Interactive comment on “Estimating maximum mineral associated organic carbon in UK grasslands” by Kirsty C. Paterson et al.

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Received and published: 16 September 2020

The authors present an interesting dataset and thoughts. However, it is not so clear what the take home messages are: The influence of soil types on <20 \( \mu \)m OC? One-time physical disturbance might not play a major role for <20 \( \mu \)m OC? The evaluation of other unifactorial empirical linear models? In its current form the manuscript contains a slightly confusing mixture. Based on the interesting data I suggest to do a major shift and set a new focus beyond unifactorial models. In Table 2 there are so many highly significant correlations between various factors. These should be explored further and related with carbon stocks to develop a higher potential of the manuscript and advance insights into potential processes that govern soil organic carbon stocks in UK grasslands.
Major issues I could identify:

- As described in line 98, the so-called mineral-associated OC ‘MAOC’ is isolated by sieving. The authors should provide evidence of mineral association or reword this.

- As authors write themselves in line 200 the soil properties vary beyond only the proportion of fine fraction, this should be explored further as stated above. In Fig. 4 it seems that the relationship exists only across sites but not within sites.

- The use of abbreviations sometimes makes the reading hard; this should be minimized as much as possible (soil type names for example)

- As described in line 247, the high variability of the proportion of the fine fraction might seriously confound the analysis of sward age. Is it possible to correct for this using a multifactorial model?

Further issues:

Line 15: Introduction very general, should be more specific towards the research questions

Line 19: What are specific challenges with this?

Line 46: What is the specific soil management involved?

Line 48: Other forms of OC would be probably also mineralized, why especially ‘MAOC’?

Line 51: Please add that such estimation of a ‘protective capacity’ is empirical

Line 143: One decimal would probably be sufficient

Line 187: “very little further testing […] in other soils” There are many papers cited in the manuscript that do exactly this to my understanding

Line 194: Such validity tests probably would include grassland soils which might not always be available (as paired site), also given that there are many other specific factors
what do you specifically propose?

Line 214: Interesting paragraph but far apart from the data presented here, what can you conclude and contribute to the discussion based on the data in this manuscript?

Line 223: Add reference of original measurements

Line 225: I disagree with such simplified relation. Fungal origin should be verified with another biomarker approach. High C:N could result from root input and particulate OM (as briefly mentioned in line 238). Could C:N results be influenced by mineral N fertilizer?

Line 237: What could this “other means” be?

Line 255: Could you add a literature reference here?

Line 445, Figure 1: The data is repeated in the Appendix. Should be present only at one spot.

Line 455, Figure 2: To improve the comparison between panels, I suggest to put similar x and y scales. Also add the significance level and remove regressions from the Figure when not significant.