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Discussions

Interactive comment on "Relevance of aboveground litter for soil organic matter formation – a soil profile perspective" *by* Patrick Liebmann et al.

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RC1 Comments by Referee #3, 10.01.2020

Introduction

This study investigated the impact of aboveground litter for soil organic carbon (C) sequestration and the subsequent partitioning of litter-derived C in different soil layers and OM fractions. In general, I think the data are solid and the results are valuable for understanding fates of litter C input. I have some minor comments/suggestions that could improve the manuscript.

Authors response

We want to thank the referee for his positive feedback and the helpful and constructive criticism, which helped to improve the manuscript.

1. Comment

Lines 37 and 38: This statement may be correct only for natural ecosystems. For example, OM disturbance due to tillage may be a pathway for cropping systems.

Author response

We agree with the Referees comment and modified the sentence in line 37 to 38 of the original manuscript as follows: "In forest ecosystems, major pathways of OM to enter subsoils are rhizodepositions, root exudation and dissolved organic matter (DOM) leached from the horizons above (Wilkinson et al., 2009; Rumpel and Kögel-Knabner, 2011; Kaiser and Kalbitz, 2012)."

2. Comment

Line 99: Did you also observe the amount and chemical properties of the litter? These factors could impact litter decomposition and are important for interpreting the results.

Author response

The amount of litter was about 275 g m-2. This was defined according to measurements of Meier et al. (2005), who reported a litter input of a beech forest at 165 of 427 g m-2. Chemical properties were not analyzed, but since the labeled leaves were harvested from the same tree type (Fagus sylvatica) as in the research forest, we assume that the chemical properties of the litter resemble the natural environment of our study site. We recognize this comment and modified the sentence in line 98 to 99 of the original manuscript as follows: "For the labeling, the natural litter layer was removed manually and replaced by an equivalent amount of 275 g 13C enriched beech litter per m-2, representing a typical input of beech litter in Germany (Meier et al., 2005).

Labeled litter was prepared as . . .”

3. Comment

Line 271: "...both, inputs...".

Author response

We are unsure about the intention of this comment, but we assume it aimed at the comma? But manuscript text and comment are the same. No changes to the sentence in the original manuscript were made.

4. Comment

Line 301: For "DOM", did you mean DOM leached from surface soil layers?

Author response

In the first passage of this sub-chapter 4.2 (from line 290 to 314), we discuss the overall role of DOM for MAOM formation, without a specific focus on litter-derived DOM but rather relate the DOM in general, i.e. of different source. To prevent possible misunderstandings, the sentence in the original manuscript was modified in the following way: “Decomposition of roots can substantially contribute to the subsoil SOM pool as well (Rasse et al., 2005), but since root density (Heinze et al., 2018; Wordell-Dietrich et al., 2019) and root exudation (Tückmantel et al., 2017) are low in the Grinderwald subsoil, we assume that the increasing share of MAOM with soil depth rather suggests an increasing importance of DOM as a dominant source of C in this forest subsoil, irrespective of its origin.”

5. Comment

Is it possible that rhizodeposition still made a considerable contribution in subsoil MAOM although root density and exudation were low, given that subsoil MAOM contents were also very low?

Author response

A considerable contribution of rhizodepositions is possible, as we also found that about 20 % of the deep subsoil SOC is present as POM (Fig. 2), most likely derived from roots, but we assume that it is not the dominant source of subsoil MAOM, as we discussed this in lines 298 to 301 of the original manuscript. And, yes, in absolute number, the content of subsoil MAOM is very low (Fig. 3a). In this paragraph, we wanted to highlight the shift in the importance of the different functional OM fractions from 25 % to 77 % with increasing soil depth.

6. Comment

In addition, it looks that microbial decomposition of root derived C may also increases 13C values and decrease C/N ratios of MAOM; so I am wondering if the observations can fully support the conclusion that DOM leached from the surface soil layers was a dominant source.

Author response

We agree that microbial decomposition of either root-derived C or also litter-derived C may increase 13C values. Together with preferential sorption of 13C-depleted substances, both processes account for the 13C pattern with soil depth, as we discuss in line 304 to 306. Since we do not link our observations and conclusions to recent litter-derived C alone in lines 290 to 314, we think that the response given in Referee’s comment #4 and the respective modification in the text are sufficient to clarify that we relate this observation to DOM of different origin. We want to add here that the natural 13C pattern with soil depth was taken into account and used to determine significant enrichments of the labeled samples. This was expressed in eq. 7 (lines 204 to 210).

7. Comment

Lines 360 to 364: Could you explain where the majority of litter-derived C goes; emitted as CO2?

Author response

This is a good and very important comment/question, which we will definitely address. We made a detailed mass balance regarding the fate of recent litter layer-C, including DOC monitoring and surface CO2 monitoring. Both will be subject of another publication (currently in preparation), which will focus on the budget in contrast to the present publication where focus in on the fate of litter-derived OM in soil. To answer the question: The majority of the labeled litter-C on the one hand indeed emitted as CO2 ($\sim$ 36-40 %) and on the other hand remained in the litter layer ($\sim$ 35-40 %).

8. Comment

If so, why the older mobilizable OC did not emit as CO2?

Author response

Older OC definitely did emit as CO2 (katabolic pathway), or is recycled by soil microorganisms and consequently used as a source to build-up biomass (anabolic pathway). Microbial decomposition is also the primary reason for the strong decrease of SOC (Fig. 1) and MAOM-C (Fig. 3) with increasing soil depth. What we wanted to highlight in the lines 360 to 364 was, that the mobilizable OC fraction contains predominantly C older than 22 months, despite showing a higher 13C value compared to bulk soil or MAOM.

9. Comment

Line 366: Did you measure the amount of litter residues after 22 months?

Author response

Yes, the removed litter residues after 22 months were measured and amounted to 405 g m-2 per site. Considering an initial mass of 275 g m-2 added litter, we removed about 130 g m-2 more litter than applied. This difference may have resulted from freshly fallen litter material, which was smaller than the mesh size and therefore accumulated during

the 22 months. The proportion of remaining labeled litter within the removed litter was about 25 %, corresponding to about 35-40 % of the initial applied labeled litter, mentioned in comment #7. We modified the sentence in line 102 to 103 in the original manuscript as follows to include this information: “In November 2016, the labeled litter was removed manually and amounted to an average of about 405 g m-2 per plot. We thus removed more litter than we initially applied due to incorporation of small leaf debris and beechnut shells during the 22 months. About 25 % of the removed litter were residues of the applied labeled litter.”

10. Comment

Line 395: This statement (and may be statements in other places) is also related to the comment on root-derived C contribution to subsoil OM.

Author response

We agree that roots should always be considered in the context of soil OM. But since we already discussed the (not dominant) impact of roots at our study site in section 4.2, we prefer to end the manuscript with highlighting our main implication and not with findings of other publications (e.g. Rasse et al. 2005).

Rasse, D. P., Rumpel, C. and Dignac, M.-F.: Is soil carbon mostly root carbon? Mechanisms for a specific stabilisation, Plant Soil, 269(1–2), 341–356, doi:10.1007/s11104-545 004-0907-y, 2005.

11. Comment

Figure 1: What about the differences of bulk OC between these two sampling times; increasing, decreasing, or no detectable change?

Author response

For the majority of depth increments (9 out of 14), there were no changes between both sampling times. However, for 5 increments, including 0-5 and 5-10 cm, bulk soil

OC was smaller at the second sampling compared to the first, likely due to variations in litterfall, bioturbation, and decomposition as a result of differences in precipitation. In the 22 months of litter application, about 950 mm precipitation was measured while it was only about 570 mm in the 18 months thereafter.

12. Comment

Figure 3: I would suggest deleting the grey points if they were not reliable.

Author response

We highly discussed this topic among the authors before the submission and now during the review process. We know that such low values are not realistic for SOC in any soil depth. The reason for these values is a nitrogen content close to the detection limit. Nevertheless we included the values, since all other figures show complete data sets and we wanted to be consistent and also transparent by not excluding data. To prevent misinterpretation, we decided to clearly mark them in grey. We would like to keep the data in the manuscript as presented in the original manuscript, also referring to the other four referees who accepted this presentation.

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