

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

G. Certini et al.

certini@unifi.it

Received and published: 20 October 2014

Anonymous Referee #2

Received and published: 14 August 2014
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 ap-

C134

proaches 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, omissions, 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 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

C135

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 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).

Replies to Referee #2

Introduction We thank the Referee because the remark is correct. We have in fact deleted the last part of the paragraph starting with “In Southern Norway...” as it did not contain any essential information and was also somewhat confusing, as pointed out by Referee #2. Relevant information from earlier studies at this site is included in the Discussion.

Materials and Methods The species name of Sphagnum has been included as sug-

C136

gested. P274, L25-26: We have explained in the text the differences in the two chemical shift regions of the NMR spectra 60-90 and 90-110 ppm. P. 275, L. 5: We have deleted “most recently formed biomass” and added information about how the different litters were sampled and treated. P. 275, L.11: The litterbags were not buried, but left “en plein air” on the ground. We have corrected the Caption to Figure 3 and clarified the procedure in the text. Moreover, the sampling and analysis of the belowground biomass (roots) has been described. We revised the section devoted to the statistical analysis in order to make it clearer and more informative.

Results: P.276, L6: The differences in the material presented in Table 1 and the material used in the litter decomposition study (Fig. 4.) were discussed more thoroughly in the Materials and Method section. P. 278, L. 17-18: The word “drastic” was indeed not fully appropriate and, hence, it was substituted by “marked”.

Discussion: P. 280, L.9: We have deleted the reference to Wieder and Starr (1998), as the data there reported are not comparable with ours due to methodological differences. P. 280, L. 19-20: The Referee’s observation was correct. The last part of this sentence was in fact deleted. P. 281, L. 8-9: We apologize for forgetting to correct this point before submitting the paper. In fact, the references Verhoeven and Toth, and Scheffer et al. should not have been listed here. They were thus removed. We have also corrected our statement on polysaccharides and decomposition, as suggested by the Referee. P. 281, L. 16-24. This part has been deleted and we have followed the Referee’s suggestion to interpret the differences in decomposition more as a consequence of drought rather than of antibiotics. Indeed, we have observations to support that drought plays some role in slackening decomposition but none to support the same for antibiotics. P. 282, L. 15-24. This part has been shortened, as requested by the Referee.

Tables and Figures: Table 3. The Referee’s comment is correct and the heading of the 162–190 ppm column has been corrected accordingly.

C137

Technical comments: P.273, L.3 and 18: g was italicized. P.278, L8: The paragraph was split, as suggested by the Referee. Caption of Fig. 4: "trial of Fig. 3" has been changed to "biomass in the litterbags".

Personal note We understand the Referee's regret, which is ours as well. Unfortunately, sometimes the limited budget and time available as part of a project requires painful sacrifices.

Please also note the supplement to this comment:

<http://www.soil-discuss.net/1/C134/2014/soild-1-C134-2014-supplement.pdf>

Interactive comment on SOIL Discuss., 1, 267, 2014.