

Interactive comment on “Mean age of carbon in fine roots from temperate forests and grasslands with different management” by E. Solly et al.

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We thank the two anonymous referees for their time and appreciate their comments and suggestions on the manuscript.

Anonymous Referee #1

This is a really nice manuscript. I have hesitated to submit a review as I have actually not been able to detect any major problems with it, although I have now read it several times. In general the manuscript is pedagogic and well written. The topic is relevant, the study is novel and the authors do not make any claims that were not supported by their data. As a reader, however, I had some problems with following how land use/land management differed between regions and within grassland and forests. I do

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not think that this was because the manuscript was poorly written, but because the experimental design was quite complicated. This different land use/land management is also a potential methodological problem, although as far as I understand I think the authors actually handle this quite well. The one thing that disturbed me while reading was that the hypotheses were not presented again in the discussion section, but instead just referred to as the first, the second and the third hypothesis. I had to go back in the manuscript to remind myself which the hypotheses were and which number they had. Please remind the readers in the discussion section what the hypotheses were. Other than that I can only congratulate to a nice piece of work.

Response: We are pleased about the favourable response and we agree that restating the three hypotheses in the discussion section improves the reading of the manuscript. Therefore we will include them in the revised version of the manuscript.

Anonymous Referee #2

Overall, the work provides an interesting contrast between the mean ages of C in grassland sites and forest sites. In particular, the finding that mean C age in fine roots was more variable in forests was interesting, and upon thinking about it is logical. The authors do a good job explicitly stating they are not measuring and reporting root turnover (lines 92-94). This is an important distinction. However, because it is so important it would be good to restate that distinction or difference later in the paper again (discussion section perhaps) just to make sure that readers do not misinterpret the data.

Response: We agree that it is important not to confuse C mean age with turnover time of fine roots, thus we would like to restate this difference at the beginning of the discussion section.

Regarding statistics, the authors layout their plan for statistics in the methods section. However, when describing the results in the results and discussion sections it is sometimes unclear which results were tested in what way. For me, this caused confusion as to why some results were described as significant in some places and not significant

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in others.

Response: Throughout the results and discussion sections we would be happy to clarify when the results are significant or not significant and further explicitly specify which method we used for the statistical analysis.

A final weakness, that unfortunately cannot be undone at this point, is the use of a simple size classification of <2 mm to define fine roots. There is now ample evidence to show that this distinction is not appropriate and should be avoided. All future studies should take note of this and work to utilize/develop more functional definitions of “fine roots” based on their likely roles for absorption, transport and/or storage. This limitation here does make it difficult to interpret some of the results. For example, the paper reports that the man C age in fine roots was more variable in forests than it was in grasslands. This may be true, but it may also be that forests and forest species produced more variable amounts of short-lived, absorptive roots vs. longer lived, transport/ storage roots. Both of which can be easily found below 2 mm. Despite this weakness, I still feel that the manuscript is of sufficient quality, novelty, and importance for publication.

Response: It is true that the diameter size class selected in the manuscript may include variable amounts of roots with different functions, especially in the forests, and that this may limit the ability to precisely quantify mean root C ages for roots with different roles. Therefore, we would like to indicate this limitation in the introduction section and suggest in the conclusions that in the future studies using this approach may define fine roots according to their functionality. However, the intention is to look at the standing stock of root biomass, which admittedly mixes a smaller mass of rapidly cycling roots and a larger mass of older roots in forest environments. We do not interpret the radiocarbon data as indicating the turnover, merely the mean age of C in standing root biomass as it is frequently reported (according to a size cutoff). The way to test if the reviewer is correct is to compare the mean age of the standing stock with the mean age of C respired from it (or in residues following decomposition). We are currently collecting these data and perhaps will be able to use a more sophisticated approach to

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assess root dynamics (rather than the mean age of C in roots) when we are able to put them all together in a belowground C and ^{14}C budget. We agree with the reviewer that the age is a mean that can mix faster and slower cycling pools but, until we can better have a way to quantify them, we think it is still important to show the large differences between forests and grasslands.

A more specific comment from Line 200: Does this approach assume that age of C in fine roots (average root age plus length of time C is stored in plant) is constant through time or at least since 1950? If is not constant through time, is it valid? It probably won't be constant through time due to interannual variability in climate leading to different storage capacities and fluxes and due to periodic disturbances quickly draining reserves.

Response: The model implemented by Gaudinski et al. (2001) assumes an average input and output of C to the root fraction every year. Thus, significantly large variations of storage capacities and fluxes between years (for instance, a greater root production in dry years rather than wet years, or a larger consumption of old storage compounds in dry years) might bias the results of the model. We can also not exclude a bias if sampling at the end of the growing season rather than at the beginning tended to exclude some population of faster cycling roots from the sample. The model is further assuming a homogeneous pool of roots (or one normally distributed around the mean), therefore we should emphasize in the paper that with this method we are likely averaging more than one pool, as was shown in a later model by Gaudinski et al. (2010) -RADIX- and is required to reconcile the minirhizotron data with radiocarbon data in their study. However, we have no access to samples in the past and, without sampling at some future point, we cannot know if this is correct.

Specific Comments:

Throughout the paper, whenever there are multiple citations a space needs to be added between the semicolon and the next citation.

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Response: Yes, these changes will be included in the manuscript.

Lines 28-29: Is the difference here due to soil texture or due to other variables (temp/altitude, precip)?

Response: With this sentence in the abstract we intended to summarize that the differences in the mean age of C in fine roots which we observed between study regions are most probably a result of the different soil properties, especially nutrient contents and moisture conditions which may vary according to diverse soil texture or precipitation. However, these are probably indirect effects as differences in soil properties can lead to different species compositions and different contributions of perennial species. Another factor that could be responsible for differences between Exploratories might also be related to temperature differences. As it is warmer in the Schorfheide-Chorin than in the Hainich-Dün and Schwäbische-Alb and samples were taken in spring, it is possible that the plants in northern Germany grew already more new roots than plants in southern Germany at the time of sampling resulting in a relatively larger contribution of recent roots in the Schorfheide. We apologize for the lack of clarity in our original submission and we hope we have addressed the referee's concerns. We will clarify this point appropriately in the abstract.

Line 35: "fine plant roots" change to "plant fine roots" Line 35: add space between 2 mm

Response: These corrections will be included in the manuscript.

Line 35: this class of all roots < 2 mm will include roots with different functions (i.e. short-lived absorptive roots as well as longer lived transport roots). This is mentioned later in lines 49-50, so at least the problem is acknowledged.

Lines 43-45: The sentence might read better if "derived from the root standing stock and belowground C fluxes" were moved to the end of the sentence.

Response: Right, we will accordingly change this sentence in the manuscript.

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Lines 76-77: Majdi doesn't say cleanly that they ARE a useful method, more that they may or can be a useful method assuming great care is taken. This caveat should be expressed.

Response: True, we will change this sentence to: "Radiocarbon measurements of roots may be a useful measure for understanding belowground carbon fluxes, if the root carbon pools of interests are defined appropriately (Majdi et al. 2005)".

Lines 88-89: The wording "average time elapsed between C fixation and its incorporation into root tissues" is tricky but correct. If possible, it should be made clear that this is not the same as actual root turnover. This is cleared up in the next paragraph (lines 93-94) but it may be useful to allude to the potential problem here as well.

Response: We would like to add the following sentence after the sentence above to clarify that we are talking about fine root C ages rather than root turnover: "Accordingly, ¹⁴C investigations can be used to estimate fine root C ages rather than the direct turnover time of root systems".

Line 117: should the word "managed" be added between age-class and forests?

Response: Yes, the word managed should be added.

Lines 124-127: A little more explanation/clarification here would be useful.

Response: We agree that adding some details here would be useful. To evaluate land-use and disturbance intensity in the forests we used an index called LUDI. This index was established by Luyssaert et al. (2011) by combining values of stand density and diameter at breast height for a relatively unmanaged forest and different management schemes, in conjunction with selfthinning values. The LUDI is calculated as the sum of two components, the "planning intensity" which relates to the potential stand density and the associated changes in diameter and the "operational intensity" which relates to the standing biomass (or diameter) at a given stand density. Thus, the LUDI distinguishes between the long and short timescales which are associated with management

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and disturbance.

Line 132: change “selected always” to “always selected”

Response: This correction will be included in the manuscript.

Line 133: “20 m long in grasslands and 40 m long in forests”! I assume the cores were evenly spaced across the transects in both systems (i.e. cores were further apart in the forests)? Please clarify.

Response: The soil cores were evenly spaced across the two transects in both grasslands and forests. We would like to clarify the sentence as follows: “The two transects were 20 m long in grasslands and 40 m long in forests, the soil cores were evenly collected at a distance of 7 m in the forests and of 3 m in the grasslands”.

Line 135: mixing the material selected? Does this mean that not all sample was used? If all sample was used (within the 0-10cm increment) than it might be better to say ‘collected’

Response: Right, all sample collected within the 0-10 cm increment was used. This correction should be included in the manuscript.

Line 139: <2 mm. I still think this is a problem as this size classification will contain many roots of different function and very different turnover times.

Line 152; add space, 500 m² Line 154: Insert comma after “grasslands”

Response: Yes, we would adapt these changes in the manuscript.

Line 157-158: this is not clear. Can more details be added? Lines 160-161: Can more details be added here?

Response: We agree to clarify and include more details about the vegetation survey in the manuscript.

Line 176: add space between 2 mm

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Response: A space will be accordingly added in the manuscript.

Lines 230-234: While statistical significance is given in tables, it would be helpful if the authors details what results were significant here in the text as well. Terms like “greater” “pattern was reversed” “greatest” “highest” “higher” “slightly higher” may all be useful but should be qualified/clarified as to what is significant or not.

Response: We agree, and we will precisely explain when the results are significantly different according to statistical analyses.

Lines 263-265: very interesting

Line 265: In regards to the interpretations of the unit risk ratios reported, it is difficult to know if this has much meaning beyond this study. Qualitative descriptions of the risk ratios may more helpful/appropriate. Lines 266-267: This is based on the risk ratio information?

Response: It is true that specific interpretations of the unit ratios may be difficult to extend beyond this study. Thus, we agree that qualitative descriptions of the risk ratios would be more appropriate in this context and we will include them in the manuscript instead of the interpretations which we previously reported.

Lines 269-274: These two sentences appear to contradict each other. Did diversity increase or decrease with soil N? Please clarify.

Response: Plant diversity significantly decreased for increasing values of soil available N evaluated with the Ellenberg indicator values. We would like to change the sentence as follows: “Plant diversity ranged from 1.0 to 2.9 (mean: 2.0 ± 0.1) and declined significantly for increasing values of soil available N and soil moisture according to the Ellenberg indicator values ($P < 0.05$, Table 5). The absolute number of perennial species present on plots ranged from 11 on plots characterized by higher soil available N content to 47 on plots characterized by lower soil available N content (mean: 22 ± 2), and also decreased significantly for increasing values of Ellenberg indicator values for

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soil moisture content and root N concentrations ($P < 0.05$, Table 5”).

Line 277: Should this be a “>” symbol instead of “<”?

Response: Yes, this should be a “>” symbol because fertilization, grazing and mowing activities as well as the LUI index did not seem to directly influence fine root C mean age significantly.

Line 282: it might be helpful to restate the hypothesis and/or rationale here.

Response: We agree. The hypothesis should be restated here.

Lines 295-296: This sentence seems to be the start of a new paragraph.

Response: Yes.

Lines 296-298: True, but this was across 1st to 5th order roots (distal roots being 1st order), all of which were < 2 mm in diameter and therefore all of which were included and lumped together in this study.

Lines 366-368: I do not follow the logic here. Please explain.

Response: In this sentence we try to explain that the mean age of root C in grasslands can be influenced by changes in plant species diversity and in the number of perennial species due to changes in soil moisture and available nutrients in the soil, which in turn may be influenced by different management practices. We will clarify this sentence in the manuscript.

Lines 369-370: Larger C inputs in more fertile sites? Table 5 shows no significant response with fertility and either standing root biomass or mean C age. Please explain.

Response: For more fertile sites we intended sites with a higher content of available nitrogen according to the Ellenberg indicator values. However, it is true that no significant responses between the available nitrogen in soils and the root standing biomass or the mean C age were observed, therefore this sentence should no longer be included in

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the manuscript. We apologize for the imprecision.

Lines 376-378; Yes, this is a valid take-home message from their work.

Figure 2: The font size needs to be increased for all parts of these figures (except for the panel identification). Currently, they are difficult to read without increasing the viewing to 125% or even 150%.

Response: Yes, we agree that the font size has to be increased in Figure 2.

Bibliography:

Gaudinski JB, Torn MS, Riley WJ, Dawson TE, Joslin JD, Majdi H (2010) Measuring and modeling the spectrum of fine-root turnover times in three forests using isotopes, minirhizotrons, and the Radix model. *Glob Biogeochem Cycle* 24. doi: Gb302910.1029/2009gb003649.

Gaudinski JB, Trumbore SE, Davidson EA, Cook AC, Markewitz D, Richter DD (2001) The age of fine-root carbon in three forests of the eastern United States measured by radiocarbon. *Oecologia* 129: 420-429.

Luyssaert S, Hessenmoller D, von Lupke N, Kaiser S, Schulze ED (2011) Quantifying land use and disturbance intensity in forestry, based on the self-thinning relationship. *Ecol Appl* 21: 3272-3284.

Majdi H, Pregitzer K, Moren AS, Nylund JE, Agren GI (2005) Measuring fine root turnover in forest ecosystems. *Plant and Soil* 276: 1-8. doi: 10.1007/s11104-005-3104-8.

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