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

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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 #1

Received and published: 6 August 2014 The manuscript “Litter decomposition rate and soil organic matter quality in a patch- work heathland of Southern Norway” deals with the role of different driving factors on litter decomposition and the relations between vegetation covers and SOM dynamics. The manuscript is well writing, of a broad international interest and address relevant scientific questions within the scope of this journal. Although it does not present nei- ther novel concepts nor a new approach to

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the study of the litter decay, the relationship between soil organic matter composition, litter decay and vegetation cover is very interesting. The main concern with this paper deals with the lack of enough explanation on the methods used to reach the objectives. Furthermore, some specific objectives that are mentioned within the manuscript (ie: “site effect”, “drainage effect”, “vegetation effect” on litter decay) are not clearly showed at the end of the introduction C50 SOIL 1, C50–C52, 2014 Interactive Comment Full Screen / Esc Printer-friendly Version Interactive Discussion Discussion Paper Open Access section. Authors should specify clearly which are they looking for and explain better the experimental design justifying everything that they do and do not do. Some more statistical analysis are needed (see below). Objective given (page 271; lines 1-2) is not enough clearly formulate. Methods and experimental design: Some more explanation is needed in order to make the manuscript more easily understandable by the reader: You take samples from the dominant vegetation (three site per dominant vegetation). However, in your results (Figures 3 and 4) you show different vegetation combinations (three per dominant vegetation). You should explain this in the experimental design and soil sampling. Did you take the same number of disturbed than undisturbed soil samples (page 272, lines 15-24)? Which kind of analysis did you do in the disturbed samples?. Explain in this section. If your sampler cylinders are 7.0 cm high it should be the uppermost soil layer but not 10 cm as you show in Table 3. The structure of section 2 should be reformed. “soil water analysis” is a confusing title here. It should be better to separate between 2.2. Experimental design and field measurement; 2.3. Soil and vegetation sampling and analysis; 2.4. Litter decomposition experiment. Litter decomposition determination is not well explained: authors do not explain the number of litterbags established per each dominant vegetation combination. Furthermore, they do not explain the initial analysis for characterizing the plant biomass immediately before to start the decomposition experiment. This point is very important because at the discussion section (page 281, lines 25-29 and page 282, lines 1-8) they explain that the vegetation taken for doing basic characterization is not the same that the vegetation used for the decomposition experiment. At least,

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authors should explain this aspect in the methodology section and why they did not use the same material for both things. This fact might make the comparison between vegetation types in decomposition more difficult to interpret. Other important aspect missing in the decomposition experiment is the “ash correction” which means that you have to take into account that during the incubation the litter gets mixed with a significant amount of mineral soil. Harmon et al 1999 give a way to sort out the proportion of the litterbag sample mass that is actually litter. Statistic is also not well explained. Some more information is needed: for example did you assume ANOVA assumptions without testing it? Furthermore, how do you check the “site effect”, “soil drainage effect”, “vegetation effect” . . . on the litter de- composition? Results and discussion: Some contradictions are given in relation to the organic forms of N: for example: page 276 lines 10-15; page 280, lines 1-2. You use the term “SOM” in the results but you analyzed carbon (page 279, line5, 11). Furthermore, the first paragraph from this page is difficult to understand. Line 11: “Sphagnum was richer in SOM than..” but not significant differences among them were detected. Page 279, line 17: insert a reference about the soil drainage as a driving factor of decompo- sition. In the same page, lines 16-25 a discussion about the DOC variability is given but at the end you do not conclude anything about it and how it might affect your results. In page 280, line 15-20: a discussion about the hot water extracted is given. What do the authors mean? Is it relevant for the discussion?. In page 281, Lines 1-10, the inconsistency between the analytical results (CNMR and chemical analysis: tables 3 and 2, respectively) and litterbags experiment results should be better explained. Fur- thermore, it seems that the litterbags experiment does not contribute to reach your final conclusion. In this sense, there are some speculations that are not based on your data (example: about the antibiotic substances as inhibiting factor for development of micor- rhizal). You should explain it better. Table 1: you do not show the significant differences between vegetation types probably, because you only took 2 samples per vegetation type. You did not explain this in the methodology section. Table 2: letters indicating significant differences are not correct. You should homogenize if “a”, “b” represent the lower or the

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higher values. Table 3: you do not show the significant differences among vegetation types and/or soil. The reference: “Klavina et al 2012” (page 286, line1-4) is not within the text.

Replies to Referee #1

General comments: The referee asks for clarification of objective and for more explanation of the experimental design and method. We noticed from the referee's comments that there have been some misunderstandings and therefore acknowledge that we must explain and clarify what we have done better. We hope we have achieved this in our amended manuscript. Concerning the general objective of the study, we think it is sufficiently clear: it “was to assess whether in this environment the current vegetation cover is a good proxy for SOM quality and dynamics”. In the revised version of the ms. we actually tried to better explain the experimental design and our *modus operandi*, hence including also the required information about the number of disturbed samples taken or the kind of analyses we did on them. The referee has probably misunderstood the approach used in the litterbags experiment, where “we installed 32 litterbags of each vegetation type on the surface of each sampling site, except *Calluna* under *Sphagnum*, since a substitution of *Calluna* by *Sphagnum* was judged to be highly improbable.” (see current lines 281-284). This is the reason why in Figures 3 and 4 we show three trend lines per dominant vegetation. Right is the comment dealing with the 7.0 cm high cylinders. Now we specified that we completely inserting them “at about five cm depth in each soil profile”. We have changed the headings of the Materials and Methods section to accommodate some of the shortcomings pointed out by the Referee. The litter decomposition experiment and the characterization of the litter prior to the decompositions study have been explained better than in the previous version of the ms., as requested by the Referee. We are not sure which publication by Harmon et al (1999) the Referee refers to, but of course the analysis had been harmonized to facilitate comparison between samples. This is now explained more in detail in the text. However, the problem associated with

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the obvious presence of mineral material into the litterbag is not particularly relevant in these soils, given the mesh size, lack of bioturbators and the thickness of the underlying organic horizon. We have explained more of the statistics used in the litter decomposition study as the Referee asked for. Concerning the Referees' comments to P.276 L. 10-15 and p 280 L. 1-2, we think they may be due to a misunderstanding. P 276 was in fact referring to the N measured in the solid phase of soil, while P. 280 was referring to the soil solution and, therefore, to different pools of nitrogen. We hope that the Materials and Methods were made clearer in the revised version of the ms. We used SOC and SON to refer to two different components of the soil organic matter (SOM). We checked the text over to assess if there are places where this binary terminology may be misleading for the reader. P. 279. L. 1 and 11: Both sentences appropriately modified. P. 279 L. 17: We inserted a couple of adequate references (Hobbie et al, 2000; Laiho, 2006) for what we, honestly, took for granted. The criticism on the poor explanations provided at pags. 280-281 was partly justified. As a consequence, we reorganized this part, for the purpose to also solve the issues pointed out by the Referee. For example, a few additional considerations on the connections and incongruences between the results from the NMR investigation, the chemical analyses, and the litter decomposition study have been provided in the text. Moreover, we removed the sentence about the antibiotic substances as inhibiting factor for development of micorrhizal hymenomicetes, which actually based on no bodies of evidence. Concerning the criticism of what hot water mean and what is its signification for the study, please note that a bit further we state: "Some authors have proposed hot water extraction of SOM as a method to measure the labile SOM pool..." In the revised version of the manuscript we actually discussed a bit better the inconsistency (and the consistence as well!) between the analytical results (NMR and chemical analysis) and the litterbags experiment results. Nevertheless, we do not agree at all with the Referee's comment that it seems that the litterbags experiment does not contribute to reach our final conclusion. On the contrary, just thanks to such an experiment we verified that litter decomposition in this environment is not

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chiefly driven by the litter composition. Concerning the sentence about the antibiotic substances please notice – as already said above – that we removed it. The Referee's comment to Table 1 is based on a misunderstanding. Behind each mean there are 3 replicates (vegetation taken from the 3 pit for each vegetation site). These samples were not analysed to look for significant differences between different plant species but was included to illustrate the gross quality of the litter they release to soil. We made clearer this point in the Materials and Method section. In Table 2 we have homogenized the use of "a" and "b", indicating the significant differences as suggested by the referee. For the comment to Table 3, see the above reply. P. 289 L. 1-4: The referee has perhaps overseen the reference, which was actually present in the text (L. 453).

Please also note the supplement to this comment:

<http://www.soil-discuss.net/1/C128/2014/soild-1-C128-2014-supplement.pdf>

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

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