

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

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The manuscript “Litter decomposition rate and soil organic matter quality in a patchwork 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 neither novel concepts nor a new approach to 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

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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, 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 ex-

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ample did you assume ANOVA assumptions without testing it? Furthermore, how do you check the “site effect”, “soil drainage effect”, “vegetation effect” . . . on the litter decomposition? 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, line 5, 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 decomposition. 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. Furthermore, 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 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, line 1-4) is not within the text.

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