttons to fit data without clear ideas about the processes behind". Quite on the contrary, all model assumptions are based on ecological models and processes, commonly known in aquatic biogeochemistry and observed in various other (nutrient) uptake studies, such as saturation uptake kinetics, nutrient spiralling, the nutrient efficiency loss model, hydrological retention, and uptake and transformation processes.
The parametrization of the model is based on previous studies (mostly the nutrient spiralling concept, additionally Dodds et al., 2002; O’Brien et al., 2007, all of them were cited in many other studies). For nutrient addition studies these have proven suitable. INSBIRE extends these concepts from single to multiple compounds which needs to be proven useful, but can be one logical and justified next step from recent research. We explained the extension by the parameter l in lines 345 to 348 because we have not found something comparable in any other study. The initial introduction of the additive parameter l to the model was solely motivated by knowledge about the processes and tested statistically in the following steps. We consider the Bayes factor a suitable metric in model selection to avoid over-parametrization and provided references to support this assumption.
INSBIRE quantifies relations in uptake processes by means of Bayesian posterior distributions but not by means of single values. This approach has proven beneficial in many ecological studies and is justified in lines 101-103 and 807-809. We addressed this idea more understandable in the manuscript.
We see difficulties in the interpretation because of the novelty of the approach and its application in only one study so far but our study (e.g. figure 7) shows clear trends in the data. We see a high potential in using INSBIRE in many other field and lab experiments, especially regarding interactions in multi-component mixtures..
Details on INSBIRE that are only necessary for applications in other studies were placed in

the supplement material to allow a fluent reading for people interested in our findings on the carbon cycle in aquatic ecosystems. We focused on a well-known theoretical, biogeochemical basis and a step-wise guidance to INSBIRE. Restructuring the methods section according to the research questions will help the reader to follow the intentions, decisions and aims.

Introduction

The introduction lacks of context regarding the interactions between DOM and nutrients, and why this is an important issue. The two first paragraphs are very broad, focusing on the importance of DOM on the biogeochemical and ecological functioning of freshwater ecosystems and on the impact of agriculture on DOM (which is not the subject of the study), and do not help the reader to understand why "the effects of changed DOM and nutrient supply on the DOM and nutrient uptake in streams remains in the dark" (lines 54-55) or why the authors "expect a complex interaction between the different DOM fractions and the available N and P to explicate the bioavailability and the aquatic retention of the DOM" (lines 66-67). The introduction could be improve by including more context about DOM/nutrient interactions based on previous works (e.g. Guillemette and del Giorgio 2012; Vonk et al. 2015; Catalán et al. 2018). The conclusions/limitations of these studies should be added/discussed in the introduction in order to clearly identify the big picture of the manuscript.

The study aimed at analysing the in-stream uptake of different complex DOM sources and the relation of DOM uptake on the occurrence of and the interactions between different DOM fractions including co-leached nutrients. However, the aim was not to study the interaction between nutrients and DOM uptake; thus, no nutrients were added, but some were naturally still contained in the organic matter leachates (albeit at generally low concentrations) and therefore also included in the analyses as parts of the DOM leachates (similar to the organic DOM components). We decided against citing too many nutrient (and DOC) addition studies about nutrient-DOC interactions in the introduction not to mislead readers about the aims of our experiments (namely, that we tested the effects of external nutrient sources on the DOM uptake). For this reason, we have added "co-leached" to the term nutrients. We have also re-written the entire introduction, clarified the aims, and especially pointed out the role of the nutrients as inorganic part of the DOM leachate. We have also included the Catalan and Guillemette as well as other stuies in the introduction and discussion to interpret general interactions between the DOM uptake and (co-leached) nutrients. Unfortunately, studies in lake systems or lab incubations (Guillemette and el Giorgio, Vonk et al, and many others) or using artificial substrates such as acetate (Catalan et al) are only limited applicable for our study.
We want to stress that DOM addition studies, which investigate the uptake and the interactions of the individual inorganic and organic components of complex DOM sources, are scarce and the interpretations are complicated, which makes our study and the developed INSBIRE approach an important step towards unravelling the mechanisms of DOM bioavailability.

The last paragraph is I think the most interesting part as the authors propose a new model based on the nutrient spiralling concept to quantify DOM/N/P interactions. How-ever it is very technical (e.g. lines 76-85) and hard to follow for readers that are not familiar with these concepts. Thus, while I fell that the INSBIRE is of potential inter-est, I was lost before the end of the introduction and didn't understand how it may differ from existing models. I think this paragraph should be reformulated in a more understandable way, details being provided in the Material and methods section.

We focused on usability and differences to existing models in the introduction and shifted the technical details to the methods or supplement material, which improves readability.

Lines 45-47: problem with references.

References were corrected.

Line 48: add references.

We added references.

Line 60: add reference.

We added references.

Lines 62-63: this sentence is quite abusive. Moreover, the authors face the same problem with their approach as they are not able to identify any transformation pathways(they only make hypothesis).

We rephrased this part. We wanted to stress that mass balance approaches work well with P and N, but are limited if dealing with C components. We do not claim to identify transformation pathways, although they would provide a much more detailed insight into DOM uptake, as the method of spectroscopic analyses is not suitable for this purpose.

Methods

Line 94: Please provide information regarding the water residence time of the study site.

We added mean water residence times for the stretches (Figure 2).

Line 113: After how long the plateau was reached after leachate additions, and how it compares with the travelling time of the stream?

We added this information to the manuscript (Figure 2, lines 200-201).

Line 138: some additions are very low compared to ambient DOC, so how the authors can be sure that they are measuring uptake for leachates and not from ambient DOM?Please provide more justification here.

Indeed, some additions were quite low mainly due to our attempt to keep peaks within a realistic range, methodological issues (restricted leaching from some sources, low pumping rate), and small environmental changes in background concentrations and discharge. We corrected plateau concentrations by background and removed measurements that deviated from the ambient concentration less than two times the measurement accuracy of our lab instruments to remove questionable values. This information was added in lines 317-319.

Line 149: specify the number and origin of EEMs included in the PARAFAC model.

We added this information. The number of EEMs was 176 and their origin was the very stream, the experiment took place in (line 240).

Line 181: overall the description of INSBIRE, including equation and hypothesis made, is very hard to follow. It seems that several choices are made but justification and/or implications on the model *results are not provided. For instance, how is defined the threshold that determines if some data are removed or not (line 204-205)? How do the authors justify the addition of a product of power functions to include interaction, and what do they mean by interaction (line 239)? How do they determine if/when adding the wetted width is beneficial (line 240) and how do they related wetted witch to stream surface bed and/or retention processes? I think that all the presentation of INSBIRE should be reconsidered. Also, I didn't see any figures about errors from the model.*

We added the following information: We removed measurements which deviated from the ambient concentration less than two times the measurement accuracy of our lab instruments (lines 317-319). The power function was chosen after testing several functions found in various nutrient addition studies (power function, linear function, Michaelis-Menten type function, exponential function and asymptotic regression function) and calculating the Bayes factor of the models. In most of the cases, the power function showed the best fit. Besides, the power function has only one parameter and is therefore less prone to over-fitting. This fact and the better comparability amongst the relations was the reason to use the power function throughout the study. We are aware, that the power function does not provide an upper limit of the uptake, which would be ecologically sound. However, as the power function has been described and used in various other uptake studies, where saturation was not reached, we consider this a valid decision (lines 333-341).
The wetted width (as all other influencing factors) was added in cases, where the Bayes factor supported this decision. From a detectable influence of the wetted width, we concluded its importance for the respective processes. More wetted width means more surface covered with sediments and benthic microbes and we, therefore, think there is an important connection. The presented difference in impact of the wetted width is for us a clear hint, that some uptake processes might primarily take place at the sediment surface and others in the water column. Previous studies have located retention processes either in the benthic zone or in the water column. We can show a quality/nutrient dependence and demonstrate a method for this inference (lines 349-352, 565ff).
However, we have realized from the two reviews that we need to better define and distinguish between the terms of mathematical interaction/correlations, influencing factors, and ecological interaction in the revised manuscript.
Errors from the model were presented in figure S01.

Lines 197-199: I don't understand these sentences.

We assume that you refer to the use of priors and agree that the use of priors is not explained in depth within the mentioned sentences. The use priors is an important and needed part of Bayesian statistics. However, due to the length of the manuscript we do not include a detailed explanation, but add a reference with an in-depth description (lines 307-309).

Lines 259-260: and? What does it imply?

Concentration data is often lognormal distributed, hence we first chose lognormal error distributions. Therefore, it was our first choice as well, but our data did not fit this assumption and we used a normal error distribution instead. We mentioned this because we thought the question might arise. Exemplary studies, although from other geochemical fields of study addressing error distributions are: Ott (1990) A Physical Explanation of the Lognormality of Pollutant Concentrations, doi: 10.1080/10473289.1990.10466789, Ahrens (1954) The lognormal distribution of the elements (A fundamental law of geochemistry and its subsidiary), doi: 10.1016/0016-7037(54)90040-X (Supplements, lines 63-64).

Results & Discussion

Lines 306-307: some statistic tests would be helpful to measure the level of significance of trends.

We calculated the Bayes factor in favour of an exponential decay over constant concentrations and could show the evidence for this assumption for all DOM fractions and SRP (lines 485-491).

Figure 4: this figure is confusing. What I see here is mixing between leachates and stream waters along the stream reach, while the authors argue that at point 0 the mixing is full. If I understand well, these data are data collected directly in the stream,it would be interesting also to see the data corrected for dilution. Table 5: hard to read.

We improved the description and the readability of the graph (Figure 4, line 493). Sampling letters were exchanged by the dates and separate x-axis ticks for background measurement dates were removed. We ensured a lateral in-stream mixing at point 1 by measuring a uniform conductivity at several points in a cross section. Figure 4 shows indeed a decline of substances due to reactions. The graph was changed to show the data corrected for dilution but still shows nearly the same pattern. Table 5 is now table 7 (lines 597ff). The contents of former table 5 were partly moved to the supplements to keep it simple within the manuscript.

Line 501 & 517: these statements are a lit bit ambitious.

We rephrased the statements. We only wanted to highlight the novelty and the potential of the approach as well as its limitations to encourage others to elaborate it, test and extend its applicability. We also wanted to highlight that the approach is versatile and based on well studied principles combined in a novel way.

References

Casas-Ruiz, J. P., N. Catalán, L. Gómez-Gener, and others. 2017. A tale of pipesand reactors: Controls on the in-stream dynamics of dissolved organic matter in rivers.Limnol. Oceanogr. 62: S85–S94. doi:10.1002/lno.10471

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Guillemette, F., and P. A. del Giorgio. 2012. Simultaneous consumption and productionof fluorescent dissolved organic matter by lake bacterioplankton. Environ. Microbiol.14: 1432–1443. doi:10.1111/j.1462-2920.2012.02728.x

Vonk, J. E., S. E. Tank, P. J. Mann, R. G. M. Spencer, C. C. Treat, R. G. Striegl, B.W. Abbott, and K. P. Wickland. 2015. Biodegradability of dissolved organic carbon in permafrost soils and aquatic systems: A meta-analysis. Biogeosciences 12: 6915–6930. doi:10.5194/bg-12-6915-2015

Casas-Ruiz et al., Catalan et al. and Guillemette et al. were considered and added at the appropriate points.