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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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R2 Budge et al. present a rare and important dataset on organic C pools and dynamics in alpine soils. This information is urgently needed to enable proper modeling of global C stocks under current and future climate conditions. The authors made also the attempt to relate differences in SOC pools to biotic and abiotic factors. However, considering the relationships presented, their conclusions seem to be overinterpreted. I agree with Reviewer #1 that this paper does not provide unequivocal evidence of the main drivers of changes of SOC in space and time, as stated in the title and the abstract. R2 The main factors influencing SOC dynamics, according to authors, were either not presented (temperature, litter quality) or their relationships with SOC pools were not shown (pH). Therefore, the main emphasis of the ms should be put on SOC

pools themselves, their quality and MRT, and a discussion on potential climate change effects using growing-season length and growing-season soil temperature (rather than MAT, see below). Title, abstract and conclusions should be revised. R2 The elevation gradient might be too small and sites to heterogeneous to draw final conclusions regarding possible drivers. Yet, additional data and relationships might help explain some of the intersite variation. The effect of temperature on SOC was mentioned in the text, but because only mean annual soil temperature ranges were presented in the Results section, no final conclusions could be drawn. It seems that MAT is not meaningful here, but instead soil temperature trends during the growing season should be presented. Furthermore, growing season length might be of importance for explaining SOC trends. Litter quality was obviously not measured in this ms. The litter+root fraction was likely composed of variously aged litter and live and dead roots. Litter quality cannot be derived from this fraction. The approach used by the author (plant functional groups) is highly speculative, and was applied to the highest and the middle elevation only. It is unclear, e.g., why MRT of site 2564 m is relatively high. Unless comparable values of litter quality for functional groups can be found in the literature, the effect of litter quality on SOC trends can only be inferred very generally from differences in vegetation among sites.

Authors: While the gradient is small, it was chosen to limit the influence of factors which are known to affect carbon distribution, such as land-use, management and geology. While it is well known that a combination of factors act in concert to determine carbon distribution, conclusions were drawn based on significant statistical relationships between the variables measured. As discussed in response to reviewer 1, any influence of soil temperature on other variables could not be established and as there was only little variation between the 2 sites measured, only an indication of soil temperature was reported in the manuscript. The distribution of plant functional groups was intended to show the variation of plant species, and therefore, plant composition and in turn, litter input, between the sites. This method was used on all sites, although the implications of certain functional groups was only discussed for 2 sites. In addition, plant functional groups were never discussed directly as litter input and neither were they compared with SOC content, distribution or turnover.

R2 A major role was attributed to pH, but relationships with pH were only mentioned in the text, and the use of the Ellenberg values requires more explanations. Site 2481 m which is characterized by somewhat higher pH is covered by legumes at least twice as much as all other sites. N fixing might have increased N availability in the soil, but this was not measured.

Authors: N soil concentrations will be added to Table 1 of the revised version.

R2 Much more care has to be taken of the details. This is especially relevant concerning accurate reporting of data, as follows: 1) POM C and mOM C in Table 2 have to add up to 100%. However, for 0-30 cm at site 2379 m the sum is only 90%. 2) The contribution of POC to SOC at 0-20cm was presented as 39.6-57.6% in the Abstract, as 45.9–57.6% in Results. 3) R-square of the relationship in Fig. 4 was presented as 0.90 in the figure itself, as 0.95 in Results.

Authors: Corrections will be made to the POM C and mOM C values reported in Table 2, to the POM C proportions reported in the abstract and to the R-squared value reported in the text.

R2 In agreement with Reviewer #1, where possible more statistical analyses are inevitable to corroborate the conclusions of this study. It is acknowledged that replications had often to be bulked for radiocarbon and other measurements.

R2 Specific comments R2 General: R2 Several typos stand out in the text, e.g. on line 22, page 6211; lines 2 and 23, page 6214; line 25, page 6216 (unit); lines 7-8, page 6218; line 20, page 6221; caption of Fig. 3. The references to Fig. 4 on line 26 of page 6220 and to Fig. 1a on line 8 of page 6222 are wrong.

Authors: We thank the reviewer for his or her rigor. Corrections and alterations will be made in the revised version of our MS.

R2 Acronyms: The ms is overloaded with acronyms. To increase clarity, it is suggested to minimize their numbers and to keep them simple. It is unclear why OM had to be added as a separate acronym, instead of using SOM throughout the ms. The use of multiple similar acronyms is confusing, e.g. mOM, MOM C %, MOC, or POC, fPOM, oPOM, POM, POM C, POM C %, etc. Carbon-to-nitrogen ratio should preferably be designated as C/N ratio or C:N ratio rather than as CN ratio.

Authors: In the revised version, the text will be simplified and clarified as much as possible: OM abbreviations will be replaced with SOM throughout the text, POM C will be replaced with labile C and CN ratio's will be changed to C/N ratio throughout.

R2 Methods: Details of the climate model used to calculate MAT and MAP of sites should be added. It is unclear what authors call "fine earth" in subsequent sections of the paper. Is it 0-200 μm, i.e. have 0-63 μm and 63-200 μm been combined after removal of litter and roots? But what about 200-2000 μm? Why is it called fine soil later in the ms? Why were roots and litter not separated? This could have provided necessary info on litter and root quality, at least regarding C/N ratio. Were roots live, dead or live+dead roots? Was root +litter MRT used as a measure for plant MRT for time-lag calculations? Litter is obviously a different state than plants, and roots might be composed of unknown portions of live and dead material.

Authors: References are given on the calculation of MAT and MAP through a climate model however extra details will not be added to the text because of the complexity of the approach. Readers are referred to the references cited. The soil analysis section will be corrected. The appropriate term, fine earth, will be clearly defined in the materials and methods section and then used consistently throughout the paper. Root and litter material was not separated due to the difficulty in the identification and physical separation of the living/dead, course/fine root or litter material. Ideally, it would have been desirable to separate the material. However, in terms of litter contribution to SOM, which we consider in our manuscript, we consider that all the root/litter material obtained comprises the C input source for SOM. As will be clarified in the text, time-lag was calculated from the average root/litter MRTs calculated from measurements across four depths.

R2 It is unclear how composite bulk soil MRT of 70 yr could be used to calculate fraction bulk soil MRT. Applying eq. 1 to 70 yr results in a negative value. Was a correction factor applied to this, e.g. due to the time lag? This should be clarified.

Authors: As already explained in the M&M section, the model was not applied to MRTs' below 70 yr because these data are out of the calibrated range. Details of the application of the time-lag period and equation 1 will be added to the text in the M & M section and in the legends below Figures 5 and 6 of the revised version.

R2 NMR methods should be clarified. It is unclear, e.g., why 60-90 ppm was used for regression against MRT, but a broader range for assessment of microbial transformations.

Authors: As will be stated in the revised text, other regions were also compared to MRTs however, in this manuscript we report the most significant result. A reference to the chemical groups used to determine microbial transformation will be added. See also our reply below regarding the meaning of the different spectral regions.

R2 Discussion: "... decreasing degree of transformation in labile material with soil depth" The discussion of decreasing transformations of free POM with increasing depth is problematic. The degree to which free POM was decomposed at the time of core extraction appears to decrease with depth according to NMR data. However, MRT of this fraction increased with depth, which indicates that free POM at depth is older than the equivalent fraction at the surface. High MRT goes normally along with a high degree of transformation or decay, as mentioned on l. 1 of p. 6224. If free POM at depth is older by 170 yr on average, but less decomposed than free POM at the surface, then decay rates of free POM at depth would have to be very seriously impeded by some controls on decomposition. Higher bulk density should not have imposed physical protection on SOM, since BD is rather low in all soil samples of this

study.

Authors: As stated and referenced in the text, increasing C/N ratio's with depth have been reported previously, however reasons for this trend with depth are unknown. In our manuscript we have reported data and figures which give an important indication of the environmental factors measured, however we can only speculate as to why we see the patterns we have observed. In this case, we not only discuss the influence of soil physical properties but particularly that of limited nutrient contents. Our data show that, in agreement with more recent findings on the relationship between age and degree of transformation, that relationship must not always be positive. In our case, this is observed for subsoil horizons. We will add the following detail to the manuscript: "Therefore, potentially labile and little transformed carbon sources such as fPOM may age at deeper layers due to environmental constraints in alpine soils".

R2 "Acidic soils are often depleted in major cations while on less acidic soils, plants benefit from higher availability of macronutrients." Cations and P were measured, which should allow a discussion rather than speculations.

Authors: Reference to the nutrients in Table 1 will be added to the discussion, see also point above.

R2 "Most strikingly, across a variety of fractions and sites 90% of the variability in MRTs could be explained by the content of O-alkyl-C (mainly polysaccharides) (Fig. 4), showing the strong role of litter or POM quality on C turnover in alpine soils." The Alkyl-C/OAlkyl-C ratio was used here as a measure of transformation of SOM. This ratio did not correlate with MRT. % O-alkyl-C might be a measure of polysaccharides, which would include a wide range of labile and recalcitrant compounds, such as starch and cellulose, and would not be usable as a measure of POM quality.

Authors: Although the ratio Alkyl-/O-Alkyl C is not independent of O-Alkyl-C itself, the meaning of each of these measures may differ. As discussed above, degree of transformation must not be related to age (think of preserved wood in a peat bog, for example).

O-Alkyl-C content as a measure of more labile substances (we refrain from characterizing cellulose as 'recalcitrant') is an important driver for the decomposition process itself and is, according to many decomposition studies, the spectral region declining most rapidly. R2 "Examination of replicate samples at two sites reveals considerable spatial variability in SOC storage and MRT of fPOM in 5–10 cm sections." Despite the limitation of the number of samples that can be processed for radiocarbon, 3 replicates are not a good basis for a discussion on spatial variability. A larger sampling effort is needed to assess spatial variability, especially if calculated variables, such as C input are used. Such variables are affected by variability of two different factors, here radiocarbon and SOC stores.

Authors: To the knowledge of the authors no data showing radiocarbon replication in terms of replicate variation has been published. While we agree that there are limitations for such a low number of replicates, given that there is currently no information available on the spatial variability of MRTs, this information is valuable to provide an indication of spatial variation which has important implications for the application of carbon turnover models. However, a comment will be added to Discussion 4.4 to acknowledge the limited conclusions which can be made from such a low replicate number.

R2 References: R2 References written in a language other than English should be designated as such. Authors: This refers to FAL (1998) and we will add this information.

R2 Tables and figures: R2 Table 1: It should be considered to express Root/litter dry matter as C store and move it to Table 2 for comparison with SOC pools. Table 2: Variables need to be explained in the table caption. Why was SE of POM C not presented?

Authors: It will be considered to add root/litter C to the MS however, while interesting, this detail will only be added if it is relevant to the story of the MS. SE of POM C is the same as SE of mOM C as it was calculated by difference. However, a SE column will be added next to both columns to prevent any confusion.

R2 Table 3: MRT of free POM at 5-10 cm depth of site 2564 m is available from Fig. 5, and should be added here to present change in MRT along a continuous depth profile.

This point is not clear, however the graphs will be changed to clarify points brought up by the first reviewer and therefore it seems that this point is no longer relevant.

R2 Table 4 can be removed and results included in the text.

Authors: As explained above, the authors feel this is important information which has never been published previously and therefore it is important to show it in a format which is as clear to the reader as possible and therefore a table format is most appropriate for this information.

R2 Table 5: Soil depths are unclear. Phytomass would be better presented as g m-2. Unless the chemical representation of O-alkyl-C can be made clear (see above, polysaccharides can be any among numerous labile or recalcitrant molecules), Fig. 4 could possibly be removed and the information included in the text.

Authors: Soil depth used to calculate site values will be added to the table caption. Phytomass values will be changed to g m2. As the relationship between O-Alkyl-C and MRT has important implications for the influence of litter input and carbon turnover, we feel it is important to include this graph to show the relationship indicated. See also our reply above.

R2 Fig. 5: Bulk soil MRT is shown for 5-10 cm at site 2481 m in Fig. 6. Could be shown here as well.

Authors: Bulk soil values will be removed from this graph to avoid replication of results and therefore this point is no longer relevant.

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