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.

There are three important points here that we want to address individually: 1.) how these results are framed in the abstract and title of the manuscript; 2.) the duration of the simulation and when to end it;, and 3.) how the seasonality of leaf litter additions may affect our overall conclusions drawn from the simulation modeling. We address these in turn below.

1. *Framing of simulations*. When drafting this manuscript, we were seeking concise language to differentiate between the instantaneous measurements we made in the lab, and the long-term cumulative uptake dynamics that we simulate. In part because we express the simulation models on an areal basis, we had settled on using “ecosystem” as a descriptor of these rates. However, this comment illustrates that different language would more clearly communicate the results and implications of our study. In revising this manuscript, we are proposing to change the phrase “ecosystem uptake” to “long-term cumulative uptake,” as this describes our results more specifically. Most importantly, we don’t intend to imply that our modeled results are capturing the full complexity of nutrient uptake at the ecosystem scale.
2. *The duration of the simulation models*. The reviewer is correct that if we chose to end our simulation models around the one- or two-year mark then our conclusions from the simulations would be opposite. However, the slow breakdown in our models indicates that the leaves are roughly one-third to one-half broken down at those points in time – so they are not very close to their residence time in our simulations. We discuss some possible reasons for the long residence times in the paper (Page 9, lines 271-276), and don’t contend that our modeled rates represent the actual residence time of these leaves in a stream. If we reduce the residence time of leaves in our models to more closely match observations in the field (by increasing the base rate of respiration, not the response of respiration to temperature), then we see the lines representing cumulative uptake cross at around 100 days into the simulation (instead of at day 2000). We would be happy to include some of these “ambient speed” versions of the simulations in an appendix of a future version of this manuscript and elaborate on this further in our discussion.
3. *Seasonality*. In temperate climates with deciduous vegetation, leaf inputs to streams are highly concentrated at the end of the growing season. Thus, after about a year there would be more leaves added that are also taking up nutrients and contributing to

ecosystem nutrient uptake. This would mean that uptake rates in both the high and lower temperature simulation would increase. Our intention, and how we would like to refocus the paper in a revision, is on the behavior of a cohort of leaves over its residence time in a stream. We hope that the above-mentioned changes to the wording used to describe the results of our simulations will help make the intended scope of our modeling clearer. Including seasonality would be important for predicting ecosystem-level rates through time – but we don't believe it is necessary to understand the overall long-term effects of temperature on cumulative uptake rates. Our models indicate that in the warmer simulation each gram of leaf is taking up less nutrients over its residence time, and this wouldn't be changed by adding seasonal inputs (although areal rates would indeed change). Over a long enough simulation, the inputs of leaves would reach a dynamic equilibrium with decomposition. Because in the warm simulation each gram of leaves is taking up less nutrients over its residence time, the general finding of lower cumulative uptake in the warmer simulation should hold with the addition of seasonal inputs to the model. We would be happy to include seasonal simulations in an appendix of a future version of this manuscript to confirm that our main conclusion is robust to seasonal leaf inputs.

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.

Our intention was draw a distinction between donor-controlled systems, like forest streams, and systems in which the primary energy inputs are from *in situ* primary production. In revising this manuscript we would like to clarify this by changing the line to read "In autotrophic systems, increases in temperature drive increases in primary production that result in predictably higher demand for nutrients (Rasmussen et al., 2011); however, in donor-controlled detrital systems, such as soils and forest streams, increased rates of metabolism stimulated by increases in temperature or nutrients can lead to reductions in pools of the dead organic matter that fuels metabolism, eventually reducing microbial biomass (Walker et al., 2018; Suberkropp et al., 2010)."

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

Thanks for pointing this out. In revising our manuscript, we would like to acknowledge that this mainly applies to deciduous vegetation. We would like to change this line to read "In temperate ecosystems with deciduous vegetation, there is strong seasonality in the input of senescent leaf litter inputs."

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?

250 mg C m^{-2} is a little lower than is typically observed as the sites where we incubated leaves for this study, but the total mass of leaves at the start of the simulation should not alter the main findings of the simulation models. In revising this manuscript, we plan to use values of initial input of leaves from Suberkropp et al. (2010), which also provides monthly estimates of leaf litter standing stocks for more than five years.

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).

We would be happy to add this table to a revised version of the manuscript.

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.

Thank you for catching this, which was a mistake in the analysis. We had made a conversion from grams of ash-free dry mass to g C for the initial mass of leaves in the stream, which we did not need to do. We plan to fix this mistake in a revision of the manuscript, and it will not impact the major findings from the simulations.