

Interactive comment on “Microbial respiration per unit microbial biomass depends on soil litter carbon-to-nitrogen ratio” by M. Spohn

Anonymous Referee #1

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Dr. Spohn investigates the relationship between mass-specific microbial respiration, or $q\text{CO}_2$, and litter chemistry to understand how nutrient availability affects both mass-specific and total respiration. I like the cross-study approach, and I think it is appropriate for this question. While the author presents some interesting and strong relationships, I think further analysis is needed before the conclusions presented can be made. In particular, I would like to see an analysis that models $q\text{CO}_2 \sim \%C + \text{C:N} + \text{microbial biomass C} + \text{temperature} + \text{moisture}$. Given the correlation between C:N and %C (Table 2) its hard to determine if the relationship is spurious or not. It may just be that higher C:N soils have higher C concentrations, and this drives the increase in $q\text{CO}_2$. The author needs to present more detail describing the models that were run, how variables were chosen, etc., which I detail below.

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Were litter incubations done in the field or lab? If a mix, it would be interesting to know the results when lab/field is included as a covariate.

Line 83 “Units were converted to gain microbial biomass C” – I think you mean to obtain. The word gain made me think you were trying to estimate biomass growth for a second.

Your methods section needs to state the models you ran, which terms were included in each model, and how you decided whether to include or exclude a parameter from a model. Did you include all predictors and then remove them based on AIC scores? Did you include only a subset based on some a-priori reason?

Line 105 “other statistically significant correlations . . . are due to autocorrelation.” Did you test for this? How? I think you need to run multiple regression models to get a better handle on this, and check variance inflation factors.

Is there any reason to report Pearson scores rather than R² values?

Lines 125-130- Hessen and Anderson’s arguments contradict themselves. “Disposal of C via respiration may need nutrients to maintain the proteins of the respiratory chain.” For sure this is true, but it is also true of the alternatives these authors suggest such as storage or building defenses, as those also require N-containing enzymes. If a microbe is already ‘fat’ with storage compounds, then overflow respiration seems like a reasonable strategy.

An emerging paradigm is that at low C:N there is lower qCO₂ because decomposer CUE increases when nutrients are more available (sensu Cotrufo et al. 2013 Global Change Biology). This is consistent with the findings of Bjorn Berg, who you cite. I would strongly recommend you include this in your discussion.

Both in the abstract and in the last paragraph of the discussion the author claims that this relationship may explain increased soil C storage under N deposition. However, most C-stored in soils is of microbial, rather than plant origin (sensu Schmidt et al.

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2011 Nature). Given this, can we extend results of leaf-litter studies to make claims about the drivers of soil C storage?

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