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

M. Spohn

marie.spohn@uni-bayreuth.de

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Comment 1: "Dr. Spohn submitted a manuscript regarding microbial respiratory quotients (qCO2) and litter C:N ratios based on a literature compilation. The manuscript is short, simple, and well-focused on an interesting question relevant to Biogeosciences regarding over- flow metabolism in soil microbes. The literature search resulted in a relatively sparse dataset (14 studies with 48 observations) relative to other literature reviews of qCO2 (e.g., 66 studies and 355 obs, Hartman & Richardson, 2013). However this is to be expected, as Dr. Spohn's manuscript focuses on qCO2 in litter, rather than soil. This is an appropriate choice for this manuscript, as the high C:N ratio of litter relative to microbial biomass is particularly relevant to the subject of overflow metabolism. I enjoyed reading this manuscript, and the results are clear and compelling. However I

have some concerns and suggestions that I hope will serve to improve the manuscript."

Answer 1: I would like to thank the reviewer very much for the constructive comments.

Comment 2: "Major concerns: (1) The author introduces overflow metabolism as a controversial subject of current debate; however the existence of overflow metabolism in some organisms is indisputable and has been the subject of several decades of research. Overflow metabolism is clearly supported by molecular biology work in plant mitochondria, as the alternative oxidase and uncoupler proteins allow for the oxidation of organic molecules into CO2 without a corresponding production of ATP (Atkin et al., 2005; Plaxton & Podesta, 2006). There is also a well-developed literature on overflow metabolism in bacteria, particularly E. coli, although the molecular mechanisms seem to be different (e.g., Vemuri et al., 2006). While I understand that the molecular mechanisms are not fully understood in the complex community of organisms that decompose litter, I suggest that the author briefly acknowledge this literature as support for the general concept of overflow metabolism."

Answer 2: It's true that the manuscript reads as if there was still discussion about the existence of the process itself, and not only about its relevance in ecosystems. I will add a sentence on overflow respiration in microorganisms referring to the mentioned study by Vermuri et al. (2006) and to two review papers on the subject (Russell and Cook, 1995; Teixeira de Mattos and Neijssel, 1997). Moreover, I will state that there is debate about the relevance of this process in ecosystems, but not about the existence of the process in microorganisms itself.

Comment 3: "(2) Line 53-55. There is little reason to expect overflow metabolism to be forest-specific, so why limit the data compilation to the forest literature? Consider broadening the analysis to include studies regarding litter decomposition in other systems (e.g., grasslands) and residue decomposition in crop systems."

Answer 3: I did not restrict the analysis to forest soil litter layers. In fact, some of the data come from the soil litter layer of Coco plantations and of a heathland (see Table 1).

I did not find more data from ecosystems other than forest that met the criteria of the literature search. Since this analysis deals with soil litter layers, studies that measured the qCO2 on plant detritus were not considered (and I am also not aware of any study that measured the qCO2 on plant detritus).

Comment 4: "(3) Lines 119-130. This reads like the author is pursuing to discredit the notion of overflow metabolism, when the results clearly support it. I suggest the author clearly state that the results were consistent with overflow metabolism in the decomposition of forest litter, possibly in the first and/or last paragraphs of the discussion section. Furthermore, I am unconvinced by the argument on line 127 that "... microorganisms may use C that is in surplus to their demands of somatic growth for promoting their fitness by C storage, buildup of structural defenses, viral repellents or establishment of symbiosis." The additional processes listed by the author are not infinite C sinks. Consider the case that the microorganisms have already satisfied the C demands of structural defences, viral repellents, etc; what should they do with the "extra" C in this case? The concept of satisfied C demands need not be confined to somatic growth."

Answer 4: In the discussion section of the manuscript, three possible explanations for the main finding are critically discussed. All three discussed mechanisms could potentially explain the observed relationships. In order to be more explicit, I will add the following sentence add the end of the discussion of the three possible explanations. "All three mechanisms can explain the observed relationship between the qCO2 and the soil litter layer C:N ratio; and based on the data presented here it cannot be concluded which of the three mechanisms is most relevant to the observed relationship."

The reviewer is right in saying that there are limits to the amounts of C that can be stored by microorganisms or invested into buildup of structural defenses, viral repellents or establishment of symbiosis. Though not infinite, the amounts of C that microorganisms can invest into establishment of symbiosis, the release of low weight molecular substances or communication are likely very large. I will add a sentence, stating that there are limits to the amounts of C that microbes can store and likely also

to the amounts of C microbes can invest into buildup of structural defenses, viral repellents or establishment of symbiosis. The size of these limits, i.e. the amounts of C that microbes can invest into other processes than somatic growth, remain to be explored.

Comment 5: "Minor concerns: (1) The authors report a three-part analysis showing that (1) qCO2 was positively correlated with litter C:N, (2) basal respiration was positively correlated with litter C:N, and (3) microbial biomass was not correlated with litter C:N. This exploration of the data was very well done. The reader may be able to see this most clearly if point #3 was demonstrated with a figure. Please consider a 3-panel figure with qCO2, basal respiration, and microbial biomass all plotted in relation to litter C:N."

Answer 5: I considered adding another figure, showing that there's no correlation between the soil C:N ratio and the microbial C:N ratio. I decided not to do this because it is common practice to only show correlations, but not the absence of correlations in figures.

Comment 6: "(2) lines 106, 113- tense change; consistently use the past tense. It is common practice to discuss previously published literature in the present tense to recognize the current relevance of the established research. However it is more appropriate to discuss the current manuscript in the past tense."

Answer 6: Yes, I will correct this.

Comment 7: "(3) Line 137. "Adapt" has a specific biological meaning that is not appropriate here"

Answer 7: That's true. I will replace "adapt" by "adjust".

References

Russell, J. B., and Cook, G. M.: Energetics of bacterial growth: balance of anabolic and catabolic reactions, Microbiol. Rev., 59, 48-62, 1995.

Teixeira de Mattos, M., and Neijssel, O.M.: Bioenergetic consequences of microbial adaptation to low-nutrient environments, J. Biotech., 59, 117-126, 1997.

Vemuri, G. N., Altman, E., Sangurdekar, D. P., Khodursky, A. B., and Eiteman, M. A.: Overflow metabolism in Escherichia coli during steady-state growth: transcriptional regulation and effect of the redox ratio, Appl. Environ. Microbiol., 72, 3653-3661, 2006.

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