Biogeosciences

Discussions

BGD

Interactive
comment

Interactive comment on "Which are important soil parameters influencing the spatial heterogeneity of ^{14}C in soil organic matter?" *by* S. John et al.

S. John et al.

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Received and published: 20 May 2016

First of all, we would like to express our thanks for the review. We considered all comments carefully, which significantly improved the general quality and structure of the MS. Below, we will provide a point-to-point reply to the comments.

Sincerely, Stephan John

Author comments on anonymous referee (RC#2); published on 15 March 2016

1) I was wondering why the authors did not include the topsoil. This is surprising as it was said at several passages in the Introduction and Discussion chapters that for topsoil, aboveground litter plays an important role for SOM. Similarly, a negative point is that several parameters were obtained solely down to a depth of 110 cm.

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[Discussion paper]

This study was performed in the framework of a larger research project on subsoil carbon dynamics which uses a comparable sampling scheme (starting in 10 cm) at different sites excluding the A horizons (5 cm). We consider the A horizon of being of minor importance since this study should solely focus on subsoil. Moreover, we needed to sample large and comparable volumes of soil from each sampling depth (using steel cylinders) to obtain enough material for some analysis (e.g. compound-specific $14C$), which would not be possible from a 5 cm thick A horizon. Unfortunately, we were able to fractionate and analyse only 5 depth because of limited capacities and funding in our collaborative project.

2) PCA as the only statistical tool is not sufficient for a comprehensive and sound interpretation, especially when taking into account that some of the studied soil properties may correlate with each other. Here, PCA did not yield unambiguous results, so it is not safe to declare roots as the main factor, especially as dissolved organic carbon was not investigated.

In addition to the PCA we insert general linear regression models with $14C$ as dependent variable in relation to the soil parameters investigated. These results confirm the importance of the root mass already indicated by the PCA. The regression analysis are shown in a new Figure 3 including regression coefficients.

3) It is nowhere explained, why it is desirable to improve understanding of SOM cycling and turnover. One or two sentences would be enough to show the reasons for such research like in the current study.

We added two sentences at the beginning of the introduction explaining the overall importance of the study.

Detailed remarks:

TITLE 4) I miss the information, that the soil under a tree, thus likely forest soil was investigated. This information does not appear before the middle of the Abstract, and

towards the end of the Introduction.

We added this information in the abstract and introduction.

ABSTRACT

5) Lines 28f: This is a rather sudden transition from general points to the current study.

We modified the abstract according to the remark.

6) Lines 32f: It was not mentioned here that only fine roots were analysed.

We added this according to your comment.

7) Line 45: to determine SOM" sounds strange. Please rephrase.

Changed according to your comment

INTRODUCTION

8) I missed some references like Marschner et al. (2008) when talking about different turnover times of SOM compounds (lines 54ff), or Kautz et al. (2013). Although the latter review deals with nutrient uptake from arable and not forest subsoils, it could be included in the current study, as the Introduction is partially written very general.

We are very thankful for the suggested references and added Marschner et al. (2008). In our opinion, the study of Kautz et al. (2013) does not fit completely in this context.

MATERIAL AND METHODS

9) Lines 120ff: If profile A was sampled directly beneath the tree, I am sure that there were also coarse roots present.

Our analysis on root mass was exclusively based on fine roots and is explained in more detail in chapter 2.3.1.

10) Lines 129f: Please give the temperature used for combustion of the samples.

We added the combustion temperature.

11) C2 Lines 161f: The authors treated the samples with acid to remove inorganic carbon. This contradicts the statement made in lines 129f.

Yes, but it is essential to remove any potential inorganic carbon for 14C analysis which may also derive from packing material. AMS 14C analysis are done on μg to mg amounts of soil and are easily contaminated. We chose a very weak acid treatment with which we did not influence 14C results (according to own tests).

12) Lines 180f: Why were some analyses like particle size not measured on the complete sample set?

According to comment 1, the density and particle fractionation could only performed down to 110 cm depth.

RESULTS

13) Lines 273ff: In my opinion, it is not necessary to give decimals for the pMC values.

We agree in this point, but we will maintain decimals for the pMC values since the measurement errors ranging on this scale.

14) Lines 312ff: The phrasing with the "<" signs is not easy to read.

Changed according to your comment.

DISCUSSION

15) Chapter 4.1 has the heading "Influence of root-derived OC on 14C distribution". However, first it begins with repetition from the Introduction as well as citing references rather than discussing the own data, which I find inappropriate. Second, the complete second paragraph discusses the root depth distribution rather than its influence. The actual statement made about root influence on 14C distribution is restricted to the end of the chapter and is written rather vague.

We agree in this point and rewrote this part.

16) At the end of chapter 4.3 (lines 433f.), it sounds like grain size would have a stronger effect on 14C distribution than roots, which contradicts the overall statement of the study. Please rephrase.

We rephrased this part to make clear that there is only an apparent influence of grain size on 14C distribution.

Further notes:

17) Language is partially bad and has to be considerably improved, also to avoid mis-understandings in case of ambiguous statements (e.g. lines 140ff, lines 190ff.).

The language has been edited.

18) Throughout the manuscript, SI units should be used, also for C contents.

Changed according to your comment

19) From Fig. 1, I can not clearly see if the horizon boundaries were even or undulating, and if they occurred at identical depth throughout the transect. Maybe a picture would be good.

We added the horizon boundaries in this figure to make clear that the samples were taken at identical depths and horizons.

20) I also noticed that the horizon terms refer to the German soil classification system, whereas the soil classification itself refers to WRB. Please use a uniform system.

We changed this according to your comment.

21) The supplement shows the same data like Figure 2. One of the two might be skipped to avoid repetition of the same data.

We skipped Figure 2 and replaced this by the supplementary table which is now table 1.

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