Physical and stoichiometric controls on stream respiration in a headwater stream

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Response to reviewers

Reviewer # 1

The authors have added information to the experimental methods and have made several improvements to the discussion. I agree with these changes.

The authors have chosen not to make any modeling changes. I believe their reasoning is partially based on a misunderstanding. The authors define "reach-scale" metric as any metric derived from reach-scale observations. This is fine, but it's important to note that the two reach-scale metrics being compared in their Da analysis are fundamentally different. Their reaction timescale (inverse of Eq. 5) is a LUMPED metric that reports the combined influence of transport and reaction processes. In contrast, the transient storage timescale is inferred from a model fit of an average LOCAL process (i.e., retention during a single immobilization in the transient storage zone). That is true even if the model fit is based on reach scale experimental data. I therefore don't think the two metrics should be used for a Da analysis because they are not independent. The reaction rate inferred from Eq 5 is inherently correlated with the transient storage timescale. See, for example, Eqs 10-12 from Runkel (2007).

While I disagree with the authors' assessment that no local processes can be inferred from reach-scale data, such inferences are extremely difficult and likely not possible when multiple factors are changing simultaneously. As such, I agree that a lumped metric such as Eq 5 is likely the best that can be used to ensure a statistical analysis is robust (as opposed to basing the analysis on model fits of the parameters in Eqs 1-2). In any case, the authors should make a few changes to clarify the modeling steps:

-L193: Remove or change the middle part of the Eq 5. The integrated reaction rate is not equal to the rates in the main channel and transient storage defined in Eqs 1-2, because it depends on the integration of reactions and solute immobilization in transient storage. See Haggerty (2013).

Thanks for this suggestion. We made the change as you requested.

-L212-214: This statement needs a citation or to be proven. As I detailed in my first review, I question whether it's true that the rate inferred from Eq 5 is equal to the mean of the rates inferred from Eq 6 except for in limited cases.

Thanks for this comment. We have decided to clarify and simplify the scope of this paper by changing the description of equation (6) to match the equation used in the original TASCC *manuscript by Covino et al. (2010b). The new (this review) and old (past versions) equations (6) are numerically equivalent, since our stream data features Peclet numbers much greater than 10 (see Gonzalez-Pinzon and Haggerty (2013)). This change resulted in minor modifications of the text around Equations (5) and (6). Given the increased complexity required to describe the connection between TASCC and different commonly used solute transport models (e.g., ADE, TSM with and without sorption), González-Pinzón will develop this concept more in depth in another manuscript that exclusively focuses on the mathematical analogies.*

-Clarify whether the values of lambda_raz being used for the analysis are derived from Eq 5 or from Eq 6.

Thanks for this suggestion. We have now clarified the text around figures that use equations 5 or 6. We have done the same in the figure captions and legends.

Reviewer # 3

In this paper, Dorley et al. address the question of how the metabolic activity of streams changes with changes in nutrient and carbon supply. The authors use different tracers, which are various combinations of dissolved N, P and organic C, and inject them together with a conservative chloride tracer and the intelligent tracer resazurin, whose conversion to resorufin is an indicator of aerobic respiration. The results are certainly of interest to the readership of Biogeosciences, as the physical and chemical control of flux metabolism is not yet clear and may change fundamentally in a changing climate.

In the first review process, two anonymous referees dealt intensively with the manuscript. The authors have substantially revised the manuscript and largely considered the critical comments of the reviewers. The conclusions were fundamentally revised and a reference to other current studies was made. Nevertheless, in my opinion the explanation regarding the Damköhler analysis has to be integrated into the description of the methods. The anonymous referee #1 recommended to discuss the results with different various recent modeling studies e.g., Roche & Dentz 2022, which are not included in the new list of references.

Thanks for these suggestions. We have added the equation for the Damköhler number in the Methods (see Equation 7). Our Introduction, Results and Discussion sections extensively describe this number's information content and application.

Given that we lack data from within benthic biolayers in our work, the discussion of the work by Roche and Dentz (2022) to contextualize ours would be largely speculative and inconclusive. We are aware of their exciting progress and believe that datasets such as the one published (by some of us) in Knapp et al. (2017) would be much better to entertain a more fruitful discussion. With that in mind, and given the recent publication of their work, we do not want to add noise to the high impact they will likely have. As the reviewer verified, in our review #1 we incorporated almost all the other suggestions provided.

Roche, K. R., & Dentz, M. (2022). Benthic biolayer structure controls whole-stream reactive transport. Geophysical Research Letters, 49, e2021GL096803. https://doi.org/10.1029/2021GL096803

Knapp, J. L. A., González‐Pinzón, R., Drummond, J. D., Larsen, L. G., Cirpka, O. A., and Harvey, J. W.(2017), Tracer‐based characterization of hyporheic exchange and benthic biolayers in streams, Water Resour. Res., 53, 1575– 1594, doi:10.1002/2016WR019393.