General comment

The effects of global change on nutrient release from litter layers are certainly an actual and valid research objective in northern ecosystems The study presented by Hagedorn et al. contains interesting data, which seem to merit publication. However, the analysis of different ecotones with different vegetation will always lead to highly significant results. This means that the presentation of the data needs considerable improvement as the authors seem to be partly lost in data. They should consider shortening the text and removing some approaches, which do not add much information to the study.

Specific comments

The font is too small to allow easy reading of the PDF printout.
I would prefer continuous line numbering.
L38-41: Awkward statement! Microbial biomass and microbial residues also need to be mineralized for releasing nutrients.
L59: There is too much focus on overflow respiration in the current manuscript, which occurs mainly when high concentrations of low molecular weight organic substances are available to microorganisms. The authors should consider extracellular polymeric substances (EPS), fungal vacuoles and bacterial storage components, such as poly-hydroxybutyrate, as reasons for stoichiometric variability of soil microorganisms. Also, the presence or absence of Mn and Cu has often strong effects on lignin decomposition in litter layers.
L97-100: Awkward statement! Rephrase!
L143-144: I do not understand the reason for this initial leaching.
L152-153: Please, give the range of NaOH molarity.
L189: Brookes et al. (1985) and Vance et al. (1987) used 0.5 M K2SO4 for extracting mineral soil at a ratio of 1 to 4 (soil to extractant). The current authors extracted litter at a ratio of 1 to 20 (litter to extractant) with 0.05 M K2SO4. This deviation from the original references is based on previously published work in determining microbial biomass in litter, which should be cited in all fairness.
L191, L219, L226: remove “Corp”, “Inc”, and “Limited”!
L193: The kEC, kEN, and KEP values are not factors. The kEC value of 0.45 has been proposed by Wu et al. (1990), which should be cited.
L215: The formula should be given.
L243 and throughout the manuscript: The metabolic quotient is defined as basal respiration / microbial biomass C (Anderson and Domsch, 1990) and should not be used for the microbial use of a freshly added substrate.
Tables 1, 2, 3 and 4: The decimal numbers should be restricted to two, not in bold, non-significant numbers should be presented as NS.
L284-287: This is not a Results statement. Move to Materials and Methods or the Discussion section!
L306-309: It is impossible for me to get a clear information out of this poorly lay-outed Figure 1. The data of the endpoints should be given in a table.
L311-314: This is not a Results statement. Move to Materials and Methods or the Discussion section!
L325-329: Also, the layout of Figure 2 is poor. It does not make sense to adjust C, N, and P release to an identical scale. In addition, the figure contains excessive legends.
L326: I miss information on the DOC/DON ratio as quality index for the measurements.
L341-3??: I have doubts that these presentation of correlation coefficients is valid as the data are presumably not normally distributed as those presented in Figure 7.

L406-4??: Q10 values of MBC, MBN, and MBP should be removed.
L423-425: Figure 7 should be removed.
L426-428: It is not possible to distinguish the site-specific symbols using a greyscale print-out.
L435-436: Trivial statement! Remove!
L462-464: Awkward statement rephrase!
L490, L491, L502: “microbial biomass” not just “microbial”!
L579: Again, there is too much focus on overflow respiration. It is possible but cannot be clearly concluded from the current data.
L584-587: This statement is not a Conclusion. I miss a clear “take-home” message.