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## ***Interactive comment on “Free and protected soil organic carbon dynamics respond differently to abandonment of mountain grassland” by S. Meyer et al.***

### **Anonymous Referee #1**

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General comments: This manuscript is about effects of abandonment of mountain grasslands, studied along two LU-gradients (meadows to pastures to abandoned grasslands) on SOC dynamics. The study has been conducted at two sites in the Austrian (Stubai) and Italia (Matsch) Alps. The Matsch site is drier and has a greater MAT than the Stubai site. SOC dynamics was determined on soil samples (0-10 cm) using up-to-date methods including physical fractionation and bomb radiocarbon. The results showing that different SOM fractions respond differently to changes in grassland management (labile wPOM and fPOM-C fractions accumulate whereas there is little change in oPOM and mPOM-C) and that C accumulation rates decrease with time since abandonment are important for understanding SOM dynamics in relation to LUC.

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The title clearly reflects the content of the paper and all relevant parts of the manuscript are supported with appropriate references. Overall, the manuscript is nicely written, the figures and tables are intuitive and clear associated with concise legends. I do however, not understand why the authors have included the detailed information for temperature and soil moisture based on one year of measurement (Figure 2). As far as I see, Figure 2 has been referred to only once in the manuscript. In addition, the manuscript is about SOC dynamics in a longer time perspective than variations within one specific year. A small extension of Table 1 including the most important information in Figure 2 and a reduction of result section 3.1 would be one option to increase the quality of the paper. I do agree with the authors that the sampling strategy results in pseudo-replication. In addition, there are relatively few data for using more advanced statistics. In that case interpretations based on means and se are justified. However, this requires a careful presentation about trends and relationships in the data. Below, I have some specific comments to this. Due to its political (grassland abandonment occur across most of Europe) and scientific importance (enhanced knowledge of SOM dynamics important for mechanistic understanding and modelling) the manuscript should be published.

Specific comments: The abstract provides a concise and complete summary. I do however have a comment to one relatively important sentence: -Pg. 9944, L. 15-17. I have some problems to understand the sentence “The decomposition rates of labile wPOM-C declined with a decrease in litter quality, while both C input and C decomposition rates of labile fPOM increased with an increase in litter quantity.” According to Table 4 the decomposition rates for wPOM-C decreased in order Pasture > Abandoned > Meadow at Stubai and Meadow > Abandoned > Pasture at Matsch. Furthermore, decomposition rates of fPOM decreased in order Abandoned > Meadow > Pasture at Stubai and Pasture > Abandoned > Meadow at Matsch. I assume that the authors refer to a reduced litter quality following abandonment (e.g. changes in plant species composition, lower rates of N-cycling). However, litter quality is only briefly mentioned in the manuscript; the focus is on LU gradients. I suggest a more precise formulation of this sentence. -Pg. 9955, L. 3-17. Please see my comments above. If the authors decide to

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include this section, a sentence stating why the authors include these detailed one year results should be added. -Pg. 9955, L. 22-23. The authors are interpreting the results based on means and se and must therefore (because no significant statistical relationship is provided) be careful stating any trends in the data. In the sentence you state “Among SOM-C the wPOM-C and fPOM-C stocks were most affected and increased following management reduction”. A closer look at Table 2 reveals; wPOM and fPOM at Stubai: (Meadow = Pasture) < Abandoned. wPOM at Matsch: Meadow < Abandoned < Pasture and fPOM at Matsch: Meadow < (Abandoned = Pasture). I do agree that both wPOM and fPOM is (probably significantly) affected by a shift in LU, however, there is no clear increase following management reduction. The authors clearly state this (L.26 Pg. 9955 to L.4 Pg. 9956). Thus, my suggestion is to remove “increased following management reduction”. -Pg. 9956, L. 20-28. This section provides some estimates of turn over times for different SOM fractions based on bomb radiocarbon derived C decomposition rates. As an example in line 26 “For example, the mOM-C turned over in the range of 200–250 yr at Stubai and 142–200 yr at Matsch”. From Table 3 I calculate this based on  $1/k$ , i.e.  $1/0.005 = 200$  to  $1/0.004 = 250$  yr at Stubai and  $1/0.007 = 142$  to  $1/0.005 = 200$  yr at Matsch. If this is correctly interpreted, I do not understand the estimates for fPOM in L. 20-21 “sensitive free POM-C fractions turned over within 4–8 yr (???)”, while oPOM-C represented a slower cycling pool within the range of 76 (ok;  $1/0.013 = 76$ ) –142 yr (ok;  $1/0.007 = 142$ ”). Perhaps I have missed something here, but please clarify. -Pg. 9959, L. 17-18: “Among POM-C, only the increase in wPOM-C stocks can be related to a decrease in decomposition rates”. I do not quite understand this sentence when it comes to the Stubai site. wPOM stock at Stubai increases in order Meadow = Pasture < Abandoned and decomposition rates at Stubi decreases in order Pasture > Abandoned > Meadow. However, at Matsch wPOM stock increases in order Meadow < Abandoned < Pasture and decomposition rates decreases in order Meadow > Abandoned > Pasture. This makes sense, since lower decomposition would increase wPOM-C stocks. Please clarify or add “at Matsch” after “decomposition rates” in L 18. -Pg. 9960, L. 15-30: A general comment; the decomposition rates for oPOM-C

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and mOM-C are greater at the Matsch (warmer) as compared to the Stubai (colder) site. I do agree that this may be related to temperature. Perhaps is also worthwhile to mention that the oPOM-C and mOM-C stocks (Table 2) and input rates (Table 4) are greater at the Matsch as compared to the Stubai site suggesting greater biogeochemical cycling rates (but no significant effect on the total oPOM-C and mOM-C pools) at the “warm” as compared to the “cold” site?

Technical corrections: -Pg. 9944, L. 6. Add ‘two’ to “along “ land-use gradients”. -Pg. 9945, L. 15-20. This sentence is very long and has a poor readability. Please consider to re-write the sentence. -Pg. 9945, L. 10-11 and Pg. 9947, L. 1-2 Martinsen, 2010. This refers to PhD thesis. The paper including LUC effects on POM has been published in EJSS. Martinsen, V., Mulder, J., Austrheim, G. & Mysterud, A. 2011. Carbon storage in low-alpine grassland soils: effects of different grazing intensities of sheep. European Journal of Soil Science, 62, 822-833. -Pg. 9946, L. 8-9. “Gaudinski et al. (2000) estimated that the C in fine roots was fixed on average  $7\pm 1$  yr ago”. What type of system are the authors referring to? A temperate forest? Fine roots of what? The sentence should be more specified. -Pg. 9950, L. 11. For clarification; perhaps add ( $>1.6$  gcm<sup>-3</sup>) after mOM? -Pg. 9951, L. 20-22. Perhaps refer to Table 2 after “of all grasslands”? -Pg. 9952, L. 16-17. Perhaps refer to Table 2 after “abandoned grasslands”? -Pg. 9957, L. 23. Add “at Stubai” after “from pasture to abandoned grassland”.

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