Biogeosciences Discussions

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Comment on bg-2022-146

Anonymous Referee #2

Referee comment on "Contrasting activation energies of litter-associated respiration and P uptake drive lower cumulative P uptake at higher temperatures" by Nathan J. Tomczyk et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-146-RC2, 2022

Review of Tomczyk et al. Contrasting activation energies of respiration and nutrient uptake drive lower ecosystem-level uptake at higher temperatures

General comments

This study conducted multiple different lab-based experiments testing the effects of temperature and P availability on respiration and P uptake for on leaf litter fragments. The study then used the results of these different experiments to build a simple simulation model to test how temperature and P concentration would affect cumulative P uptake in situ. They found that both respiration and P uptake increased with increasing temperature, but respiration increased more rapidly than P uptake. This then led to a somewhat counter-intuitive result of warmer streams exhibiting lower cumulative P uptake in the simulation study on an areal basis.

While I think this was a well conceived, designed, and conducted study, I believe that the authors may have pushed their results a bit farther than warranted. Particularly given the scope of the laboratory study. The simulation results were used to compare to reach-scale studies despite the simulation ignoring a multitude of other P uptake and respiration mechanisms common in streams and likely affected by temperature in complex ways. I don’t think the simulation study is a problem, but I think it should be discussed for what it is: a simple simulation to develop testable hypotheses. It is not representative of real world expectations and shouldn’t be considered as such. There were also some issues with the methods and particularly the lack of detail on statistical analyses performed. I think this manuscript has the potential to be a valuable contributor to the field and poses some interesting next steps that must be considered as we continue to push further into coupled C-N-P cycles and expectations with warmer climates.

Specific comments:

Line 16: I don't know what a 0.48 and 1.02 eV value means for temperature dependence. Is this standard unit/metric used to compare temperature dependence of various processes? Not sure if this is the best choice for the abstract.

Line 17: for ranges (0.12 to 0.48, or 11 to 212) I encourage authors to use "to" instead of a hyphen because a hyphen could be misconstrued to represent a negative sign.

Line 34: Should this be increases in productivity rather than increases in growth? The Rasmussen citation quantified stream metabolic activity (GPP, ER) not growth.

Line 40: If the authors are using U in the nutrient spiraling sense, isn't U directly correlated with nutrient availability? It's in the calculation, isn't it?

Lines 60 – 61: I mean, maybe? But the Michaelis-Menten kinetics (Vmax, Ks, etc) might not kick in until super high concentrations, though.

Lines 76-78: How much of this negative effect of temperature could be due to canopies opening up in cooler months leading to more light and subsequently more autotrophic nutrient uptake? Even forested headwater streams have open canopies sometime and that short window of autotrophic activity could offset heterotrophic decreases maybe?

Lines 103 – 104: When were bags deployed initially? There are two collection dates but only one incubation date. Not a big deal but if you are going to report collection date, report deployment date, too.

Line 105: Were fragments a consistent size? Or was there a targeted size? Why cut the leaves into smaller fragments? How much leaf litter material (mass) was added to each bottle? How much water (1L?)

Line 113: Were blanks measured initially and after the end of the incubation the same way? I worry about displacement/replacement of water due to the initial DO measurement given how large DO probes can be compared to a scint vial.

Line 113: Was there at least an attempt to add a similar amount of leaf material to each vial?

Line 117: I suggest writing this out as an equation. Were the incubations done in the dark? I don't see anywhere suggesting that. If incubations were not done in the dark then this approach yields NEP, not respiration.

Line 123: nominal pore size?

Line 126: U is traditionally reported in units of mass per area per time (e.g., mg P / m2 / h). I think the approach the authors have taken here to estimate uptake as mass of nutrient per mass of leaf per time is fine but I think that something other than just "U" should be used here. Also, I'm not the biggest fan of the calculation for U as I would greatly prefer an initial and final sample collected from the same sample container. The authors are assuming that all incubation vials started with the same conditions. I don't know how I feel about that assumption. Were individual tubes amended with P? Or was a reservoir amended with P and then added to the tubes? Also, the drastic differences in incubation time is strongly suggesting an assumption of linear P uptake which I don't know can be expected to hold true across different concentrations.

Line 135: Where was this categorical block effect included? What are the different experimental batches? I don't understand this statement at all.

Lines 136 – 138: What? I do not understand this statement at all. There was a model that compared respiration and Usrp to each other and a categorical variable that indicated if the model was for respiration or Usrp? What kind of a model? How was it evaluated? What was the dependent variable? The dependent variables? More detail and description is needed here.

Line 138: A significant interaction term between what?

Line 143: Why was the centered inverse Boltzmann temperature used as the predictor variable? Why not just temperature?

Line 149: Were leaves weighed at the end of the experiment tin the same manner? How were initial SRP concentrations achieved?

Lines 150 – 154: So 6 temperatures * 8 SRP concentrations * 3 bottles per treatment =

144 individual incubations. Is that accurate?

Lines 145 – 173: So basically Usrp was regressed against temperature for each initial SRP concentration and then the slope of those regressions were compared across initial SRP concentration? Was a regression or correlation or something done here? There don't seem to be any stats, it reads like the authors plotted these out and visualized them but that's not a real satisfying analysis in my opinion. The same general though holds for the M-M analysis, too.

Line 199: It seems like there should to be an analysis section. Or more detail needs to be given for the analyses in the individual sections (as was described in some of my previous comments). How were the simulation models assessed/evaluated?

Line 203: canonical is an odd word choice here.

Line 226: These are interesting results. It's definitely a very simplified model, but I think that is acknowledged and it points towards interesting (and testable) mechanisms changing P dynamics with future warming expectations. Obviously there are many other things to consider (e.g., changes in animal behaviors altering the decomposition of leaves, shifts in phenology matching shifts in climate, changes in N dynamics and broader stoichiometric questions…) but still an interesting exercise.

Line 256: Again, I wonder how much temperature is correlated with canopy cover in some of these whole system nutrient spiraling studies. I also think it's difficult to compare a scint vial's worth of Usrp to a full stream nutrient release. The authors have quantified the effect of temperature on leaf respiration and leaf-based Usrp. They did not measure anything about other components of the ecosystem that could/would change with temperature (e.g., sediment uptake dynamics, hyporheic processes, autotrophic uptake (which would increase with decreasing temps due to autocorrelation with canopy cover). While I think the simulation study was a valuable exercise, I don't think that these results can really be extrapolated and compared to reach-scale results/studies.

Lines 262 – 263: This statement is unfounded. I disagree that the current study separated the contribution of physiological and biomass-mediated effects of temperature on ecosystem-level nutrient uptake. As mentioned in the previous comment, the study separated the contribution of these temperature-based mechanisms to affect leaf litter respiration and Usrp. Even in the most detritally-driven ecosystems (of which, Coweeta stream are definitely up near the top), there are still a multitude of other autotrohic and heterotrophic compartments contributing to ecosystem-scale respiration and nutrient uptake. This section should be either deleted or modified to be more accurate for what the study actually did do (which is still a valiant effort!).

Line 265: But this ignores potential increases in sediment-based Usrp. Or autotrophic. Or hyporheric. Maybe the leaf litter breakdown is fueling more labile DOM to reach interstitial spaces where sorbed P can be broken down.

Line 295: These are great caveats to include. I don't know how the authors can make the bold claims such as in lines 262 – 263 and then simultaneously acknowledge all of these issues.

Line 305:Not sure how this study addresses/provides insight into the effect of observational scale on temperature sensitivity.

Line 305: The majority (entirety?) of the discussion focuses on the simulation experiment, which is the weakest part of this paper in my opinion. I think more general discussion of temperature dependence of biogeochemical processes and how that can affect things more broadly would worth including initially, and then a toned down version of the focus on the simulation model. E.g. 'The results of our lab incubations would theoretically imply xyz. Our simulation studies confirm some of these expectations but revealed somewhat contradictory patterns due to abc.'

Figure 1: I know the stats and fits are included in table 1, but I still think they'd be good to include on the figures, personally. I recommend including the equations for each line as well as stats (r-2, p-value).

Figure 2a: Are each of these significant regressions in panel a? Doesn't seem like it, which would entail no relationship between SRP uptake and temperature at certain initial values. A table (supplemental?) with slopes, r-squared, etc supporting these model fits would be useful. I'm not sure what in table 1 is showing model fit for these lines. Maybe a more details statistical analysis section in the methods would help clarify things a bit.

Figure 2b: It almost looks like this is a hump-shaped relationship maxing out at mid-concentration. Inhibitory effect of high P? Any reason this particular form of curve was used?

Figure 3: Now by the third figure, I'm really having difficulty connecting individual panels from figures 1 – 3 to stats and model fits from table 1. Make it easy for me (and the other readers) by putting this info on the individual panels.