

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

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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 $<20 \mu\text{m}$ OC? One-time physical disturbance might not play a major role for $<20 \mu\text{m}$ 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.

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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

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– 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.

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