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Comment on bg-2022-214

Anonymous Referee #1

Referee comment on "Physical and stoichiometric controls on stream respiration in a headwater stream" by Jancoba Dorley et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-214-RC1, 2022

In this study, Dorley et al. examine how stream metabolic activity varies with changes in nutrient and carbon supply. The authors co-inject various combinations of dissolved N, P, and organic C together with a conservative chloride tracer and the smart tracer resazurin, whose transformation to resorufin is a proxy for aerobic respiration. The study would interest the readership of Biogeosciences, given that the physical vs. chemical controls on river metabolism remains to be determined and may fundamentally shift in a changing climate.

The authors interpreted their results using both fits to the transient storage model (TSM) and TASCC analysis. Their analysis show that the reach-scale raz decay rate did not change with discharge or with the nutrient treatment. The invariance of this reach-scale rate suggests that the myriad factors governing reach-scale metabolism are co-varying in such a way that solute delivery to bioactive zones and transformation in bioactive zones remain in balance as discharge changes, OR it suggests that there is some unexplained parameter that limits metabolism. The authors choose the former, stating that reduced transient storage decreases with discharge, but this decrease is balanced both by increasing metabolic activity in remaining transient storage zones and by the changing limitations on metabolic activity associated with their different treatments (see conceptual diagram in Fig 7). While this argument is interesting, I believe there is much more to be done to demonstrate that it is actually valid. I explain several key steps the authors can take to improve interpretation of their results in the Main Comments below, followed by specific comments and technical corrections.

MAIN COMMENTS

- The authors should provide more details of the study design and more clearly describe how the results were interpreted. While it is very reasonable to have a limited description since they are using a published model and fitting algorithm, the authors would give their conclusions much stronger support by including a baseline set of details in the main text and SI. This set includes a description of the objective fitting function (provided in equation form somewhere), goodness of fit for each result, details on how parameter uncertainty was estimated (i.e., what explains the spread in Figs 2, 4, 5), and presentation of BTCs together with and best fit-model BTC. The time series are currently very difficult to access because they are scattered in Hydroshare, and the full conductivity time series are in a very large CSV.
- The Damköhler analysis needs to be revised or further qualified to acknowledge the additional factors that influence reach-scale transformation of raz. As described in Fig 6 and in the text, the authors are relating the reach scale transformation timescale to local timescales in the transient storage zone. This interpretation is too simple because the current definition of Da only gives insights about transformation during a single excursion through the HZ. Reach-scale transformation depends also on the number of exchanges through the HZ and on the travel time through the reach.
- The current conclusions depend on a wide range of assumptions about metabolic activity in transient storage, the location of metabolically active transient storage zones, and solute retention in biofilms. These assumptions oversimplify the mechanisms governing reactive transport in the reach, which suggests that the invariance of reach scale metabolism to treatment could be caused by a number of different factors beyond stoichiometry. I raise several points in the minor comments where I believe the text needs to be qualified or strengthened.

  The authors must better discuss how findings from recent studies might also explain their results. It is well known that we need models that acknowledge the spatial heterogeneity of reactions in the HZ (Boano et al., 2014), which cause a breakdown of the assumption that increased hyporheic residence time "should consistently result in higher biological demand…" (L50, L403). Several modeling studies (Frei et al., 2019; Li et al., 2021; Roche & Dentz, 2022) and field studies (Knapp et al., 2017; Schaper et al., 2018) have recently shown that exposure time in bioactive zones is a dominant control. Others have shown that exposure time in bioactive zones (Marzadri et al., 2017) and discharge-dependent hyporheic exchange rates (Grant et al., 2018) indeed explain the variability of reach-scale rates inferred from the LINX II dataset, which should be discussed in the intro and/or section 3.4 (L369-371).

  Alternative explanations for the consistent As/A across rounds are that the discharge controls the extent of the hyporheic zone in the main channel (Kaufman et al., 2017; Voermans et al., 2018), or that the extent of the bioactive layer in the hyporheic zone is so similar between rounds that it causes the reach-scale rate is roughly the same.

- I highly recommend the authors alter the analysis of reach-scale metabolism in a few ways.
  - The authors appear to already be using the model equations from Knapp et al (2018, supporting information) to interpret results of their conservative data. It shouldn't take too much work, then, to use the same code to interpret the raz processing rates. Doing so will allow them to utilize the full dataset to test the conceptual model they pose in Fig 7. They will be able to incorporate the rru time series into model fits, thereby allowing them to better constrain the raz-> rru transformation rate. Importantly, it gives direct estimates of reaction rates in transient storage zones, which frees the authors from having to use reach-scale rates to interpret how reaction rates in transient storage are changing.
  - Remove the TASCC analysis for two reasons: (i) using Eq (5) alone gives a simple, asymptotic rate that is an exact measure of reach-scale transformation when

interpreted using the reach-scale travel time, and it maps directly to the physical parameters governing reactive transport (as idealized by the TSM). (ii) Results from Eq 6 do not map directly to the model physics, which severely limits the transferability of results. The rates estimated from Eq 5 and Eq 6 will only match when interpreted at the plateau concentration, since Craz_{plateau}/Ccons_{plateau} = m0_{raz}/m0_{raz,inj} at that concentration.

I suspect that interpretation described on Lines 174-180 is an artifact of the experiment design. Specifically, the "[temporal] mean value of all the processing-rate coefficients [from Eq 6] is [nearly] equal to the processing rate coefficient estimated from [Eq 5]…" only because the BTC is a long step injection. The authors could quickly test this speculation by comparing results from Eq 5 and Eq 6 across TSM-simulated injections of different durations, holding all else fixed. The results will not vary if you use eq. 5, nor should they (physical system has not changed). However, they will change when using the mean of Eq 6.

SPECIFIC COMMENTS

L25: Consider labeling C as a terminal electron acceptor rather than a nutrient.

L57: Consider changing optimal distribution -> optimal ratio to avoid confusion regarding probability distributions. Also, consider a more precise statement than "ecosystems…flourish", e.g., an ecosystem requires…to maximize nutrient uptake.

L59: This paragraph is confusing because biological demand, consumption of nutrients, ecosystem flourishing, and N retention are all used interchangeably. I recommend clearer and more consistent language. Here, N utilization or N consumption may be a better phrase, since N retention implies biological uptake only. In reality, the stream is also denitrifying.

L63: changing transport timescale to retention timescale, which would align the definition more closely with that used in this ms.

L77: An appropriate citation here is (Tromboni et al., 2018).

L94: Do background concentrations or other data suggest that a certain nutrient is limiting in Como Creek?

L110: How was chloride measured?

L119: How was discharge estimated (and its uncertainty)? Did you assume complete mass recovery in each reach, or was some other method used?

L122: How were these injection masses determined?

L122: Change N/A to '-' in the 4th row of Table 1.

L124: As stated in the main comments, the authors should include BTCs in the ms. (e.g., one representative BTC with model comparison in main text, and all BTCs in SI with moments). This will prevent the reader to have to work with a very large csv file to view the data.

L170: This statement is incorrect. A non-limiting nutrient would probably have a zero-order reaction rate, but the ratio of reactive to conservative concentrations would not remain constant.

L195: See Main Comment 1. I think the authors are describing a monte carlo based fitting algorithm, but I'm not sure.

L246-248: I do not follow this argument. There should be substantial hyporheic exchange (i.e., transient storage associated with the hyporheic zone) given hydrostatic head gradients through pool-riffle sequences, and the authors state earlier that there are substantial gravels in the reach.

L250-253: This is a circular argument that must be removed or changed. It states As/A is invariant to discharge, "…which suggests that As and A varied proportionally with discharge."

L258: Should be $\lambda_{raz, sample}$ correct?

L268-269. The claim that biofilms predominantly reside in pools is unsupported. It's just as likely that the large surface area-to-volume ratio of hyporheic sediments means that there is greater biomass in the hyporheic zone. I suggest the authors remove or better support this claim.

L329-330: This claim is supported by the argument on L250-253, which itself needs to be better supported.

L344: I do not understand this argument. It seems the authors are claiming that biofilms act as small transient storage zones, where retention times within the bioactive zone far exceeds the reaction rate, and nutrients therefore build up. But later they claim that these biofilms are more metabolically active, which is a different "rate limiter" from what they just described. As stated in the Main Comments, I think the authors are making it extra challenging for themselves by trying to how local processes dominate (and change) through the lens of an integrated measure of reactive transport in the reach (i.e., Eq 5).

L393: See Tromboni et al (2018). I imagine there are other fluvial ecology studies that similarly evaluate co-limitations.

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