

Interactive comment on “Land-use perturbations in ley grassland decouple the degradation of ancient soil organic matter from the storage of newly derived carbon inputs” by Marco Panettieri et al.

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Dear reviewer,

We would like to thank you for the revision of our manuscript entitled “Land-use perturbations in ley grassland decouple the degradation of ancient soil organic matter from the storage of newly derived carbon inputs.”

We have carefully read reviewer’s comments and suggestions and we have performed the necessary corrections to the manuscript. In these revision notes the reviewer’s

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queries are reported in bold letters followed by our answers and comments. We hope that our responses and the changes we made in our manuscript make it suitable for its publication in SOIL.

Sincerely, Dr. Abad Chabbi in behalf of all the co-authors.

Revision notes:

Reviewer comment: Panettieri et al. have used stable isotope probing and ^{13}C NMR analyses to estimate the evolution of soil C pools in different land use. They focused on the OM light fraction, more sensitive to land use change, and compared their results obtained for four land use: permanent grassland, permanent cropping, ley grassland and bare fallow. The experimental design is very interesting to evaluate land use change effect on OM and especially on C pool isolated by fractionation. This manuscript provides really valuable information on the impact of land use change on OM dynamics and especially the coexistence of two distinct cycles of OM in ley grassland. Only minor modifications should be made to improve the manuscript.

Answer: We would like to thank the reviewer for his/her time and for his/her constructive comments. We provide the answers to his comments and concerns and modified the manuscript accordingly.

Reviewer comment: I think that, due to conversion to pdf format, all “ ^{13}C ” have to be checked because they are not in exponent. Similarly, the unit should be in exponent too.

Answer: In fact, a problem arose during the conversion to PDF, we apologize for this inconvenient. We have carefully checked the exponents in this revised version.

Reviewer comment: The authors used indifferently the terms “temporary grassland” and “ley grassland” (TG or LG) and “bare fallow” and “bare soil” (For example in figures or L346), they should choose one and use only one term. In section 2.1, they use ley grassland (LG) and I think it is the most frequently used in the manuscript.

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Answer: We adopted the terms “Bare fallow (BF)” and “Ley grassland (LG)” within the text and for all the figures and tables.

Reviewer comment: L1- : I think that “on” (focus on) is missing

Answer: We corrected this sentence.

Reviewer comments: L20 “with grassland returning to soil larger amount of C as belowground inputs than cropping systems”: This sentence is not clear. Does it mean that with grassland larger amount of C return to soil as belowground inputs than in cropping systems? L21 fresh inputs are preferentially incorporated at the level of microaggregates, which are enriched in C in comparison with those of cropped soils: It was not clearly evidenced. For example Figure 4 shows more incorporation of fresh residue in LMA and in figure 2, I am not sure that the difference between aggregate size is significant.

Answer: We have completely reworded the sentence at lines 20-23, explaining that belowground inputs are larger for grassland than maize crop under our experimental conditions (as showed in Panettieri et al. 2017 and Armas-Herrera et al. 2016). Furthermore, we removed the part on microaggregates focusing on the fresh maize inputs found in larger macroaggregates. This is to avoid confusion about the names of mid-sized aggregate fractions in the abstract, before the detailed explanation we provided in the manuscript.

Reviewer comments: L28 In consequence, vegetal inputs from a new land-use are creating new detritosphere microenvironments rather than sustaining the previous dynamics, resulting in a legacy effect of the previous crop: It is difficult to understand without reading the manuscript. It should be more detailed.

Answer: We added a more detailed explanation at lines 29-34, as suggested. The new version of the abstract including reviewer’s suggestions is now more readable as a stand-alone text.

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Reviewer comments: L207 Samples from permanent cropland showed the higher contribution of LF to total stocks of C among the four treatments: It is not so obvious on fig 2. Are the differences significant?

Answer: We reworded the sentence, as requested (actually lines 212-214). Significant differences between treatments of LF-C relative contribution to TOC were not highlighted for the bulk soil samples. Due to the experimental design, we cannot assess significance of the values for the aggregate fractions, but trends to higher relative contribution of LF-C to TOC were found for samples of permanent cropland compared with the other treatments. This is mainly because TOC of aggregates under permanent cropland was lower, but LF-C amount was comparable to the other treatments.

Reviewer comment: L229 to 233 “under ley grassland and permanent cropland, the MWD was higher for those two treatments if compared with permanent grassland and bare fallow soils”: according to table 2, the only significant difference in MWD is between PC and BF. This section should be modified.

Answer We modified this section as suggested (now lines 236-238).

Reviewer comment: L331 exploration of PCA indicated that the type of land-use lead to the highest distances for homologous LMA and MiA fractions: In most of the soils, LMA and S+C have the highest distances: The authors should explain why they choose LMA and MiA.

Answer: We were referring to the largest distances between homologous fractions from the different treatments, not between different fractions of the same treatment. We reworded the sentence accordingly (now lines 338-340).

Reviewer comment: L327: I agree with the authors, as chemical compounds are more important in bare fallow soil, they could correspond to higher status of degradation of LF. However L329, how do the authors could say that the difference of chemical composition between aggregate size corresponds to degradation status of LF? The

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difference could reflect different proportion between the OM source : microbial, or maize, or vegetation from grassland.

Answer: We agree, we have reworded the sentence as suggested by the reviewer (now lines 335-338).

Reviewer comment: L338 The fact that mineralization of LF-C from previous land-use was correlated to the N cycle: By previous land use, do you mean grassland? The previous sentence refers to bare soil. I think this sentence should be rephrased to avoid any misunderstanding. Considering my previous comment on OM source in aggregate size fractions, the link between mineralization status and N cycle is not straightforward here. The degradation status in the different fractions should be underpinned.

Answer: We have modified the sentence adding a brief explanation on how the litter degradation affect the relative composition of SOM and the redistribution of C pools (now lines 348-350).

Reviewer comment: L349 clearly indicating that LF-C of the treatments under maize presented a more degraded status: I agree but again (CF section 3.4), it is based on the assumption that OM from bare fallow is more degraded. In consequence the authors should clearly present this assumption before, as they did L353.

Answer: We reorganized this section as suggested (now lines 357-360). First we presented the assumption that bare fallow OM is more degraded, then we placed the sentence assessing that OM under maize present a similar degradation pattern for some of the aggregate fractions.

Reviewer comment: L381 to 390: I agree with the authors but I think that, in the comparison between PG and PC, rhizodeposition could play an important role. Indeed, as mentioned by the authors in the introduction, L223 section and conclusion, the root traits are very different. But maize provides belowground OM too. The authors should consider this OM source and its effect.

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Answer: We agree, we added a better explanation citing results from Panettieri et al. 2017 and Armas-Herrera et al. 2016 in which the contribution of aboveground and belowground inputs for grassland and maize were evaluated. Of course, maize provides belowground OM, we were referring to the most abundant type of input (Lines 393-400).

Sincerely, Dr. Abad Chabbi in behalf of all the co-authors.

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