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

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The manuscript under review investigates the mineral associated organic carbon (MAOC) distribution in grassland soils of varying sward age, across the UK. The authors compared the Hassink's reference equation to calculate the saturation capacity against alternative methods, which showed a more accurate assessments of carbon sequestration potential. The paper is of good quality, with a robust methodology and well written and developed in each sections. I don't have any major concerns but, rather, some points of discussion as following:

The forced intercept to 0 is generally suggested to avoid the paradox of having MAOM without any fine (silt and clay) fractions. However, in my experience with very large

datasets, I have never seen a soil without any fine fraction (at least temperate soils covered with any type of vegetation). It seems that the saturation equation is a type of function where the x domain is always >0. Indeed, the authors forced the intercept to 0 using the BL and QR methods, therefore, it would be worth to have a more in depth elaboration of this choice.

In the paragraph in line 255, the authors reported: "The C:N ratio of MAOC was 9.84 $\pm$ 1.00 (mean $\pm$ standard deviation) falling within the typical C:N range of fungi (4.5 to 15) whilst bacteria have a lower C:N ratio of 3 to 5 (Cotrufo et al., 2019), suggesting that the MAOC in the grasslands is predominantly of fungal origin." Indeed, this is an erroneously interpretation as MAOM is not entirely composed of living microbial biomass. C:N around 9 is on the average of European grassland (Figure 3 of Cotrufo et al., 2019), while other systems 'fungal-dominated' such a coniferous forests have a much higher C:N ratio. By the way, it would be interesting to know if C:N of MAOM differs significant across sites.

The difference between the Hassink's and UK equation implicitly suggests that a universal saturation equation likely does not exist, but many equations are controlled by interacting factors as mineralogy, soil microbial community etc. This is concept is developed around line 215 but the conclusion of the paragraph is quite elusive. I would encourage the authors to developed 'a way forward paragraph' that can guide a future research. I imagine, for instance, incubation experiments with unsaturated soils (according to those equations) where excess of high quality inputs are applied to see their 'real' saturation level. In this context, I wonder if authors can produce a plot of MAOM vs estimated C input, which may reveal (or not) some interesting correlations.

In the conclusion, the QR method is recommended but is not indicated at which quantile level. This makes a substantial difference in the relative proportion of saturated soils (table 4) and, hence, a possible perception of policy priorities. Are the soils mostly saturated or not? Is the index robust enough to provide management guidance? As is it, the conclusion left me a little bit hanging.

Line 78, hypothesis ii: I see also the way around. Since MAOC is less sensitive to disturbance (than POM), the ratio MAOC/SOC is negatively related to sward age. In other words, long-aged sward grasslands accumulate more POC , lowering the ratio MAOC:SOC. The table 5 reports only the absolute values.

Line 115: Is not clear if the comparison of MAOC across sites treats the ‘site as random factor (One Random Factor ANOVA).

Line 237-238. This statement does not explain the lower MAOC proportion in UK grassland compared to other ‘grassland’ sites. Was the MAOC separation method the same?

Table 3: please, add the r2 for completeness

Figure 2: please, add x (independent variable) in the equations

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