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Biogeosciences Discussions

Interactive comment on "Litter quality and pH are strong drivers of carbon turnover and distribution in alpine grassland soils" *by* K. Budge et al.

K. Budge et al.

jens.leifeld@art.admin.ch

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R1 General comments: R1 This paper investigates the distribution, quality (i.e. state of transformation) and turnover of SOM (with an emphasis on the labile POM-C pool) in alpine grassland soils across a small elevation gradient, and how these variables relate to elevation and different site factors. This is an interesting research topic as little is known about turnover rates of plant residues and SOM stabilization mechanisms and controlling factors at higher elevations in the cold-temperate zone. The study seems to be an extension of the work initiated by Leifeld et al. (2009), but at higher elevation. While both papers investigate turnover rates of different physical SOM fractions in (sub)alpine grasslands, this paper by Budge et al. zooms in on the 'quality' of the SOM fractions (by chemical characterization) to investigate decomposition stage of dif-

ferent SOM pools across the sites, and the possible importance of the strong acidity of these alpine soils and the distinct vegetation (affecting litter quality) in controlling SOM turnover in these ecosystems. R1 The paper is well written, logically organized with clear referencing to related work and clear indication of how its research findings add to our current understanding of SOM dynamics in alpine ecosystems. Valid published methods were used for SOM fractionation, characterization and determining turnover times (MRT's). MRT's were estimated by means of 14C dating (bomb 14C models). This method requires steady state conditions, which was fulfilled for the alpine grasslands included in this experiment (no historic land-use change). R1 One concern I have is about the way the data is analyzed statistically. No statistical analyses were performed to compare variables across the elevation gradient (SOC, POM%, MRT), to compare variables across fractions (e.g. SOM quality, MRT of fractions) or across depths (e.g. SOM distribution, SOM fraction quality). The only statistical analyses done were correlations between variables, though these were not always appropriate (e.g. correlations between MRT and soil depth; an ANOVA seems to make more sense).The findings of this paper should be supported by appropriate statistical analyses on the parameters measured in this study to allow proper interpretation of observed differences among fractions, sites, soil depths.

Authors: As specified below, we will provide more detailed statistical analysis using ANOVA in the revised paper.

R1 Specific comments: R1 1. Title: The title is a bit strong in its statement and I am not convinced that this correctly reflects the findings of the paper. According to the title, the paper shows that pH and litter quality are strong drivers for SOM turnover and distribution in these alpine grasslands. However, litter 'quality' was only measured in terms of CN ratio (fig. 3) (but not really discussed in terms of the differences across sites), but no further characterization was done on the litter fraction. The conclusion of litter quality being a strong driver was only made based on species abundance differences. Also no correlation was made between pH and SOM 'distribution' over the different fractions (although easily done). Even the strong correlation between pH and bulk soil MRT does not imply causation. Perhaps a title that is not as conclusive would be more appropriate.

Authors: The title will be changed/simplified to indicate key points presented in the manuscript but will not involve a debatable interpretation of the results.

R1 2. Soil analysis (pg. 6212): fine earth was defined as a fraction 0-200 um in size. What happened to the 200-2000 um fraction? Why was this not included in the analysis? Or is this just a typo? Also, was only the root/litter fraction < 200 um kept? Or should this also be root/litter fraction < 2 mm?

Authors: This was a typo mistake with the size of the fraction and therefore the numbers in the text have been corrected and the same unit (μm) has been used throughout to avoid confusion. The soil analysis section will also be rewritten to more clearly define what constitutes the stone, fine earth and root/litter sections.

R1 3. 13C NMR spectroscopy (pg. 6216): A short description on which samples/fractions, sample preparation (if any) for 13C NMR analysis, is lacking. From table 3, it looks like this analysis was done on just a few samples/fractions and without replication (no standard errors?). Also, a reference for the last sentence (pg. 6216, ln. 23) is missing (e.g. Baldock et al., 1997, Kölbl and Kögel-Knabner, 2004, among others).

Authors: Samples were chosen for 13C NMR spectroscopy to correspond with those measured for 14C content, this is stated in the revised text. Sample preparation only involved milling, which had been done previously, and due to costs analysis was not replicated, which is also now stated in the text. An appropriate reference will be added.

R1 4. Plant cover (pg. 6217): This section needs a bit more detail on the Ellenberg's indicator system. Even to just indicate what a higher vs. a lower Ellenberg's indicator means (as shown in figure 2 - would be also useful to include this data interpretation in

legend of figure 2).

Authors: A description of the Ellenberg indicator values will be added to the text and to the legend below Figure 2.

R1 5. Statistical analysis (pg. 6217): statistical analysis to compare variables across sites, fractions or depths is missing. This needs to be added to support the data trends described in the results.

Authors: ANOVA of factors site and soil depth will be included in the revised version.

R1 6. Results - Table 1: It would be good to see the variability in soil temperature and soil texture (e.g. sand, silt, clay content - did this differ across sites?) across the different sites. Soil textural differences could also explain some of the SOC differences between these sites. But this information is lacking.

Authors: Soil temperature was measured over a period of 13 months at the highest and lowest sites only and varied between season, as would be expected. However, values did not differ significantly between these sites and therefore did not reflect the variation in air temperature expected between the sites of the elevation gradient. Comparison of soil temperature with other variables measured did not indicate any significant results and therefore soil temperature was not discussed in the manuscript. Influence of soil temperature on SOC distribution across the elevation gradient is not feasible without values for all sites. It was not possible to measure soil texture at all sites and depths due to limitation of sample material, therefore replication was also not possible. Soil clay contents did not indicate a significant relationship with SOC content when statistically analysed. However, further details which are available on soil clay content will be added to Table 1 for the benefit of the readers.

R1 7. Results (pg. 6219, ln. 18): indicate that MRT is 'negatively' correlated with O-Alkyl%.

Authors: Detail will be added to the text.

R1 8. Since the authors describe a lot of correlations in the text, it could be good to present an overview table of all the correlations done between variables, with r-coefficient and p-values, but perhaps limited to the most important correlations, e.g. MRT with analysed soil properties (pH, clay content,soil temperature, nutrients), MRT with SOM quality indices (C/N, Alkyl-C/O-Alkyl-C,...), phytomass and annual C input with soil properties,...

Authors: Significant correlation relationships identified have been reported and the important relationships discussed in the text. While it may also be useful to present them in a table format there are already many tables attached to the manuscript and therefore an extra table would add unnecessary length to the manuscript.

R1 9. Correlation between bulk soil MRT and soil depth does not make sense. An ANOVA seems to be more appropriate.

Authors: We agree, see also our reply above.

R1 10. Discussion - pg. 6221, ln. 7: were any correlations done with soil temperature? Again, an overview table with correlations between SOM variables (POM-C%, MRT, etc.) and soil properties (temperature, pH, clay content,...) would be useful.

Authors: Soil temperature values are only available for the top and bottom elevation sites and only for a limited time period whereas 14C-based MRT integrates over decades to centuries. Therefore correlation analysis would not be appropriate in this case.

R1 11. Pg. 6221, ln. 20: this would read better if stated "... in the range of pH 4-5, a decrease of ca. 0.5-1 units between the higher and lower sites relative to the less acidic middle site..."

Authors: Detail will be changed as suggested.

R1 12: Pg. 6222, ln. 18: Authors mention the input of N from legumes as a possible cause for higher phytomass and C input at the mid-elevation site. It would be useful to

see also the soil N data in table 2.

Authors: Soil N concentration will be added to Table 1.

R1 13: pg. 6224, ln. 1: Could this observation (higher degree of transformation of POM in alpine vs. temperate soils) be a result of textural difference? Kolbl and Kogel-Knabner (2004) indicated a lower degree of POM degradation was associated with higher clay contents. The soils in this study had low clay contents (10%), though it would be good to have more detail on the texture of these soils. Also, the data in Table 3 does not have any standard error information, so it is hard to know what the variability is on this data to see if this observation is really valid. Was this analysis not replicated on samples? Fig. 5 and 6: were replicate samples pooled for both figures (from methods section, it seems like they were for figure 6: bulk soil)? Otherwise SE (and stats) are missing. Perhaps explain this in the figure legend. Should the 5-10 depth results for the fine bulk soil be the same in both figures? They seem to differ slightly.

Authors: We do not think that the higher degree of transformation is due to textural differences because clay contents in the top layers, where POM is more transformed, are higher than the average 10 % cited by the reviewer. Soil texture has already been discussed above at Point 6. As stated above at Point 3, only individual samples, without replication, could be analysed by 13C NMR spectroscopy due to the high cost of the analysis. This was also the case with samples analysed for 14C content and therefore standard error values are not available for samples analysed by these methods. In Figure 5 individual samples were measured and in Figure 6 pooled samples were measured, these details will be added to the legend of the revised version. 5-10 cm fine bulk soil values will be removed from the Figure 5 to avoid confusion and replication as these values are also shown in Figure 6.

R1 Technical corrections: R1 Discussion - pg. 6220, ln. 26: Should this be fig. 1? Figures 1 and 2: give the exact elevation in the x-axis of the scatter plots. Pg. 6221, ln. 20: 0-5-1 units? Should this be 0.5-1 units? Table 3: delete "CN ratios" as these are not shown in this table. Interactive comment on Biogeosciences Discuss., 7, 6207, 2010.

Authors: Changes will be made to the Figures and text where appropriate.

Interactive comment on Biogeosciences Discuss., 7, 6207, 2010.