

Referee comment 1

We are very grateful for the time you took to review our manuscript and for the valuable comments that will help improve the work. In this document, we have included each of your suggestions and have attempted to address them as best as possible (text in blue following each comment). All these comments will be taken into account and incorporated into the revised version of the manuscript.

This manuscript leverages a long-term conservation management trial and ^{14}C analysis to show that the soil carbon gains under regenerative agriculture can be rapidly lost once conventional crop management is renewed. Overall, the manuscript presents a clear concise dataset that supports the overall conclusions. There are some areas for improvement that are laid out below followed by a few line by line comments.

Introduction: The introduction is highly focused on implications and policy initiatives and doesn't sufficiently set up the questions that motivate the manuscript particularly for questions #2 and #3. For #2, the authors should provide background on how their management practices may result in more or less persistent soil carbon forms. In addition #3, restates #2 and then focuses on vulnerability to loss. Here a framing of a distinct #3 can focus on vulnerability to loss and the preceding introduction text could frame an argument for why the new C is vulnerable or persistent.

We appreciate the reviewer's comment regarding the structure of the introduction. We agree that there is opportunity to reduce the amount of text dealing with initiatives such as the "4 per mille" and to increase the focus on supporting the research questions. Particularly, we will follow the reviewer's suggestion to expand the literature review and develop a stronger introductory framework regarding the effects of including pastures in agricultural systems on COS dynamics.

Regarding the effect of including pastures in crop rotations on carbon persistence, this is precisely the knowledge gap that this study aims to address. In any case, we will do our best to improve the line of reasoning leading to the formulation of the research questions, while at the same time reducing the length of the text on topics that are not equally relevant, such as those focused on policy aspects.

Finally, we agree with the reviewer that research question 3 essentially repeats what is stated in question 2, so they could be combined into a single question.

Methods: The manuscript is a follow up to another similar study that occurred at the site. As such, the authors point to this paper for much of the methodology. However, there are some brief items that should still be described in this manuscript including: sampling design (how many cores) and replication, what sample points and plots were included in ^{14}C analysis and why. In addition, the data and code that is in the Github repository would benefit from a read me file that orients potential users to the files and what they are.

We thank the reviewer for this comment, which is highly relevant. First, regarding the experimental design, it is a block design with three replicates per treatment (three plots), of which we used only one treatment (three plots from the experiment) because this treatment was of particular interest for this study. From each of the three plots of this treatment, 20 core samples were extracted annually to form a composite sample representing the plot. Based on these

samples, the annual SOC time series was constructed, and subsamples were retained in a soil archive from which samples were subsequently selected for radiocarbon analysis. In summary, the sample dataset for the treatment of interest (Recovery system) contains 3 samples (one per plot) for each year since 1964.

Regarding the radiocarbon time series, due to the costs of analysis, it was decided not to analyze data for every year and not to analyze all plots in the years under study. The analysis scheme was as follows:

- 49 samples analyzed, representing 21 years of the LTE history.
- 7 years with data available for all three plots of the treatment (1971, 1972, 1984, 1996, 2009, 2011 y 2018).
- 14 years with data available for only two of the three treatment plots (1964, 1966, 1968, 1969, 1974, 1978, 1981, 1986, 1990, 1993, 1999, 2002, 2014, 2021).

The radiocarbon analysis scheme was designed to adequately capture the nuclear weapons testing peak and the subsequent decline in the radiocarbon signature after 2008.

We will include all this information in the revised manuscript.

Regarding the reviewer's suggestion to create a readme file for the code, we also find it appropriate and will do so along with the revised version of the manuscript.

Discussion: The discussion has some repetitive aspects that could be removed to enhance the conciseness and clarity of the text. There are multiple paragraphs that end with the same conclusion that there is an intermediate pool of carbon that increases under conservation management and then is lost when management is intensified. Removing some of this repetitive text would aid the manuscript.

This edit would allow the authors to speculate on what this intermediate pool could be and raise important hypotheses for future testing to find out how it is formed which could potentially aid in designing management strategies to prevent its loss. Is this carbon aggregate carbon or fungal necromass or particulate organic matter or dead roots?

We thank the reviewer for the comment regarding the structure of the Discussion section and the presence of repetitive text. We agree that many paragraphs tend to explore the same ideas. We will do our best when preparing the revised version of the manuscript to improve this aspect of the Discussion section.

Regarding the characteristics of the intermediate kinetics pool, this study does not provide any direct/measured information on its physicochemical properties, since its dynamics are derived from a model that represents it as a conceptual compartment without any direct measurements. Although we will try to improve the review and discussion related to this aspect, we want to make it clear that the modelled pools represent proportions of organic matter that cycle at approximately similar rates, but not necessarily unique physical objects. The pools are a way to organize all the complexity and heterogeneity of OM into proportions that are processed at different cycling rates.

Line by line comments:

Line 190: Very minor but I would reframe the language here as it is hard to use proxy measurements and models to “represent reality”. Models are more a hypothesis on how we think reality works.

We thank the reviewer for this comment and we agree with this point. In the revised version of the manuscript, we will modify this line of text so that it explicitly addresses the role that models as hypotheses about how the real system operates.

Line 465: This increase in fallow periods is not obvious to the reader in the methods discussion and therefore it stands out here as a confounding variable in the discussion. I would just be very specific about what the management practices are and what potentially could be driving losses.

To help improve this aspect, we will modify Figure 1 by adding a time scale to the horizontal axis so that the temporal dimension of the crop rotations that occurred in each period is clear.

The point raised by the reviewer is indeed poorly described in the Materials and Methods section. Until 2008, pastures were sown together with the last winter crop (wheat) in year 3 of the rotation. However, starting in 2008, year 3 of the rotation ends with a summer crop (sorghum), and the perennial pasture is not planted until year 4 of the rotation. This resulted in nearly one year less of effective pasture cover and, consequently, a modification in the temporal dynamics of carbon inputs.

In addition to revising Figure 1, the description of the crop rotations during the different phases of the LTE will be improved to clarify their implications for the study's results.

Line 510: What are these strategies?

We appreciate this comment because it shows that our discussion was overly general and not fully aligned with the state of the art regarding what is known about the mechanisms of carbon sequestration processes and, consequently, the associated agronomic practices. We will revise the text of this section of the discussion to focus not only on the results of this study but also on the current state of the art regarding the challenge of increasing or maintaining high SOC stocks. We will state that the literature indicates that increasing the more persistent C compartments is particularly difficult and that there is substantial uncertainty regarding which agronomic practices can effectively achieve this goal. It is also clear, both from the results of this study and from what much of the scientific literature on the topic indicates, that building slow-cycling carbon pools is something that can take considerably long periods of time, beyond the timescales relevant for climate change mitigation. For example, if we look at the structure of the model, we can see that a large proportion of new inputs is lost before it can enter the slow-cycling pool. This small proportion of inputs takes a long time to accumulate in the slow-cycling pool.

However, there are some lines of discussion that could be stated more explicitly as possible mechanistic hypotheses and, based on that, used to frame possible agronomic strategies more appropriately. Essentially, the implications of management changes on microbial growth and the generation of microbial necromass, as well as their implications for SOC dynamics, are interesting and relevant to discuss these aspects. However, as noted above, it is likely that, as observed in this study, storing C in slow-cycling pools may not be achievable on human timescales, even though C accumulation may still occur in compartments with faster or

intermediate cycling rates. We will improve the review related to this topic and include it in the discussion.