Biogeosciences Discussions

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Comment on bg-2022-146

Anonymous Referee #1

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-RC1, 2022

General comments:

Tomczyk *et al.* performed laboratory experiments to analyse differences in the temperature dependence of respiration and SRP uptake by litter-associated microbes. They found that SRP uptake had lower activation energy than respiration, indicating an increase in the ratio of C respired to SRP uptake at higher temperatures. They also found that the temperature dependence of SRP uptake increased with initial SRP concentration. This is an interesting and novel study with important results, as most research is concerned with how warming will affect carbon cycling alone. The experimental methods and statistical analyses are appropriate, and the paper is well written with a clear presentation of data and models.

My main concern for discussion is the simulation exercise, which I think was a valuable addition but could be more realistic or its implications less overstated, particularly in the Abstract (L18-22). The stated aim of this simulation is to investigate the effects of C depletion on SRP uptake over the residence time of a pulsed leaf input. The residence time of this leaf is stated in the paper to be 2 years, but the simulation exercise extends for 3000 days. I note that in the example shown in Fig 4b, there is point at around 2000 days at which the cumulative SRP uptake in the colder treatment overtakes that of the warmer treatment. Prior to that, the implications of the simulation are opposite of the eventual conclusion: cumulative SRP uptake is higher in the warmer treatment (as C has not become depleted). Would ending the simulation at either 1 year (when another pulse of litter would become available in the following autumn) or 2 years (the typical residence time of this leaf) be a more appropriate time span to consider ecosystem-scale effects? Although the limitations of the simulation are well considered in the Discussion section, without building in the seasonal variation in litter availability and temperature the simulation may be too simple to inform the conclusions drawn in the Abstract.

Specific comments:

Line 34: I don't quite follow the logic here of the comparison to an autotrophic system. Does this line refer to an increase is autotroph growth or heterotrophic microbial growth? An increase in growth/biomass in any case would lead to a higher demand for nutrients.

Line 70: Might be worth clarifying this is the case for temperate systems in the northern hemisphere that have deciduous riparian vegetation.

L179: Is the 250 mg C m^{-2} based on observations from the catchment or a similar one? Are there measurements to provide a typical mass of detrital leaf litter in each season?

L190: Could the parameters of these different scenarios perhaps be presented in a table? It is difficult to compare from this text (although it is clearer in Fig 4a).

Figure 4b: The methods indicated the simulation started at 250 mg C m^{-2}, however the y axis here begins at 150 mg C m^{-2} at day 0. Please clarify.