

Interactive comment on “Litter decomposition rate and soil organic matter quality in a patchwork heathland of Southern Norway” by G. Certini et al.

Anonymous Referee #2

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General comments

The authors aimed to separate general factors affecting decomposition rate of plant litter in a heathland: the intrinsic litter quality and the decomposition potential of the habitat represented by three vegetation types. They established a short-term, reciprocal litter bag transplant decomposition experiment and determined selected chemical characteristics of the plant litter, soil environment, and soil organic matter. As the three vegetation types reflect availability of soil water, the results can be used in estimation of expected climate change – drought – on soil C dynamics. Although the authors do not employ novel approaches to test a ground-breaking hypothesis, I consider this research as a valuable contribution with interesting consequences, worthy of publishing. On the other hand, the manuscript suffers from several confusing inaccuracies, omis-

C73

sions, inconsistencies or contradictions, related particularly to Materials and Methods section.

Specific comments

Introduction

Page 270, Line 23–24. “... little work focused on the solid phase of SOM... , most of the research focused on SOM storage” – I do not see logical compatibility between “solid phase of SOM” and “SOM storage”.

Materials and Methods

I think the readers would appreciate to know the species identity of *Sphagnum* moss(es) dominating in *Sphagnum* patches and notably used in the decomposition experiment. Different species may have contrasting ecological strategies, which are reflected also in their chemical composition and decomposition rate.

P. 274, L. 25–26. Cellulose and hemicelluloses represent major polysaccharide fraction of plant matter. How do the two regions (60–90 and 90–110 ppm) differ, i.e. which polysaccharides are represented by 90–110 ppm?

P. 275, L. 5. How were the twigs of *Calluna* treated? Were the leaves separated as done for the previous characterisation? How old were the twigs – current year's growth? I am also not sure whether the “most recently formed biomass” should be considered as litter. It can be in case of *Molinia*, but probably not in *Sphagnum* (the capitulum does not senesce) or *Calluna*.

P. 275, L. 11. “... litterbags of each litter type were installed on the ground” vs. Figure 3 caption: “Residual mass in buried litterbags...” or P. 268, L. 10–11: “placing litterbags ... under each type of vegetation cover”. Where were the mesh bags placed? Buried? How deep?

Statistical analyses: The statistical design is not clear and completely described. How

C74

was the decomposition rate and litter C and N changes evaluated? Are there any random factors in ANOVA designs?

Results

P. 276, L. 6. There is no information in Materials and Methods that (why, how) the belowground biomass was also sampled and analysed. Moreover, it is confusing to present the plant C/N here (and in Table 1), although the values are not comparable with the litter chemistry presented in Fig. 4, which is based on different plant material (as explained in Discussion). This should be clarified earlier (in M&M).

P. 278, L. 17–18. I do not see any “drastic decrease” in *Molinia* and *Sphagnum* C content in Fig. 4. They had just lower initial C content.

Discussion

P. 280, L. 9. Wieder and Starr (1998) did not present Hot water C in % of SOC and they also used different extraction procedure (100 °C for 3 h instead of 80 °C for 16 h). Are such results really comparable?

P. 280, L. 19–20. “Approximately half”, which was 52 % in non-Sphagnum soils is not greater proportion than 49 % in Sphagnum soils (according to Table 2; especially without statistically significant difference).

P. 281, L. 8–9. Neither Verhoeven and Toth nor Scheffer et al. mention sphagnum (pectin-like) polysaccharides. According to Hajek et al., those polysaccharides rather hamper decomposition than being hard to decompose (which is also true but not relevant because they do not prevail in the biomass).

P. 281, L. 16–24. *Sphagnum* decomposition was slow in *Calluna* site. Is it possible that simply drought slowed the decomposition, rather than hypothetical antibiotic substances?

P. 282, L. 15–24. The last part of discussion is lengthy, describing details of the cited

C75

reference – this part can be reduced, or even omitted (sounds speculative).

Tables and Figures

Table 3. It is probably Carboxyl, not Carbonyl C in the range of 162–190 ppm.

Technical comments

P. 273, L. 3 and 18: the unit of relative centrifugal force is “*g*” or “ $\times g$ ” (*g* is italicized)

P. 278, L. 8. The paragraph should be split to separate NMR and decomposition.

Figure 4. “. . . trial of Fig. 3. . .”?

Personal note

I regret that the NMR spectroscopy was not applied also on the incubated litters after, e.g., one year. This would provide valuable, novel insight in degradation of such contrasting litter types. I would also prefer to have longer incubation period of the litter bags (e.g., 2 years instead of the 9 months).

C76