SOIL Discuss., referee comment RC2
https://doi.org/10.5194/soil-2021-5-RC2, 2021

Comment on soil-2021-5

Anonymous Referee #2

Referee comment on "Nonlinear turnover rates of soil carbon following cultivation of native grasslands and subsequent afforestation of croplands" by Guillermo Hernandez-Ramirez et al., SOIL Discuss., https://doi.org/10.5194/soil-2021-5-RC2, 2021

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.

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.

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?

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.

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"?

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

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.

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

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.

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."

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.

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.

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.

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?

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

Line 456: I guess "study studies" should be "study sites".

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.

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.

Please also note the supplement to this comment:
https://soil.copernicus.org/preprints/soil-2021-5/soil-2021-5-RC2-supplement.pdf