

## ***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.***

**Anonymous Referee #1**

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Panettieri et al. have used stable isotope probing and  $^{13}\text{C}$  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

C1

be made to improve the manuscript. I think that, due to conversion to pdf format, all “ $^{13}\text{C}$ ” have to be checked because they are not in exponent. Similarly, the unit should be in exponent too. 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. L1- : I think that “on” (focus on) is missing 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.

L28 In consequence, vegetal inputs from a new land-use are creating new detritus-sphere 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.

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?

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.

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

C2

have the highest distances: The authors should explain why they choose LMA and MiA. 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 difference could reflect different proportion between the OM source : microbial, or maize, or vegetation from grassland.

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.

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.

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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