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# *Interactive comment on* "Preferential protein depolymerization as a preservation mechanism for vascular litter decomposing in *Sphagnum* peat" *by* Hendrik Reuter et al.

**Anonymous Referee #2**

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The main aim of the study was to evaluate the fate of plant litter nitrogen in a decomposition experiment involving litters and peaty soils with contrasting N status. It was necessary to distinguish between two fractions of protein nitrogen in the litter: (1) remaining original N that has not been depolymerized by decomposers' enzymes and (2) newly synthesized microbial N. The authors proposed a novel approach how to distinguish the two fractions; they measured precise FTIR spectra to evaluate peaks of total protein nitrogen and microbial DNA phosphorus (assuming that the DNA P is associated only with the microbes). Assuming constant microbial N:P stoichiometry they could express the microbial N fraction. I am not a microbiologist, so I am not able to review critically the assumptions leading up to the evaluation of preferential protein



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depolymerization. However, I appreciate the careful explanation of all the evaluation steps supported by references. The manuscript is well and clearly written but I would like to discuss following issues.

How is the microbial N invested in extracellular enzymes accounted for? How relevant is this fraction in the evaluation of the N fate in the decomposing litter? How can it differ in N-poor/rich soils and litters? How this fraction can affect the proposed method leading to the evaluation of preferential protein depolymerization?

The concept also does not mention that the extracellular enzymes may mediate N acquisition from dissolved organic N, which was not analyzed in the soil water. How relevant is this N pool in the tested soils?

Why anoxic conditions were chosen for the experiment? Most plant litters, also in peatland habitats, are first exposed to oxic conditions. Do you think the conclusions are fully applicable also in oxic decomposition where fungal decomposition often prevails?

Other comments: Chapter 2.3 Infrared Spectroscopy: I think that more details about the target compounds and their absorption bands can be provided here in the method description than only later in the Results and Discussion.

P4, L21: How effective was the 17-h period in leaching the litter? Is it possible that a significant proportion of the mass loss can be still attributed to the leaching and not entirely to microbial activity?

P4, L23: How was the rhizome litter defined? (I expect that a continuum between living and highly decomposed rhizomes can be found in the soil).

P12, L12 and Figure 4b: The linear model has the intercept very close to zero (as indicated by the trendline in the graph), obviously statistically not different from zero. What is the relevance of the zero intercept? Does it support the assumption of the entirely microbial origin of the rhizome litter N and P?

Technical comments: Table 1, first column: “soil substrate” can be clearer (as it is used





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also in the text)

P4, L4: “N mineralization/immobilization”: does the slash sign denote a ratio or something like “and/or”?

P4, L12: Replace “sedge-brown moss peat” by “sedge–brown-moss peat”

P7, L1: The C/N in the senescent leaves was measured after the leaching? If so, “leached leaves” can be used.

P19, L12: Although the data on CuO-oxidation lignin monomer products were not used in the paper, the supplement should contain a description of the method (or a reference).

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