Biogeosciences Discuss., 12, C5862–C5864, 2015
www.biogeosciences-discuss.net/12/C5862/2015/

Biogeosciences
Discussions
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Interactive comment on "Time since death and decay rate constants of Norway spruce and European larch deadwood in subalpine forests determined using dendrochronology and radiocarbon dating" *by* M. Petrillo et al.

Anonymous Referee #1

Received and published: 29 September 2015

The authors present a straightforward and interesting study about the decay of coarse woody debris in subalpine forests. The approach to determine the age of deadwood with dendrochronology and radiocarbon dating is rarely used and it delivered comprehensible and necessary new results. Wood decay is dependent of various factors, and the spatial heterogeneity needs to be considered, therefore I mainly suggest delivering more information about the sampling protocol, the sampling sites and the wood composition. The comparison with organic matter decomposition in soil is not helpful, especially because there a different models and time-frames of SOM degradation in

discussion, which were not and cannot be considered here. For more detailed comments see below.

p. 14799, l. 1: Use "amount", not size. Or do you mean the size of the individual pieces of deadwood? l. 3: Rather use "rarely" investigated, not poorly. l. 14: This is not clear, are the ages similar or varying, or similarly varying? l. 19: Review the sentence (Cellulose and lignin time trends half-lives . . .).

p. 14801, l. 8-12: Please cite the original references, it should be clear who are the authors that were subsumed under "some" or "others". l. 17: Please give a short definition of CWD, and mention it earlier. I also suppose that the studies that were cited before consider CWD?

p. 14802, l. 15-16: I would suggest not to relate your results to soil organic matter, especially without giving any references for your statement. The stability of SOM was investigated in numerous studies, leading to different models and questions concerning the importance of physical protection in relation to chemical recalcitrance.

p. 14803: The sampling protocol should be complemented with information about the size of your sampling plots and the number of CWD samples taken. Interestingly, larch was mainly sampled at the South-facing sites while spruce was mainly sampled at the North-facing sites. This information can be derived from the tables but it should be mentioned in the text since it is important for the interpretation.

p. 14804, l. 15: The methods to determine cellulose and lignin content are described here although the results have been published in another paper. I would suggest adding the results in a table (e.g. Table 5). Otherwise you should just mention that cellulose and lignin content (and also density) have been measured according to Petrillo et al. (2015) and remove the detailed description of the lab procedures. In the methods section: twice the same solution (5% NaOH), is this correct?

p. 14806, l. 1: It would be helpful to add the density values, e.g. in Table 5.

p. 14807: The ages of the CWD cluster in two age groups of about 30-50 years and 205-220 years, considering the first row of average age in Tab. 6. How can this be explained, is it a result of the wide range of calibrated ages?

p. 14811-12: The paragraph on soil organic matter is unnecessary as it is now. As mentioned above there are totally different processes and factors involved that influence the degradation or preservation of SOM compounds (see, e.g., Lützow et al. (2006) in European Journal of Soil Science 57; or Marschner et al. (2008) in Journal of plant nutrition and soil science 171). The residence times of different compounds is a matter of debate, and your comparison (l. 9-10) is made without relating your results to actually published results for SOM in detail. It might be more interesting to relate your findings to recent work on carbon emissions or forest ecology.

Interactive comment on Biogeosciences Discuss., 12, 14797, 2015.