**Response to Reviewers: EGUSPHERE-2025-1711**

**Reviewer #1:**

**Authors**

Firstly, we would like to thank you for the time and effort you have put into this review. We believe that all your comments have been very valuable in improving the first manuscript submission.

In this document, we have included your comments in black and our response to each comment immediately below in blue color.

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Issues with chaOM fraction:

Figure 3a. The reported mean C/N ratio for the chaOM fraction is 4.41, and several values appear to fall between 0 and 2. These values are exceptionally low for any known pool of organic matter. I recommend that the authors clarify whether the C/N values presented for the soil fractions reflect total N or organic N (i.e., after subtracting inorganic N). If only total N was used, recalculating C/N based on organic N would likely provide a more accurate picture of the biochemical composition of these fractions.

**Answer:**

We are not aware of the removal of inorganic N prior to soil fractionation in other studies, and in soil, C:N ratios are always calculated as OC:TN, given the inorganic N pool is typically negligible. Additionally, given that the fractionation protocol includes several rinsing steps with water, and that ammonium and nitrate are highly soluble, we would expect most of the inorganic N to be removed during the fractionation process. Therefore, the C/N ratio reported in this study primarily refers to organic C and N contents. That said, we are thankful the reviewer brought our attention to these low values, as we share the concern regarding their being unrealistic. This fraction is largely made of sand with minor amounts of OM, thus it is particularly challenging to get reliable estimates on the Elemental analyzer. We have reanalyzed 30 random chaOM samples (15% of the dataset) on another elemental analyzer (CN802, VELP Scientifica, Italy) that can analyze larger samples with greater precision and accuracy, but to which we did not previously have access. We detected significant deviations from the initial measurements only in samples with very low C and N contents (below 0.3% and 0.09%, respectively) in the chaOM fraction. These values were close to the calibration limit of the elemental analyzer used initially to analyze the samples. To correct this analytical limitation, we have reanalyzed all samples whose C and N contents were below the aforementioned thresholds using the new analyzer. In total, we reanalyzed 111 chaOM samples, making up 59% of the entire dataset and four MAOM samples (2% of the entire dataset). When analyzing the corrected data, we have observed much more plausible C/N ratio values for the chaOM fraction (Figure 1), although they are still relatively low. The MAOM and lPOM C/N ratios have changed slightly compared to those presented in the first manuscript submission (Figure 1) due to the new C and N content values of the four MAOM samples that were reanalyzed, as well as the exclusion of five samples that did not meet the

C recovery check (explained in lines 255-256 of the former manuscript) after chaOM C content correction. We are much more confident in our results after these reanalyzes.

This new data has not changed the overall story, but it has resulted in a few changes that we report below, at the end of this document.

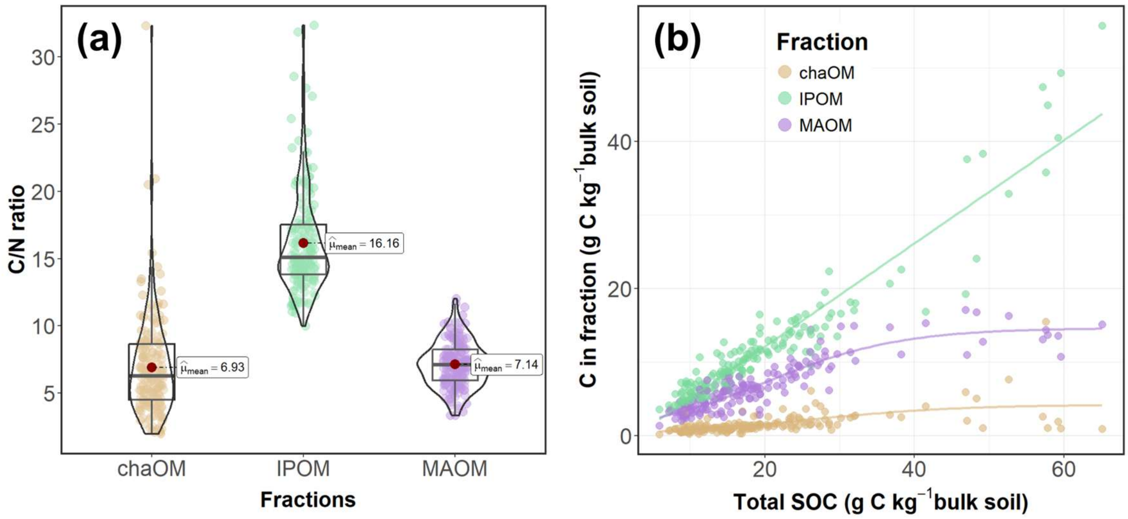


*Figure 1. Violin plot and boxplot (with median and quartiles) and mean values (expressed with labels and red dots) for the ratio of carbon to nitrogen content (C/N ratio) in the soil organic matter fractions (a), and relation between carbon content in the soil organic matter fractions (in g of C per kg of soil) and the total soil organic carbon (SOC) content (b). "chaOM" refers to the coarse heavy mineral-associated organic matter, "lPOM" to the light particulate organic matter and "MAOM" to the fine mineral-associated organic matter. In panel "b", a linear regression line depicts the relationship between lPOM and total SOC, while logistic curves illustrate the relationships of chaOM and MAOM with total SOC.*

The manuscript reports the use of a combined size-density fractionation protocol following Leuthold et al. (2024), in which the light fraction (<1.85 g cm$^{-3}$) is separated from the heavier material. The latter is subsequently sieved at 53 µm to yield a coarse heavy-associated organic matter fraction (chaOM; >53 µm) and a fine mineral-associated organic matter fraction (MAOM; <53 µm). The chaOM and MAOM fractions were later combined and reported together as MAOM.

While the decision to include chaOM within MAOM appears justified based on the rationale provided in Leuthold et al. (2024), this approach diverges from the more widely accepted definition of MAOM as material denser than 1.6–1.85 g/cm$^3$ and smaller than 50–63 µm (Lavallee et al., 2020). Consequently, this choice may limit the generalizability of the findings and their integration into broader syntheses. That said, since the chaOM fraction accounted for less than 5% of total SOM, the implications are likely minor.

Nonetheless, as emphasized by Lavallee et al. (2020), there is a pressing need for greater consistency in the operational definitions of POM and MAOM to facilitate comparability across studies. I recommend explicitly acknowledging that organic matter >53 µm is not typically included in the MAOM fraction.

**Answer:**

The referee is correct that the chaOM was originally considered distinct from the MAOM and more similar to the POM. In fact, it was initially named heavy POM and included in the POM fraction during size separation. However, more recent work has pointed to the distinct nature of the chaOM fraction from POM, suggesting that it should be considered coarse MAOM. For instance, Samson et al. (2020) found that chaOM characteristics and sensitivity to inputs quality were in transition between both MAOM and POM fractions. In addition, Leuthold et al. (2024) found that, upon a deeper study of the chemical and spectroscopic composition of soil fractions, chaOM and MAOM were more similar to each other than to light POM. In this regard, we believe that, in our study, once we have observed that MAOM and chaOM share similar C/N ratios and relationships with total SOC (Figure 1), it would be inconsistent to mix chaOM and light POM together. Other studies combining chaOM and MAOM in a single fraction after assessing C/N ratios are not rare (Santos et al., 2024; Zhang et al., 2021), and some global meta-analysis combine datasets from both only-density and only-size fractionation methods (Chang et al., 2024; Georgiou et al., 2022). In fact, many studies that only perform size fractionation do not show the C/N ratios of the fractions (Dai et al., 2025; Díaz-Martínez et al., 2024), whereas our study is transparent regarding the success of fractionation and the different properties of the isolated fractions.

In any case, we agree with your proposal to explicitly acknowledge that organic matter >53 µm is not typically included in the MAOM fraction. We have replaced lines 259-261 of the former manuscript with this paragraph (including also a new figure in supplementary material):

*"Since the chaOM and the MAOM shared similar C/N ratios, which were lower than POM C/N ratios (Fig.3a), we merged these two mineral associated OM fractions and present them together as MAOM (Santos et al., 2024; Zhang et al., 2021). Thus, in this work, POM and MAOM represented the light (<1.85 g cm $^{-3}$) and heavy (>1.85 g cm $^{-3}$) fractions respectively. This contrasts with other studies based on size fractionation, in which light POM and chaOM are pooled together (Cotrufo et al., 2019a; Dai et al., 2025; Díaz-Martínez et al., 2024). However, our decision is supported by recent findings of Leuthold et al. (2024) who observed greater chemical similarity between chaOM and MAOM than between chaOM and POM. The pooling of the two mineral associated OM fractions is not expected to modify the results or interpretation of this work, as the C content of chaOM only accounts for an average of 8% of the total SOC, and its relative importance is similar across all managements (Fig. S1)"*

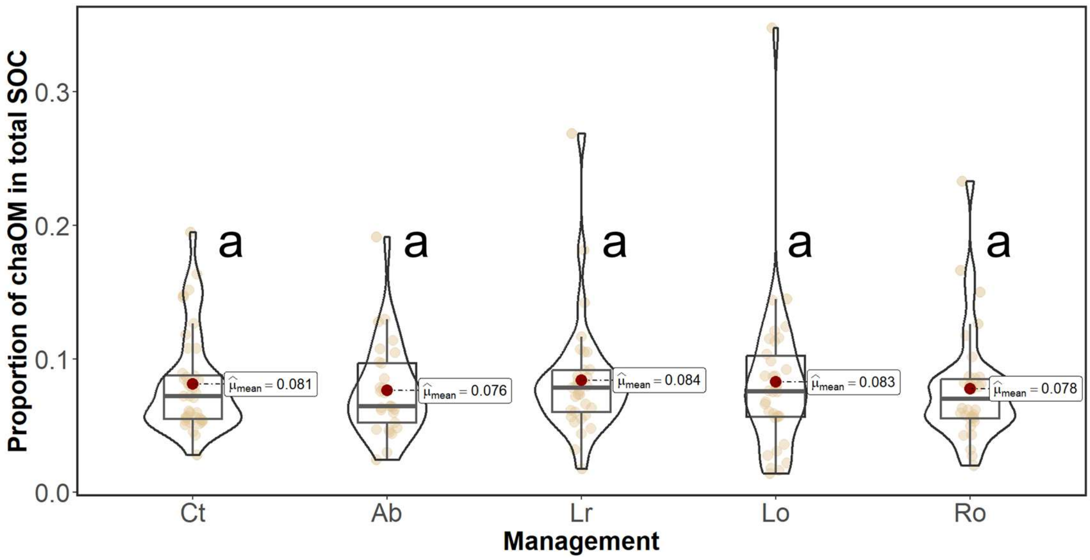


*Figure S1. Violin plot and boxplot (with median and quartiles) for the ratio between coarse heavy mineral associated organic carbon (chaOC) and soil organic carbon (SOC in each management (Ct = Continuous grazing; Ab = grazing abandonment; Lr = Recent legume sowing; Lo = old legume sowing; Ro = Rotational grazing). Labels and red dots indicate mean value. Lower case letters indicate significant differences ($p < 0.05$) between managements according ANOVA testing.*

L184-187. 2.2. Climatic variables. This section consists of a single sentence, which may not warrant a standalone subsection. I suggest either expanding it with additional context or incorporating it into a broader methodological section.

**Answer:**

We agree with your proposal. Instead of expanding the section, we have decided to remove it entirely and include a reformulation of lines 149–152 of the former manuscript:

*"The region has a continental Mediterranean climate, but on a local scale, in relative terms, farms can be grouped into three main climatic regions (Fig.1b, c) according to the average climate for the period 1980–2018 (García Bravo et al., 2023). A cold-dry region [12.9 ºC mean annual temperature (MAT) and 445 mm mean annual precipitation (MAP)] in the north; a warm-wet region (17.3 ºC MAT and 603 mm MAP) in the middle of the latitudinal gradient; and a warm-dry region (17.0 ºC MAT and 510 mm MAP) in the south."*

L248. SOC was already defined.

**Answer:** Thanks for noticing, we deleted the SOC definition here.

L249-250. It would be useful to specify whether inorganic carbon (e.g., carbonates) was measured or removed prior to C analysis.

**Answer:**

Carbonates concentration was measured in all bulk soil samples, with a value of 0 in all cases. We have included a brief explanation in this regard after line 248 of the previous manuscript, in section '2.5. Soil organic matter fractionation":

*"First, 10 g aliquots of all samples were ground in a ball mill and the Bernard's calcimeter method (Sherrod et al., 2002) was used to test for the presence of inorganic C. Only four samples (0.02 % of the database) contained traces of carbonates, with $CaCO_3$ contents between 0.2 % and 0.8 %. These samples were excluded from subsequent analyses."*

Figure 4. Please add the slope of the regression line. This figure should be in the results section.

**Answer:**

Suggestion followed. We have moved Figure 4 to the Results section, right after line 354 of the original manuscript, and included the slope of the regression line in the plot: "MAOC = (0.86 ± 0.9) * CS"

L299. This sentence should be included in another paragraph.

**Answer:** We have moved it to the end of the section.

Figure 5

- The PCA plots refer to the components as "Dim", whereas the figure legend refers to them as "Axes". For consistency and clarity, I recommend using a single term.

**Answer:**

Thanks for the suggestion, we have changed all component names in the PCA to "Axes" for consistency.

- Axis 1 is labeled "C/N axis". However, C/N is a single measured variable, and a PCA is not required to assess it alone. This axis also includes strong loadings from other traits such as ABG cellulose and CWM hemicellulose. Therefore, the axis appears to represent a broader concept, perhaps related to litter quality or decomposition potential, rather than just the C/N ratio. I suggest reconsidering the label to better reflect the multidimensional nature of the trait composition.
- A similar issue applies to Axis 3, labeled "Lignin axis". Lignin is again only one of several traits contributing to the axis. Since this component also captures variation in ABG hemicellulose and CWM LNC, among others, it likely reflects a broader litter chemistry or quality gradient rather than lignin content per se.

**Answer:**

We agree with the reviewer that the axis labels do not capture their multivariate nature, and their names can be confused with those of variables included in the PCA. We have changed the first axis name to "N-C stoichiometry", because it largely reflects the C/N ratio of plant inputs, which is obviously positively correlated with the cellulose and hemicellulose content (the source of C in plant tissues) and negatively correlated with the N content of vegetation and its leaves. With regard to the third axis, we have changed its name to "productivity-lignification (P-L) axis" to highlight that the axis is negatively correlated with the plant ABG production and positively correlated with lignin content.

- In panel (h), ABG hemicellulose is more negatively correlated with the axis than CWM SLA, yet only CWM SLA is shown in the schematic.

**Answer:** Sorry about this omission, we have included ABG hemicellulose in the schematic.

- Finally, I recommend reducing the number of figure panels by integrating each schematic (panels e–h) below the corresponding PCA biplot (a–d). This would streamline the presentation while preserving the valuable visual summary of trait syndromes.

**Answer:** That is a very good idea, we have changed the figure accordingly. Attached below is the figure with all the proposed corrections (Figure 2).

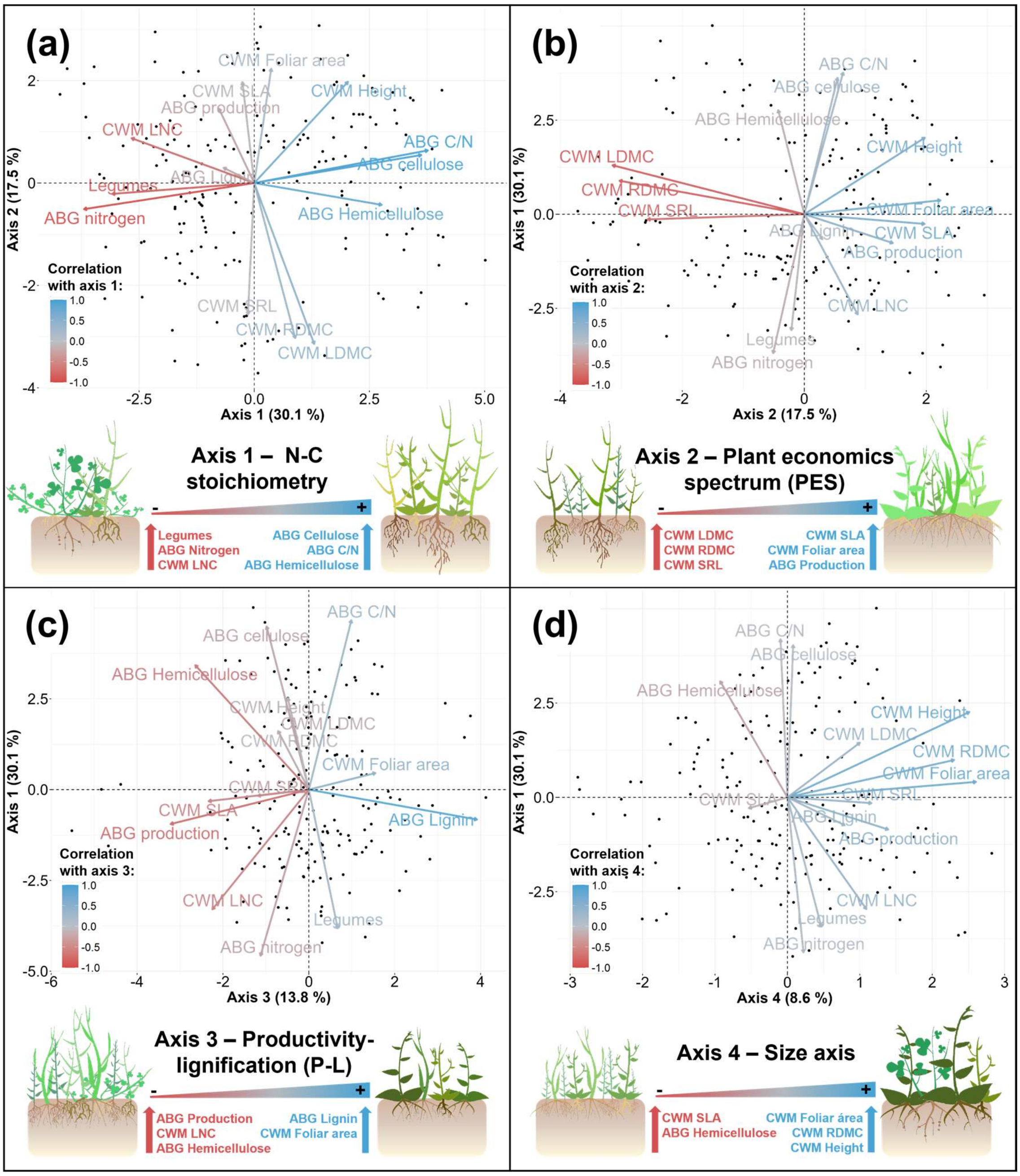


*Figure 2. (Figure 5 in the manuscript) Representation of the 4 main axis of variation in the principal component analysis (PCA) summarizing the vegetation characteristics variables. Panels a, b, c, and d illustrate the correlation between the different variables included in the PCA and the new axis, with a graphical representation of the characteristics of the plant communities at the end of each axis. Representative species of each axis are shown in Fig.S1.*

L322. … bulk density (BD). What was the range of BD? Was it similar across treatments? If not, equivalent mass corrections should be considered.

**Answer:**

The bulk density values in our data range from 0.66 to 2.1 g/cm$^3$, with some variation between the treatments. However, the bulk density used in equation 1 to calculate carbon (C) stocks is the bulk density of the fine soil (less than 2 mm) after excluding coarse material (gravel and large roots). This fine soil bulk density ranges from 0.43 to 1.66 g/cm$^3$. Since the gravel content in the soil differs between treatments, the distribution of fine soil bulk density across treatments does not exactly match that of normal bulk density (more information below). To limit the influence of our approach, we also analyzed the POC, MAOC, and SOC contents (Figures S4 and S5 of the former supplementary material). The POC, MAOC and SOC content analysis is briefly discussed in lines 547-548. The results of the content and stock analyses were very similar. To be clearer and more transparent about this issue, we have included the following lines in section "2.6: Data Analysis," below line 343 of the original manuscript:

*"Given that average bulk density values partly differed among managements (Figure S3), and that this variation may affect carbon stocks, the same analysis procedure used for POC, MAOC and SOC stocks was used to analyze the change in POC, MAOC and SOC contents."*

In this study, where samples were taken at a fixed depth, mass equivalent correction of C stocks would be equal to analyzing C content, as we have done.

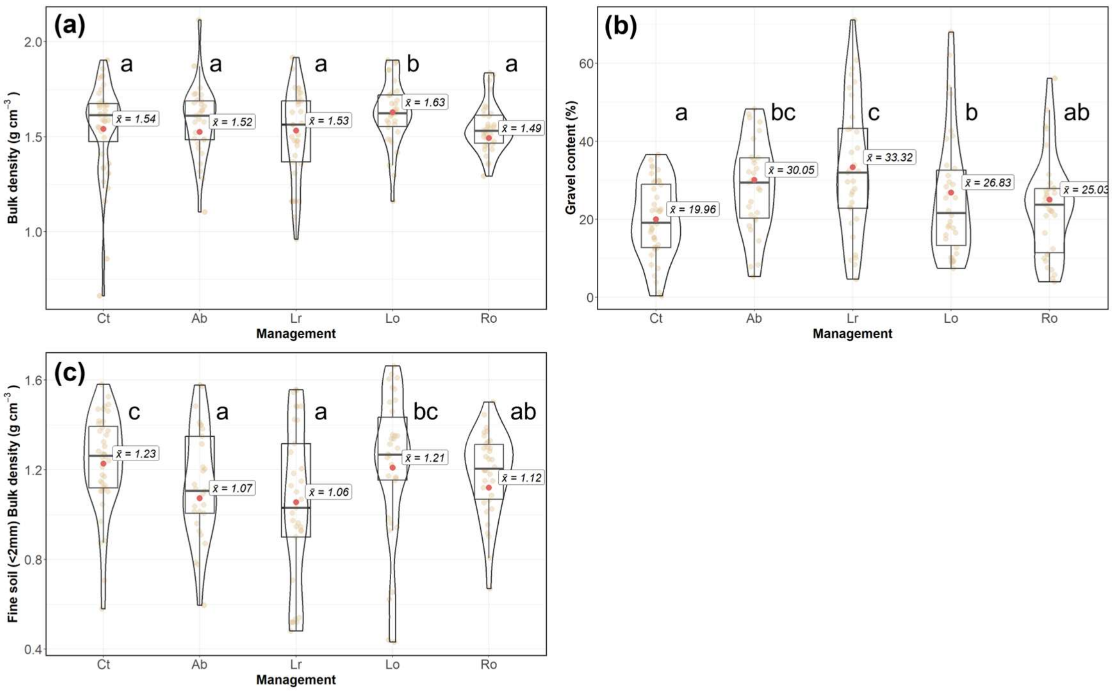


*Figure S2. Violin plot and boxplot (with median and quartiles) for the (a) soil bulk density, (b) soil gravel content and (c) fine soil (<2mm) bulk density, as used in equation 1 in the main text, in each management (Ct = Continuous grazing; Ab = grazing abandonment; Lr = Recent legume sowing; Lo = old legume sowing; Ro = Rotational grazing). Labels and red dots indicate mean value. Lower case letters indicate significant differences ($p < 0.05$) between managements according ANOVA testing.*

L340. Could you please clarify what is a direct and indirect effect?

**Answer:**

Lines 329-331 specify the direct and indirect (through effects on other variables) paths through which the effects of the variables can be transmitted.

L364-365. "…informing on lignin content and vegetation productivity, was negatively correlated with the POC, MAOC and SOC stocks and the microbial biomass". Lignin content was negatively correlated with vegetation productivity. Therefore, those variables were negatively correlated with lignin, but positive correlated with vegetation productivity.

**Answer:**

That is correct, but in this case, we are explaining that the third axis of the PCA (now the "productivity-lignification (P-L) axis") is negatively correlated with POC, MAOC, and SOC stocks and microbial biomass. Figure 5 shows that this PCA axis is positively correlated with lignin and negatively correlated with ABG productivity.

L430-432. "The silvopastoral character of our farms could explain the low MAOC/SOC ratios, as litter from scattered trees increases carbon stocks in woody grasslands, especially in the POM fraction". This explanation is plausible; however, your results show that lignin and C/N were negatively correlated with POC. How do you reconcile this apparent inconsistency?

**Answer:**

To avoid this inconsistency, we have decided to remove the interpretation of low MAOC:SOC ratios due to the silvopastoral character of the study area (lines 430-432 of the former manuscript), maintaining the explanation based on sampling depth, which is more robust.

L439. I don't see a saturation curve in Fig. 3 b. Figure 4 shows a better way to illustrate the C-saturation deficit. It would be interesting to compare the results with the maximum C-loading found by Georgiou (~ 86 mg C g silt+clay).

**Answer:**

Following the reviewer's suggestion, we have included a saturation curve in Figure 3b (Figure 1 on this document). On the other hand, in line 439, we point out that even though our soils were far from the MAOC saturation capacity according to the results of Georgiou (as shown in Figure 4), we still observed a limit to MAOC accumulation at high total SOC concentrations. This is shown in Figure 3b, where POC increases linearly with increasing SOC, but MAOC seems to saturate and stay stable above SOC contents of 30 g/kg$^{-1}$. To clarify this point, we have rewritten lines 437-440 of the former manuscript:

*"Carbon concentrations in the fine soil fraction (clay + silt) were well below the saturation point observed in previous studies (Cotrufo et al., 2019b; Georgiou et al., 2022). However, we observed a certain limit to MAOC accumulation in our system, as its content remained stable above SOC contents of 30 g/kg$^{-1}$ and stayed below 20 g kg$^{-1}$ even when SOC reached values above 60 g kg$^{-1}$, following a saturation curve (Fig. 3b)."*

L497-498. However, roots promote MAOM formation not from root biomass but from rhizodeposition (see Sokol, N.W., Bradford, M.A., 2019. Microbial formation of stable soil carbon is more efficient from belowground than aboveground input. Nature Geosci 12, 46–53. https://doi.org/10.1038/s41561-018-0258-6; Villarino, S.H., Pinto, P., Jackson, R.B., Piñeiro, G., 2021. Plant rhizodeposition: A key factor for soil organic matter formation in stable fractions. Science Advances 7, eabd3176.)

**Answer:**

We agree that this positive relationship between rhizodeposition and MAOC formation somewhat contradicts our discussion on this point. Without data on rhizodeposition or root biomass, we can only propose and discuss possible explanations for the relationship between the size axis and MAOC stock, pointing out their shortcomings. Therefore, we will reword the paragraph from lines 502 to 510 of the former manuscript as follows, including the references proposed by the reviewer on the role of rhizodeposition:

*"We also observed that taller and larger plants (high values on the size axis) were associated with higher MAOC stocks and MAOC/SOC ratios, although the mechanism driving this correlation was unclear. Generally, plant height is positively correlated with shoot:root ratio (Li et al., 2008), and several studies have found a higher contribution of shoots, rather than roots, to the MAOC fraction due to the higher recalcitrance of root tissues (Huang et al., 2021; Lavallee et al., 2018; Ridgeway et al., 2022). However, rhizodeposition, which is closely linked to root biomass, has been shown to promote MAOC formation over POC (Berenstecher et al., 2023; Villarino et al., 2021). On the other hand, a greater accumulation of standing litter, rather than surface litter, might be expected in communities with bigger plants, and some studies in semi-arid grasslands have observed higher rates of microbial degradation and release of soluble compounds (thus contributing more to MAOC) in standing litter, compared to surface litter, due to greater retention of night-time moisture in the former (Gliksman et al., 2018; Wang et al., 2017)."*

L546. In line with my previous comment, bulk density values and changes should be reported. Previously, the acronym BD was used for apparent density.

**Answer:**

Please see our answer above about BD.

L590. Mineral-associated organic carbon was previously defined as MAOM.

**Answer:**

Thanks for noticing, we have removed the definition here.

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**Additional changes:**

The changes made to the database following the reanalysis of the samples mentioned in the response to the first comment have resulted in some slight changes to the study results, which are outlined below.

The effects of rotational grazing have become significant and positive for both POC and MAOC (Figure 2.a & b). In the previous analysis, these effects were only significant in the case of MAOC and were close to being significant for POC (Figure 7 in former manuscript). On the other hand, the negative effect of recent legume sowing on the MAOC/SOC ratio is no longer significant, and the positive effect of abandoning grazing has become significant (Figure 2.d). The rest of the relationships have remained the same as those reported in the previous manuscript.

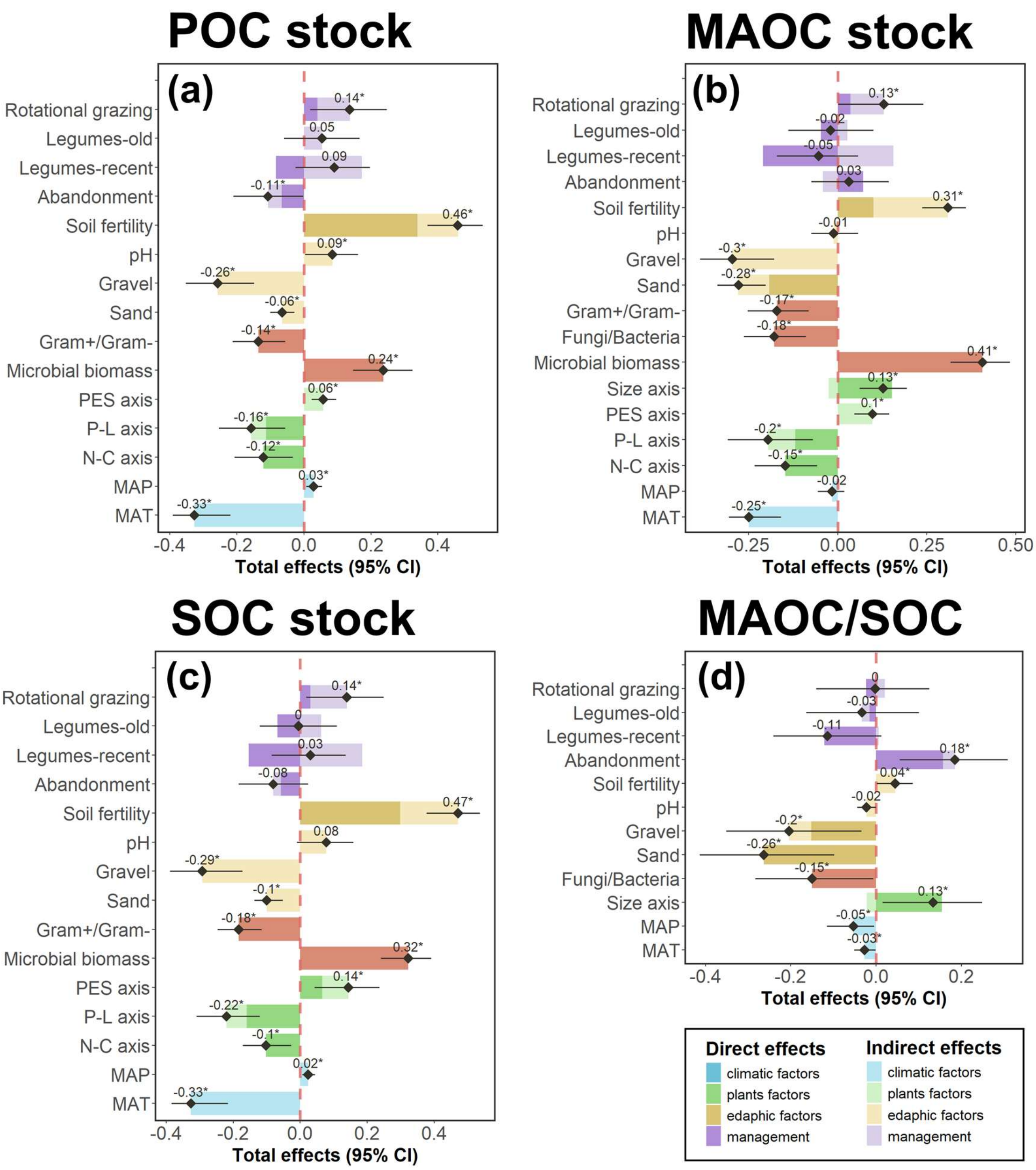


*Figure 2. Direct, indirect and total standardized effects of all studied variables included in the structural equation model (Figure6) over the (a) particulate organic carbon (POC), (b) mineral-associated organic carbon (MAOC), and (c) soil organic carbon (SOC) stocks and (d) the relative MAOC abundance (MAOC/SOC). Bars indicate direct (dark colors) and indirect (light colors) effects, and the black points-ranges indicate the total (i.e. direct + indirect) effect (with its 95% confidence interval). Stars over the total effect values indicate significant effects at a level of 0.05.*

**References:**


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**Reviewer #2:**

**Authors**

Firstly, we would like to thank you for the time and effort you have put into this review. We believe that all your comments have been very valuable in improving our manuscript.

Bellow, we have included your comments in black and our response to each comment immediately below in blue color.

Please note that, motivated by reviewer 1's justified concerns about the data for the chaOM fraction, we have reanalysed 111 samples from that fraction, which has slightly changed some of the study's results. These changes are detailed in the response to reviewer 1.

**Specific comments**

**Abstract:**

Please describe the reason for only sampling to 8 cm depth in the introduction and/or M&M, and consider mentioning the depth in the abstract or, alternatively, write "upper topsoil" instead of just "topsoil" in the abstract. I believe there might be arguments for choosing a relatively shallow depth, but the arguments are not mentioned nor discussed (microbial dynamics predominantly occurring in the upper topsoil? No/low tillage, etc.). In terms of the management effects on soil C stocks, the conclusions of the study are limited to the upper 8 cm - which should be discussed.

**Answer:**

Thank you for this comment. We have added the specific depth in the Abstract line 20 "on topsoil (0-8cm) SOC ..." and in the Introduction, line 117 "The aim of this work is to evaluate the impact of rotational grazing, LRP and grazing exclusion on topsoil (0-8cm) SOC stocks ...". In the Method section 2.4 we already report the soil depth sampled.

Additionally, we have included the following paragraph in the discussion, after line 423 of the former manuscript, to justify our decision and discuss its limitation:

*"Our results are limited to the upper topsoil (the first 8 cm), and it would be necessary to analyze deeper layers to fully understand the processes of SOC formation and stabilization and the effects of management changes. However this top layer is the most important for mediterranean grassland functioning as it contains the majority of roots and therefore most of the microbial, nutrients and water dynamics (Acosta-Gallo et al., 2011; Moreno et al., 2005). Furthermore, changes in management primarily affect the SOC of the topsoil layer, especially in the short term. (Ward et al., 2016)"*

**Keywords/introduction:**

I find the expression "Legume enrichment" slightly unclear/confusing. In my opinion, it connotes with "isotope enrichment experiments", where the legumes themselves are isotope enriched. Consider if writing "Legume sowing" og "legume inclusion/addition" as keyword and where it is mentioned in the manuscript (e.g., line 58), could be an alternative.

**Answer:** We appreciate the suggestion and have replaced "enrichment" with "sowing".

L109-112. Species richness effect on MAOC/POC fractions in managed grasslands have been investigated recently by Mortensen E.Ø., Abalos D., Engedal T., Lægsgaard A.K., Enggrob K., Mueller C.W., Rasmussen J. (2025) Smart mixture design can steer the fate of root derived carbon into mineral-associated and particulate organic matter in intensively managed grasslands. Global Change Biology 31: e70117. DOI: 10.1111/gcb.70117. The study observed no effect of species richness *per se*, but effects of plant functional group (grasses vs. legumes). Please consider this in the introduction and/or discussion section, i.e., how that compares with the findings of your study.

**Answer:**

The article is very interesting, although it focuses solely on the formation of MAOC and POC derived from rhizodepositions, so its comparability with our study is limited. However, we have considered it appropriate to cite in the introduction (line 109-112):

*"Other vegetation characteristics such as species richness has been shown to positively influence SOC stocks (Lange et al., 2015; Steinbeiss et al., 2008), but its effects on SOC fractions have been poorly evaluated, with inconclusive results in grasslands (Mortensen et al., 2025). In addition, the relationships between SOM stocks and fractions and plant functional traits have rarely been studied (Manning et al., 2015; Mortensen et al., 2025; Xu et al., 2021) despite the latter being widely used to predict ecosystem functioning and responses (Funk et al., 2017)"*

In addition, we have referred to the article in the discussion (lines 477–478 and 484–486 of the previous manuscript).

**Material and method:**

L162: What is the lower range of time for the legume enrichment/sowing? Does the study also include paddocks where legumes have been sown within the last year or 2, or is there a minimum of "incubation time" for the effects to be observed? E.g. "5-2 years"?

**Answer:**

Table S1 provides information on the year of sowing in paddocks sown with legumes. Some paddocks were sown only 1 or 2 years before sampling. These sowings are usually carried out in October, and the effects can be observed in the same growing season (the following spring).

L163-165: It would be good with a few more words on the legume species that are chosen in those mixtures that are seeding on the farms, instead of only mentioning the broad genus names and referring to another study. Is it always the same seed mixtures, and/or on which criteria are the seeded species selected? Which are the most dominating legumes in the farms that are already established before legume sowing (based on the botanical assessment)? Here, I am thinking specifically on the root systems, whether shallow and/or deep-rooted legumes existed before/are sown. I think this is relevant for the reader, especially because of the no/negative effects of recent legume sowing on SOC and MAOC stocks. The type/species of sown legumes may have an influence on which effects legume sowing induce/not induce in the upper 8 cm of the topsoil – and which potential effects below 8 cm may not be covered in this study. This caveat should be mentioned when discussing the results.

**Answer:**

Seed mixtures are mainly marketed by a single private company in Portugal and Spain, and the specific composition remains essentially constant from year to year. In this sense, most farmers sow the same mixtures (it is a widespread practice in the area). To clarify this point, and include the sowed species, we have rewritten lines 162–165 of the previous manuscript:

*"- Recent legume sowing (Lr): Paddocks where pastures have recently (≤5 years) been sown with legume mixtures. In dehesa farms, legume sowing consists of sowing a mixture of seeds (at a rate of 20 kg ha$^{-1}$) from various species of annual legumes (pre-inoculated with Rhizobium) such as Trifolium subterraneum L., T. incarnatum L., T. michelianum Savi., T. resupinatum L., T. mutabile Port. and Ornithopus sativus Brot., along with some highly productive annual grass species such as Lolium multiflorum Lam. and Lolium rigidum Gaud. (Teixeira et al., 2015)."*

It should be noted that all sown species are annuals with relatively shallow root systems. Therefore, the effects of sowing would be expected to be noticeable at 8 cm soil depth. Importantly, several of the species sown can be found naturally in these pastures, or at least species with similar characteristics. These sowings only increase the proportion of these species (more productive and mainly legumes) in the community.

L202: I don't find any results on the aboveground species botanical composition. In line with the comment above, a (supplementary) table with species composition, or at least mentioning the dominant species, would increase our understanding of the system. Does this vary too much between the farms to provide this information, or are there some general species (non-legumes and legumes) that dominate/characterize the farms used in the study?

**Answer:**

Our vegetation surveys have identified more than 200 species. It should be noted that these Mediterranean grasslands tend to be highly diverse, typically containing 10-20 species per square meter in our study and up to 40 species according to literature. The farms, managements, and climatic regions share most of the species. To clarify this point, we have included a table in the supplementary material listing the common species in each functional group (grasses, forbs, and legumes) and have rewritten lines 147–150 of the previous manuscript:

*"The herbaceous layer is composed of species typical of Mediterranean pastures and presents a high diversity and proportion of annual C3 plants (Table S1).*

***Table S1**. Examples of the most representative species found in the floristic inventories of the study for each functional group (grasses, legumes, and forbs).*

| ***Species*** | ***Functional group*** |
|---|---|
| *Lolium rigidum Gaud.* | *Grass* |
| *Bromus hordeaceus L* | *Grass* |
| *Hordeum murinum L* | *Grass* |
| *Vulpia geniculata L.* | *Grass* |
| *Anthoxanthum aristatum Boiss.* | *Grass* |
| *Agrostis pourretii Willd.* | *Grass* |
| *Trifolium subterraneum L* | *Legume* |
| *Trifolium glomeratum L.* | *Legume* |
| *Ornithopus compressus L* | *Legume* |
| *Vicia sativa L.* | *Legume* |
| *Lathyrus angulatus L.* | *Legume* |
| *Hymenocarpos lotoides L.* | *Legume* |
| *Anthemis arvensis L.* | *Forb* |
| *Echium plantagineum L.* | *Forb* |
| *Crepis capillaris L.* | *Forb* |
| *Plantago lagopus L.* | *Forb* |
| *Sisymbrium officinale L.* | *Forb* |
| *Rumex acetosella L.* | *Forb* |

L231: As mentioned above, 8 cm is quite shallow when considering the effects of species on soil C. Please, argue why 8 cm depth was chosen instead of e.g., 15, 20, or 25 cm that would include the effect on a larger part or the entire topsoil layer.

**Answer:** As we mentioned above, we have added a sentence in the discussion arguing why we chose to sample 8 cm depth and the limitations of our study as compared to sample deeper soil layers.

L281: Is the reference ("as shown in eq. 1") referring to the same equation that are used to calculate OC in soil C fractions? If so, please make this clear, as it does not seem obvious that microbial biomass stocks can be calculated by PLFA via the same equation.

**Answer:** We have clarified this point by rephrasing lines 280–281 of the former manuscript:

*"Further, we estimated microbial biomass stocks (in mol $ha^{-1}$) by substituting the $OC_{content}$ by the total PLFAs concentration in equation 1."*

L304: Does "*the latter being considered a good proxy for the lignin content (Van Soest et al., 1991).*" refers to ADL – or to "ADF minus ADL"? If it's the first, I suggest clarifying it by writing:

“… *ADF minus ADL*, ***ADL*** *being considered a good proxy for the lignin content (Van Soest et al., 1991)*”.

**Answer:** You are right, we rephrased the sentences as you suggest.

**Results:**

L358: Please check if the mentioned negative correlation between the Fungi/Bacteria ratio and MOAC stock was statistically significant? It does not look like that in the Figure 7b annotation, and in that case, you could argue for a tendency while just stating that they are negatively correlated is wrong if it is not statistically significant. Same comment for line 455 where this correlation is mentioned again.

**Answer:**

There were inconsistencies between the confidence intervals and the significance of the relations provided by the linear mixed models that compose the SEM (Figure 6) and the total effects estimation using the "semeff" package (Figure 7). The relationship between the fungi/bacteria ratio and MAOC was significant in the linear mixed model estimation, but not significant when using the "semeff" package. This discrepancy was due to the way the bootEff() function in the "semeff" package estimates variable effects and intervals, using nonparametric bootstrapping by default. We changed the bootstrapping method to "parametric" in this function to ensure more consistency with the linear mixed models estimates (lmer models). We have updated all the graphs according to the new analysis.

L358 and line 359: For both sentences here, you would ease the readability by adding an explanatory sentence in both places, e.g. such as “… *meaning that a higher proportion of fungi over bacteria correlated with higher MAOC stock*” – or what direction the correlation may have. Especially when *ratios* are *negatively* correlated, it becomes a lot of twisting to interpret the direction of the effects, unless you are very familiar with the parameters.

**Answer:** We appreciate the suggestion. We have included that explanation.

**Discussion:**

L417: I recommend writing something like “… *long-term carbon storage in the upper soil layer/topsoil*”, or in other ways mentioned the depth restriction of the conclusions already here.

**Answer:** Done, we have rephrased the sentence as you suggest.

L437-443: This is an important point and paragraph! Good.

**Answer:** Thanks for the positive comment.

L477-478 and 484-486: These are two places where it may be relevant to discuss whether your findings align with Mortensen et al., 2025, mentioned above (doi.org/10.1111/gcb.70117).

**Answer:** We have cited that study on those lines, as we believe it is well-aligned with what we discussed there.

L541-551: In this section (the effects of legume sowing), it is particularly relevant to discuss the shallow sampling depth of 8 cm, and making us aware of the limitations of the conclusions. Sowing new species will most likely also affect carbon and nitrogen dynamics below 8 cm, and thus the total SOC/POC/MAOC stocks of the soil profile. This aspect could also be discussed in a separate paragraph for all management effects, but at for the effect of legume sowing.

**Answer:** As mentioned above, sowed legumes have shallow root systems and main impact would be expected to occur in the topsoil layer. A paragraph discussing the study limitations have been already provided in a previous comment.

L593: Please change from "are" to "were" (saying that your study found this - not common knowledge), or if generalizing this statement to more than your study, at least define the limit of this conclusion to the upper topsoil layer and to semi-arid grazed grasslands. As it stands now it is a very strong generalization, which may not necessarily be the same in other regions, agricultural systems, nor if a deeper soil profile was investigated.

**Answer:** You are right, we have replaced it with 'were'.

**Wording and typos:**

L97: Typo. Change from "MENS" to "MEMS"

L119: Consider writing "... **bio**chemical and morphological traits"

L213: I assume the last word of the sentence should be in past tense: "...measured..."

L302: Change to "These analyses..." (plural)

L322: Check grammar. I would write "Nutrient concentrations in each soil were..."

L330: Insert comma: "SOC, POC and MAOC"

L372: I assume this should be in past tense to align with the rest of the results ("increased") not indicating general knowledge which would be in present tense.

L385: Delete "it", writing just "*As expected, being negatively* ...."

L559: Change to "Our results are in line with..."

L617-621: Please check the use of "" signs. There seem to be lacking some "" around some parts. Also align first names in this section to be either full first names or just the first letter.

**Answer:** Thank you for this thorough review. We have corrected all the errors you have pointed out.

**Figures:**

Fig. 2: In the figure caption, do you mean to write "References embedded in the figure or Fig. 2....?"

**Answer:** We meant to write "References embedded in the figure", we have corrected it.

Fig. 4: There might be missing a word in the end of the first line: "... in the studies ___?___ soils and... ". Or split the sentence into two. It does not make sense how it is currently reading.

**Answer:** You are right, it is a typo, we meant to write "....in the soil samples...."

Fig. 5: Please increase the font size of most elements in the figure. As it is now it's not possible to read all variable names in the PCA's in a 1:1 printout, tricky to read the legends, and very tricky to read the blue/red text in the gradient explanations/axis explanation (in the lower part of the figure). It is a shame since the figure contains a lot of good information. Although it is nice that the text coloring follows the legend/gradient in the PCA's, it also makes it harder to read the text. Maybe the coloring can be kept if the text size increases – or vice versa if text is darker.

**Answer:** We have increased the font size as much as possible while avoiding overlap. Below is the new Figure 5, with some modifications also proposed by reviewer 1.

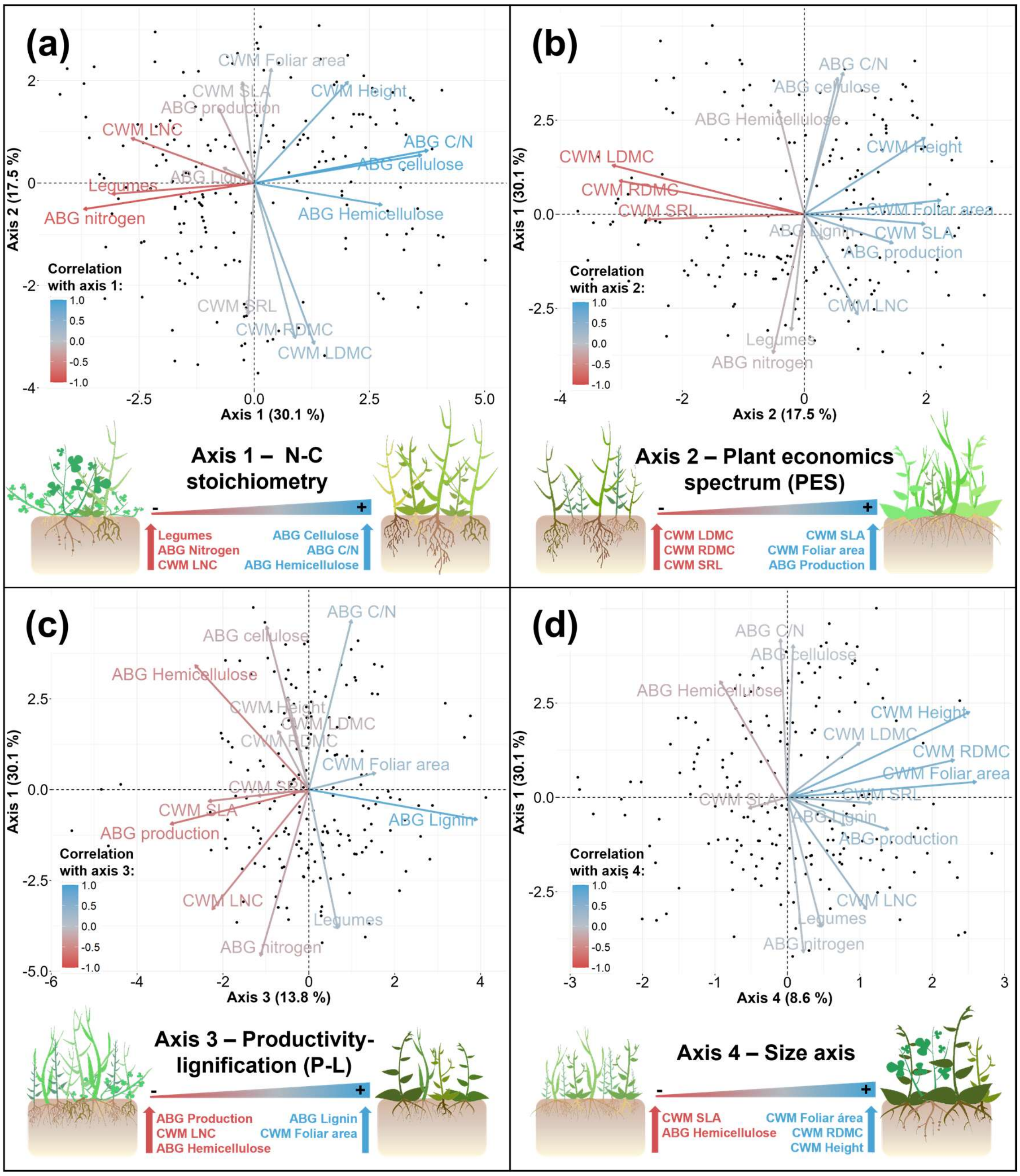


***Figure 5.*** *Representation of the 4 main axis of variation in the principal component analysis (PCA) summarizing the vegetation characteristics variables. Panels a, b, c, and d illustrate the correlation between the different variables included in the PCA and the new axis, with a graphical representation of the characteristics of the plant communities at the end of each axis. Representative species of each axis are shown in Fig.S1.*

Fig. 6: Axes font size within legends is very small, making it impossible to read certain words in a 1:1 size. Also, consider changing "R2" to $R^2$" in the center circle (stock changes). Other than that, it is a key useful figure. In the figure text, you can delete one "s", thus writing "…*by factor type…*".

**Answer:** We have made the requested corrections and increased the font size as much as possible (Figure 6). However, due to the amount of information included in the figure, the font size is still relatively small. We hope that this will be acceptable to the reviewer.

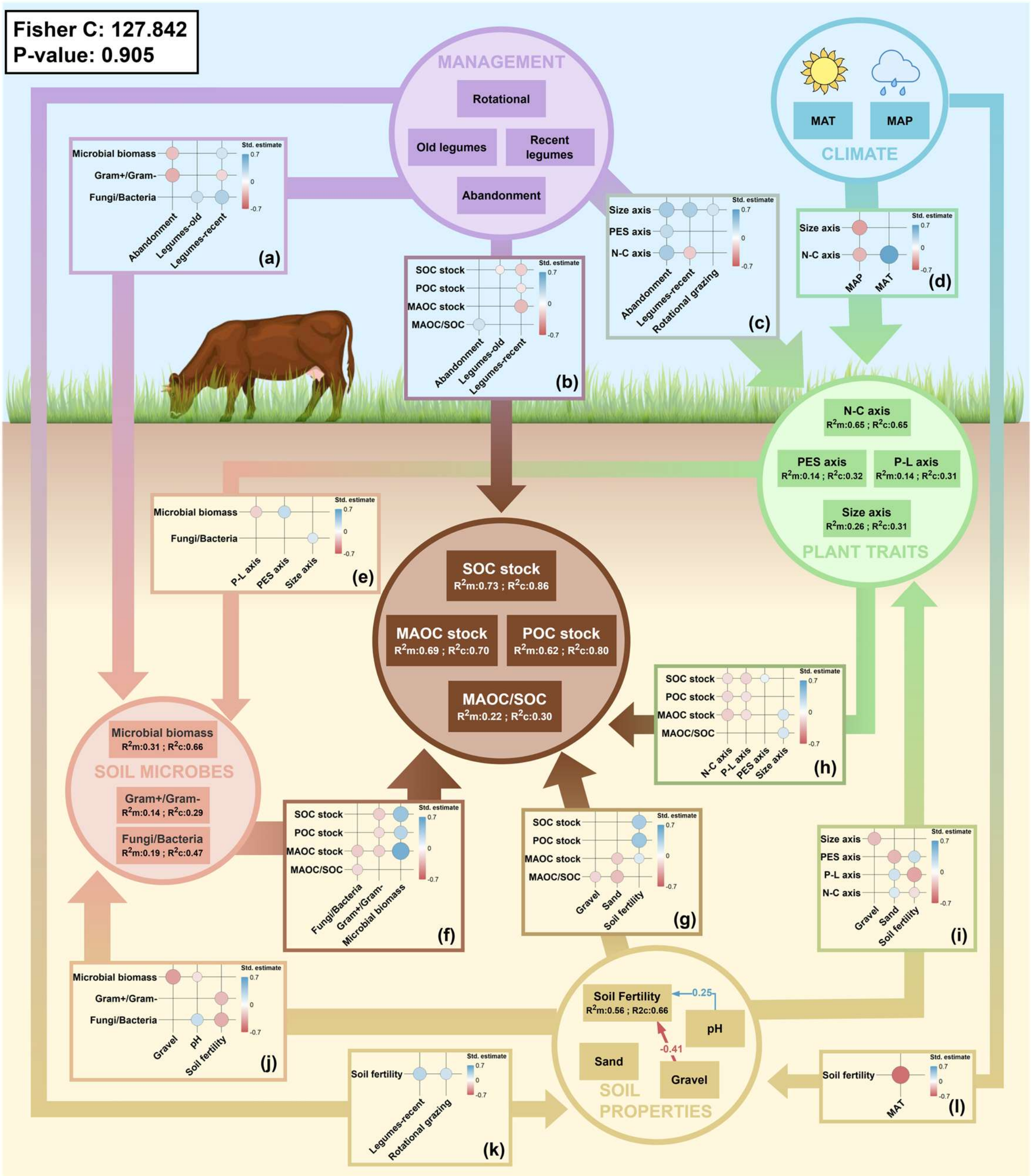


*Figure 6*. *Structural equation model representation. Factors included in the model are grouped by factor type (management, climate, vegetation traits, soil properties, microbial communities). Arrows between groups of factors indicate significant relationships between any of the variables included in both groups. The width of these arrows is proportional to the mean absolute size of the estimates between the factors in the groups. The plots embedded into these arrows show the standardized estimates of the significant relationships between the factors connected by the arrow. Negative standardized estimates are represented in red, and positive ones in blue. The size of the estimate circles represents the absolute value of the standardized estimate. Causal relationships between factors in the same group are represented by individual arrows.*

**Supplementary:**

Table 1: Please include management acronyms in this table, as done in figure S3

**Answer:** Suggestion followed, we have included it.

Figure S1: As for figure 5, please increase font size. Font color could be OK if size increases.

**Answer:** Suggestion followed, we have increased font size.

Figure S3: Increasing font size of labels would make it possible to read them.

**Answer:** Suggestion followed, we have increased font size of labels.

Figure S5: Please provide the unit for the content in the figure caption - or if it is centered and scaled / indexed, mention this in the figure caption as well.

**Answer:** As indicated in the caption, the figure shows standardized effects, which are unitless.

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