Answers to review comments one - RC2

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Nonlinear turnover rates of soil carbon following cultivation of native grasslands and subsequent afforestation of croplands

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General comments

In this study, Nonlinear turnover rates of soil carbon following cultivation of native grasslands and subsequent afforestation of croplands, Hernandez-Ramirez et al., use existing soil C data produced in the previous studies (Hernandez-Ramirez et al., 2011) in combination with new measurements of stable C isotopes to evaluate long-term C turnover rates in relation to land use changes, from grassland to cropland and subsequently from cropland to forest.

The paper reads well and I find this an interesting paper worth to be considered for publication in the SOIL journal.

The study is well introduced and the authors build on existing literature to highlight the effect of land use change on SOM. But I did not find information on previous related studies especially on long-term carbon turnover or approaches that have been used.

Answer:

We are deeply grateful for the overall positive evaluation. We certainly agree to include additional information from earlier studies focusing on long-term carbon turnover in the introduction section (within the third paragraph of the introduction). These will include Huggins et al. (1998) and Collins et al. (1999).

Collins, H.P., R.L. Blevins, L.G. Bundy, D.R. Christenson, W.A. Dick, D.R. Huggins, and E.A. Paul. 1999. Soil carbon dynamics in corn-based agroecosystems: Results from carbon-13 natural abundance. Soil Sci. Soc. Am. J. 63:584–591.

Huggins, D.R., C.E. Clapp, R.R. Allmaras, J.A. Lamb, and M.F. Layese. 1998. Carbon dynamics in corn–soybean sequences as estimated from natural carbon-13 abundance. Soil Sci. Soc. Am. J. 62:195–203.

The authors did a good job on the materials and methods section. The study sites, the model, and the land use investigated are well described. However, the reasons behind the choice of the model and uncertainty related to the model are not clearly provided. Very little information is provided on basic soil physical-chemical properties that are known to influence C turnover such as texture, pH, oxides among others. These factors drive the C stabilization mechanisms which are briefly mentioned in the introduction and discussion. Having such information in the manuscript may highlight future studies as you mentioned in the discussion section.

Answer:

Thanks for the positive suggestions. The reasons for implementing the choice of kinetics model include the reduced numbers of model parameters and the capacity to constrain the upper and lower limits of the carbon trajectories (Jastrow et al., 1996); these collectively led to enhanced fitting and the ability to successfully conduct cross-validation (as shown in Figure 2D). We are glad to include these concepts in the method section of the revised ms.

Jastrow, J.D. 1996. Soil aggregate formation and the accrual of particulate and mineral-associated organic matter. Soil Biol. Biochem, 28:665-676

We also agree with the comment suggesting to include additional soil property data. We do have soil texture and pH data available, and we are indeed glad to include the available data in the revised paper (please see below the new table 1 – to be included in the revised manuscript). We note that we have already included texture and pH information for the US sites in the method section of the original submission at the location L215-217. Although we agree that existing literature supports that soil properties drive soil carbon pools and turnover (as shown for instance in Quesada et al. 2020), we note that in the specific cases of our study sites carbon turnover data does not correlate with available soil texture and pH data. This is likely because the reduce sample size in our study (e.g., the 3 Russian chronosequence sites shown in Figure 2 is contrasting to the 14 soil classes sampled by Quesada et al. 2020). Having said this, we will include available soil texture and pH data in our revised manuscript to better characterize all 9 sites including Russia and US.

Quesada, C. A., Paz, C., Oblitas Mendoza, E., Phillips, O. L., Saiz, G., and Lloyd, J. 2020. Variations in soil chemical and physical properties explain basin-wide Amazon forest soil carbon concentrations, SOIL, 6, 53–88

The following is the new table

Table 1. Topsoil textures and pH in the 9 study sites. Numerals correspond to sites as shown from west to east in Fig. 1.

Site	Soil pH	Soil texture
1. Huron†	7.0	sandy loam
2. Norfolk†	6.8	loamy sand
3. Mead†	6.1	silty clay loam
4. Streletskaya Steppe‡	7.0	loam
5. Ivnyanskiy‡	7.5	silt loam
6. Prokhorovskiy‡	7.2	loam
7. Gubkinskiy‡	7.4	clay loam
8. Yamskaya Steppe‡	7.2	loam
9. Kamennaya Steppe‡	7.6	clay loam

† correspond to the cropland location within this site.
‡ correspond to the native grassland location within this site.

The results section is clearly described. It has a lot of information that supports most of the statements made in the manuscript. The figures and tables are informative. Figure 4, the Y-axis is not scaled across panels. Is there a reason for this?

Answer:

Thanks for noting this aspect of the Y-axis. We have made the y-scales across panels to be different to be able to zoom in and to show more details to the readers. We are going ahead with noting this aspect in the figure caption.

The discussion and conclusion are short but very informative. The authors did a good job here too. As for the materials, and methods, the authors mentioned the role of mineral and physical protection of soil C. Having information on the basic soil properties related to these mechanisms may strengthen some statements of the conclusions.

Answer:

As noted in an answer above, we agree to include available data of soil texture and pH in the body of the revised manuscript as per new Table 1 above (although there were no clear patterns between carbon turnover and these properties; likely due to the sample size as mentioned above).

In the following sections, I will provide comments and suggestions for each section of the manuscript as indicated by lines.

Specific comments

Lines 83-84: You may consider revising this sentence. Does “biological-mediated decomposition” different from “decomposition”?

Answer:

We agree with keeping only “decomposition” and removing “biological-mediated” in this sentence.

Line 86: Do “decomposition” and “mineralization” have different meanings in this context? One of them would suffice.

Answer:

We agree with keeping only “mineralization” and removing “decomposition” in this sentence.

Lines 92, 94, 96, and 102 Authors mentioned “long-term” but it would be clear to the reader if this is clarified in terms of numbers.

Answer:

This is a good clarification. We are glad to include in the text of this paragraph in the introduction section “(i.e., ranging from decadal to century scales)”.

Line: 112, Be precise by using numbers to reflect the chronosequence

Answer:

Yes, we agree. We are switching to use numerals (instead of words) to express the number of chronosequences. We are doing this at this location in the text and elsewhere.

Line 123: How deep was the core? Was it a one meter core or did the authors focus on 0-30cm? They also refer to previous studies for more details but it may help the reader if this information is added here.

Answer:

The soil samples were collected to at least “1 meter depth”. We are glad to include this info in the method section of the revised manuscript.

Lines 143-145. It is good that excluding the contribution from erosion is supported with data on topography. But it would have been much better to provide this information here. The authors may consider saying “At our study sites, the slope gradient ranges from X to Y and the topography is classified as flat. Given the flat topography, we also assumed negligible C removals or additions due to erosion or deposition.”

Answer:

We agree and we are glad to include the following sentences. “At the various study sites, the terrain slopes ranged up to 2%, with the exception of Huron site that had a 3% slope. Hence, the general topography in our study sites was classified as flat. As most sites are considered semi arid, water erosion is assumed minimal; likewise, enough vegetative cover limits wind erosion. Given the dominant flat topography and low rainfall amounts, we also assumed negligible C removals or additions due to erosion or deposition.

The authors assume that the C input and output are balanced. Are there references to support this statement? It might be from the previous studies in the region or similar conditions. Usually, long-term C turnover is accurately or well described with multiple-pools models. What makes Eq. [1] a suitable approach for this study? The authors may consider adding that information in the method section.

Answer:

The assumptions of (i) C input = C output and (ii) steady state conditions (i.e., $\delta C/\delta t = 0$) are required to implement the kinetics modelling in equation [1] (Jastrow et al., 1996; Follett et al., 1997; Hernandez-Ramirez et al., 2011). As suggested, we are glad to clarify these references and information in the text of the method section. Furthermore, the kinetics modelling and stable isotope approaches used in the study enable distinguishing two soil carbon pools; for instance, for soil carbon changes under afforestation, total carbon can be partitioning between previously-existing carbon (e.g., remaining prairie-C) and newly-accrued carbon (e.g., new tree-C). We agree that carbon turnover is more accurately quantified in the long-term compared with short-term carbon turnover measurements.

Lines 152-156: Excellent idea, to use the cross-validation method to assess the model performance. In this section, the authors may also provide information on the total number of observations in the dataset used in this process. Given that sample size can greatly affect a model.

Answer:

Certainly, we are glad to include the sample size in this test (total number of observations in the dataset). This is "(n= 6)" at the end of the sentence in location L155.

Line 236: The authors assume negligible contribution to C storage by annual cropland. How small is the C input from these sources? Are they really negligible? Any data from previous studies that support this statement? Unless crop residues (straws) are taken from the field, they should contribute to some extent to the topsoil C.

Answer:

As noted above, the underlying premise is that although every year there is a carbon input from crop residue, this carbon from crop residue is largely lost to the atmosphere with no net contribution from crop residue to the total carbon on an annual basis. Hence, because of annual decomposition, the crop residues do not influence the total carbon in the long term. Although not fully conclusive, good evidence for supporting this premise is in figure 3 for the Russian sites (and figure 2): the total soil carbon declines over the long term (decades and centuries) in the cropland sites; this is attributed to the fact that crop residue has null or minimal contribution to the total soil carbon and also because disturbance by tillage is increasing soil organic matter decomposition and release to the atmosphere. Isotope analyses in the US sites showed that nearly all the carbon in the croplands resembles the carbon of the grasslands instead of an influence of crop residues, while the tree-C contributions really shifted the isotopic composition to enable identification of carbon sources (e.g., Huron). Several references support this collective premise (Jastrow et al., 1996; Follett et al., 1997; Hernandez-Ramirez et al., 2011; Gregorich et al., 2017; Mary et al., 2020). In addition, this implicit assumption is actually required to enable kinetics modelling to allocate total carbon into two carbon pools (tree-C vs. remaining prairie-C). In other words, C added annually by crop residues was assumed to be decomposed and released by the end of every agricultural year; therefore, although crop residues are adding carbon to the soil at some point during the year, this added-C is gradually lost over the following season (with minimal or even without any net carbon retention from crop residue in the long-term). We agree to capture and include these various concepts in the text of the revised manuscript.

Gregorich, E.G., H. Janzen, B.H. Ellert, B.L. Helgason, B. Qian, B.J. Zebarth, D.A. Angers, R.P. Beyaert, C.F. Drury, S.D. Duguid, W.E. May. 2017. Litter decay controlled by temperature, not soil properties, affecting future soil carbon. Glob. Chang. Biol., 23:1725-1734

Mary, B., H. Clivot, N. Blaszczyk, J. Labreuche, F. Ferchaud. 2020. Soil carbon storage and mineralization rates are affected by carbon inputs rather than physical disturbance: evidence from a 47-year tillage experiment. Agric. Ecosyst. Environ., 299

It further is re-emphasized that this assumption is necessary to implement the kinetics modelling in this study while allocating soil C into two pools (tree-C vs. remaining prairie-C). Moreover, the annual croplands adjacent to the evaluated afforested areas were managed with tillage operations. During long-term recurrent tillage plowing and cultivation (as shown in Fig. 3 over 250 years), the amount of carbon loss from the topsoil largely exceeds the amount of new organic carbon derived from any contribution or transformation of plant residues into humus. As a result, a long-term trend of carbon losses was manifested.

Line 314: As a followup to the previous comment, it looks like there are contributions from the recently added plant residues. How did the authors separate the C isotopic signature of the original land use (native grassland) from the recent crop residues?

Answer:

As noted in the answer immediately above, the kinetics modelling approach used in the study (equation [2]) enables allocating two specific carbon pools (e.g., remaining prairie-C vs. tree-C). In other words, the underlying assumption is that the crop residue carbon that entered the system every year undergoes decomposition within a season leaving no significant net contribution to the total carbon in the long term (Gregorich et al., 2017). Crop residue in these croplands are not actually increasing soil carbon content over time; the soil carbon in these annual croplands is actually gradually decreasing (figure 3 and figure 2). To separate and quantify any potential contribution of crop residue to total carbon would have required an additional set of different measurements at our sites; for example, isotopically-labelling plant residue and tracking its decomposition or retention over multiple years.

Line 323-324: Same as the previous comments related to the “contributions of crop residues recently added”.

Answer:

In Line 323-324, we are simply re-stating that growing trees increase soil carbon as opposed to annual cropland (which actually generated major carbon decreases). Some of the concepts in the previous answers above further address this comment.

Line 456: I guess “study studies” should be “study sites”.

Answer:

Thanks for pointing this out. Yes, we are glad to correct this in the text of the revised manuscript.

Lines 509-511. This is a very interesting finding. Why would old prairie-C decompose faster compared to fresh C input from roots and litter? The remaining prairie-C should otherwise be considered as stable C (less labile)? This comment is also linked to the statement in lines 470-471. I did not see data on the C turnover rate of the forest input.

Answer:

Yes, we agree that this is a very fascinating finding. We can now see that there was a confusion in the original text, and we would like to clarify in the revised text that additions of fresh plant-C decompose faster than old prairie-C in the soil. At lines 509-511, we are glad to revise the text as follows: “This analysis showed that under afforestation, soil C remaining from original native grasslands continues to be lost from the profile likely via microbial mineralization (Fig. 3, Fig. 7). It is noted that the accretion and turnover of recently-added tree-C is much faster than these observed losses of remaining prairie-C beneath trees as the recently-added plant-C is considered relatively more labile than prairie-C.”

In the case of the C turnover rate of the tree-C in afforested soils, this key result is explained in the paragraph located in lines from L455 to L475 as well as the C turnover rate is displayed in the Fig. 6.

Lines 521-522: Sites located in Norfolk and Mead have comparable temperature and precipitation. Did the authors look at the mineralogical characteristics of these sites? The difference may also be related to the C stabilization mechanisms.

Answer:

Norfolk and Mead are geographically close to each other (Fig. 1), and hence their general climates are similar. As noted in lines L213 - L215, precipitation in Norfolk and Mead were 696 and 747 mm yr-1, and temperature were 9.6, 9.9 °C, respectively. Although they are similar, Norfolk is slightly cooler and drier. In conjunction with Huron (which is even cooler and even drier), we noted this general regional pattern in lines L521 - L522. Based on first principles, we agree that soil properties are expected to drive the carbon accrual and mechanisms for carbon stabilization in the long term. As noted in the new Table 1, the texture of Norfolk is extremely sandy while Mead had a fine-textured soil; therefore, it seems that in the case of the relatively small sample size in this study (n=3; Huron, Norfolk and Mead), carbon accrual did not relate to soil properties, but instead there is a clear relationship between ‘age since tree planting’ and ‘carbon gains in the soil profile’ where the oldest site had the highest carbon accrual (Norfolk), the youngest site had the lesser carbon accrual (Huron), and Mead with an intermediate carbon accrual since it was intermediate in age of tree planting. The tree-C input over time can be so large and the modification of both microclimate and reduced disturbance so pronounced that these tree-related factors can be overriding other putative effects of soil properties on soil carbon storage, turnover, and stabilization.