Biogeosciences Discuss., 7, C3604–C3608, 2010
www.biogeosciences-discuss.net/7/C3604/2010/

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.

Anonymous Referee #1

Received and published: 3 November 2010

General comments: 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 different 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.

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).

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.

Specific comments:

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.

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?

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).

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).

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.

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.

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

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,...

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

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.

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..."

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.

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.

Technical corrections:

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.