Reviewer 1

Authors present an interesting fertilization study from a subtropical forest in SE China. Authors make an interesting hypothesis that P addition would release SOC bound to Fe in the soil, making it available for microbial attack, and explaining higher CO_2 emissions under P addition. This hypothesis is presented as an alternative explanation to existing knowledge paradigms. The approach to test the hypothesis included sampling soils at various time points after P addition (2 weeks to 1 year) and measuring soil P pools with Hedley as well as several biological soil properties. The study is potentially very interesting and valuable for the soil science community, but the manuscript needs to be improved significantly.

General Comments

Comment

The quality of the presentation is low and needs to be improved. Most of the figures are incomplete in terms of axes labels or captions, creating a sloppy impression.

Response

We have carefully revised the figures accordingly. Clarity on the presentation of the figures and adequate captions have been provided in the footnotes. The inset bar graphs across Figures 1-3 share the same vertical axis title as the main line graph; hence, the absence of the vertical axis labels. We have captured this clarification in the figure footnotes. Part of it reads as follows (applicable to all relevant figures):

….."The inset orange bar charts indicate the annual mean values of the treatment response (n=24, $p < 0.05$) shown on the main line graphs. Each inset bar plot shares the same vertical axis label as the main line graphs. Each point on the line graph represents the mean value of each treatment (n=3, $p < 0.05$)".

Comment

The same goes for the statistical analyses, which is not complete / always appropriate. Authors need to perform and report rigid model diagnostics for linear regression and structural equation models.

Response

We have taken appropriate steps to perform model diagnostics for the models used where applicable and explained in detail as shown in the respective specific comments below.

Comment

A suggestion in terms of framing, I recommend authors to use the concept of "P and SOC competition for binding sites", which I think is a more useful concept to test and explain the observed phenomena.

Response

We have taken adequate steps to improve the framing of our concept based on your suggestions across the entire manuscript. P and SOC competition for binding sites have been used where applicable across the manuscript.

Main comments

From the abstract its not clear how the new hypothesis differs from the established knowledge because both in effect have to do with rendering SOC available for microbial decomposition. Please distinguish old and new hypotheses more clearly.

Response

We have clarified our alternative mechanism to indicate that: aside from relieving microbial P limitation, P supply induces microbial C limitation due to the enormous C needed for incorporating MBP, and the desorbed SOC satisfies microbial C need without further SOC degradation. This is mechanistically different from the currently known mechanism indicating that P addition relieves microbial P limitation to drive SOC decomposition.

Comment

Authors need to interpret the findings in a more nuanced way, given the limitations of the methods used. For example, in the abstract I read that Fe-bound P was quantified, which made me assume XANES or similar was used—a method that can distinguish Fe-bound P from other P forms. However, from the methods it becomes clear that authors used a sequential extraction. Sequential extraction contains many assumptions and limitations and results cannot be attributed to a specific P form. Authors need to rewrite the manuscript to account for the nuances of the limitations of the approach used.

Response

We quite agree with you that NaOH-extracted P is often associated with Fe and Al (hydr)oxides. It is true that the use of XANES or DCB (Dithionite-Citrate-Bicarbonate) extraction would distinguish more clearly, the Fe-bound P from other P-bound forms. We have acknowledged this limitation in our manuscript under the limitation section that was added. However, because Al-oxide concentration was very low in our soils, we based our assumption on the fact that if Fe oxides dominate over Al oxides in soils, NaOH-P will mostly reflect Fe-bound P.

Comment

The section on DOC dynamics needs to be rewritten. Currently the text is not supported by the data as presented in the figures. The text claims that DOC was increased but the data does not show it, or at least it presents a much more nuanced picture.

Response

The mismatch in the presentation on DOC and the data presented in the figure has been addressed. We have ensured to rewrite and clarify our presentation across the manuscript to avoid inaccuracies.

Comment

The linear and structural equation models used are not capturing a lot of the variability, as can be seen in the low R2s. Authors should discuss what unaccounted for variables or what methodological limitations might explain the variability.

Response

Thank you for this important observation. We acknowledge the low R^2 in the linear regression and SEM. The linear mixed effect model has now been used in place of the linear regression model earlier used. With the linear mixed effect model, we incorporated the sampling date as the random factor. This resulted in a more robust prediction of the response of the dependent variables evaluated ($R^2 > 0.4$ across all the relationships).

For the SEM, the model defined in "*model.kerch*" in the analysis assumes a linear relationship among variables and that observations are independent of each other. However, the model accounts for any measurement errors in the observed variables, which is a key advantage of SEM over traditional regression. Perhaps, the effect of multiple ecosystem factors on the relationship between variables may have contributed to the lower R^2 values obtained.

Comment

I do not have expertise of DNA sequencing approaches so cannot evaluate the quality of that work.

Response

We acknowledge your in-depth and constructive revision that has allowed us to reevaluate some aspects of our presentation and ensure their improvement. We believe your comments have undoubtedly provided an avenue to strengthen the major conclusions of the study.

Minor comments

l. 35 introduce abbreviation MBC

Response

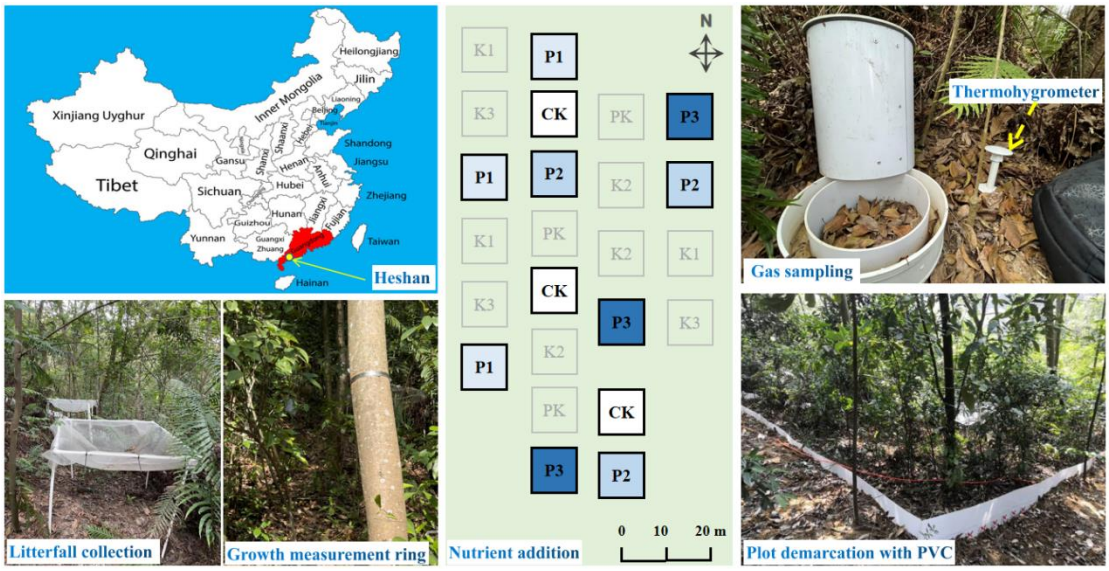
Line 37: MBC has been written in full (microbial biomass carbon) at first mention

Comment

The map in Figure S1 should be improved because it is not clear where in Guangdong the experimental site is. Also not clear what the different fill colors of the provinces are supposed to represent.

Response

We acknowledge the non-clarity of the map in Fig. S1. We have replaced the map with a better version to provide clarity on the location of Guangdong and the experimental site as shown below. The fill colors in the different provinces on the initial map used did not indicate any information relevant to the current study and have been replaced with a better map, accordingly.



Comment

l. *134 what is the World Reference Base equivalent of the soil type?*

Response

The world Reference Base equivalent of the soil type (Acrisol) has been provided as requested.

Comment

l. 158 change "samples collection" to "sample collection"

Response

"samples collection" has been replaced with "sample collection" as suggested.

Comment

l. 1*88 please cite the original reference for this method as well. From the description here it is not clear how the method used (from Hou et al. 2018) differs from the standard Hedley extraction. Please clarify.*

Response

The original method, which was Hedley (1982), has been incorporated into our citation and reference section.

We have indicated where the modified method by Hou et al. (2018) varies from the original methods and its suitability for the current soil type of the study site, viz: Unlike the Hedley fractionation method that utilizes $NaHCO_3$ for sequential extraction of P, we utilized NH_4F which is more effective for extracting readily available P fractions in acidic soils.

Comment

l. 192 what justifies assuming that NaOHPi equals Fe-bound P? This is a very shaky assumption in my opinion. It could be fully Al-bound P or a mix of many different P forms…

Response

We have acknowledged this limitation in our manuscript under the limitation section that was added. However, because Al-oxide concentration was very low in our soils, we based our assumption on the fact that if Fe oxides dominate over Al oxides in soils, NaOH-P will mostly reflect Fe-bound P.

Comment

l. 204 Please mention if a spike was used to correct for sorption. If not, please discuss implications on results.

Response

Because the soils of the study site evaluated are classified as weathered and tend to adsorb P more strongly, we utilized NaH_2PO_4 as a spike during the determination of MBP and measured its recovery to adjust for P sorption. This statement has been incorporated into the main text.

Comment

l. 251 change "supply of increasing P" to "P fertilization" or P input or P addition

Response

As requested, we have replaced the "supply of increasing P" with "P fertilization" to enhance readability.

Comment

Fig. 1 what does the small inset plot with the orange bars show? Axes labels missing I in inset plots. Also, the panel titles (a, b, c, d) are on top of axes labels.

Response

Figures 1,2,3: We acknowledge our error in not providing an adequate explanation for the inset plots in Figs 1, 2, and 3. The inset plots and the main line graph share the same axis labels. This has been duly clarified in the figure footnotes. An examples is given as follows, which was applied to all relevant figures:

….."The inset orange bar charts indicate the annual mean values of the treatment response (n=24, $p < 0.05$) shown on the main line graphs. Each inset bar plot shares the same vertical axis label as the main line graphs. Each point on the line graph represents the mean value of each treatment (n=3, $p < 0.05$)".

Figure 1: The panel titles (a, b, c, d) have been moved to the top right in the panel box as obtained in other similar figures.

Comment

Fig. 2 and 3. Same problem. What is the inset showing?

Response

As stated above, we have clarified what the inset represents in the figure captions.

Comment

l. 268 this does not seem to be supported by the results.

Response

We regret the mix-up in the result presentation. To ensure correctness, the section has been carefully revised and it was ensured that the results are in agreement with the presentation in the figure.

Comment

l. 272-273 this makes no sense. Please be more nuanced

Response

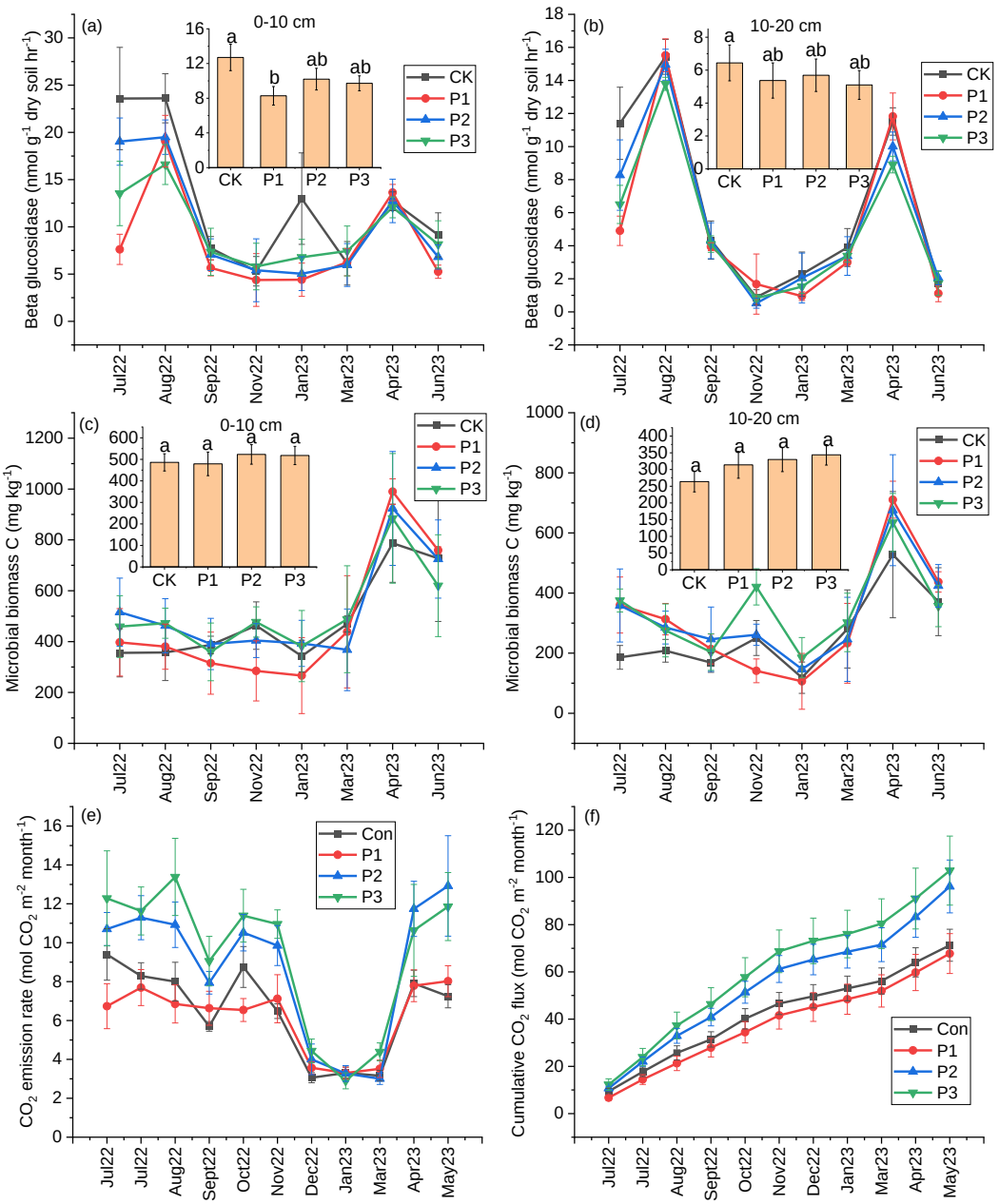
The sentence has been carefully revised to ensure clarity.

Comment

Fig. 1-3 if using post hoc testing to determine differences between treatments please indicate significant differences with letters on top of the error bars (bar plots)

Response

Figures 1-3: As suggested, we have presented the details of the posthoc test result using letters on the bar plots. An example as done for Figure 3 is shown below



Comment

Fig. 4 linear regression modeling as done here is problematic because the observations are not independent, violating assumptions of linear model. To account for the site and treatment effects, linear mixed models should be used with time point and treatment as random effects.

Response

You are right. We have reevaluated our observations and subjected our data to linear mixed-effect models to provide more robust relationships among our variables. We used the

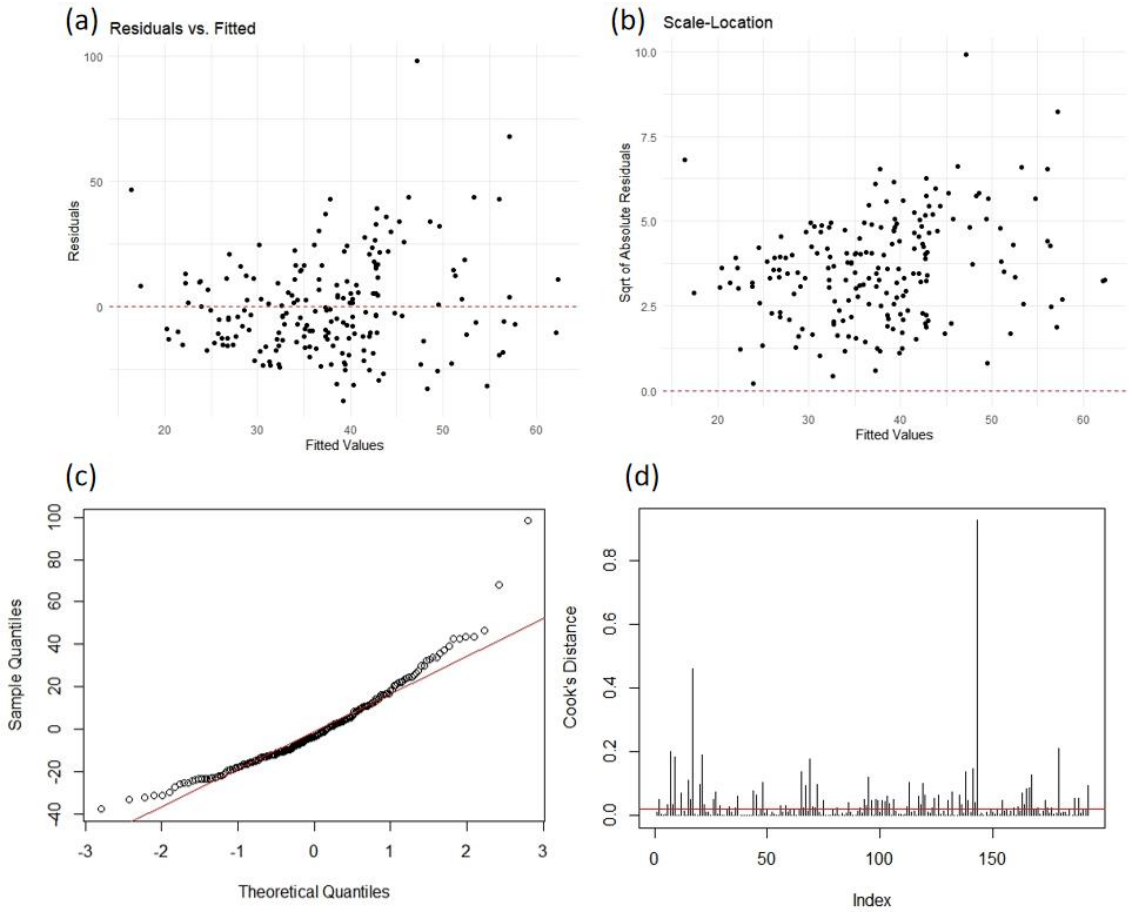
sampling date as the random factor and obtained more robust R^2 values (R^2>0.4) compared to the low values previously obtained using linear regression models.

Comment

Linear regression modeling. How was model diagnostics done?

Response

We have replaced the linear regression model initially used with a linear mixed-effect model. To carry out a diagnostic of the linear mixed-effect model currently utilized, we compared our sample quantiles against the theoretical quantiles of a normal distribution and obtained a linear relationship, indicating that our data was approximately normally distributed. We also conducted residuals analysis (to check for random scatter around zero), homoscedasticity check (constant variance), and influence diagnostics (using Cook's distance to identify influential observations). Overall, the linear mixed-effect model proved robust for our analysis compared to the linear regression model initially utilized. We have carried out the model diagonostic across all the relationships and provided a sample model diagnostics for the relationship between DOC and Fe-bound P in Figure S8 as shown below.



Comment

Fig. 5 what about the blue lines?

Response

Fig. 5: The blue lines indicate non-significant relationships and have been incorporated in the footnote of the figure.

Comment

l. 419-428 why only focus on Fe? What justifies the assumption that Fe-sorption is driving P and C dynamics, as opposed to sorption to Al oxides?

Response

We have acknowledged the limitation that part of the sorption may include some Al-induced, and in our manuscript under the limitation section that was added. However,

because Al-oxide concentration was very low in our soils, we based our assumption on the fact that if Fe oxides dominate over Al oxides in soils, NaOH-P will mostly reflect Fe-bound P.

Comment

l. 431 see alsoP and Corg competition for sorption sites in Regelink et al. 2015 (European Journal Soil Science)

Response

Ragelink et al.'s (2015; https://doi.org/10.1111/ejss.12285) study supports our hypothesis of competitive sorption between phosphate and organic C for ions in soils and has been incorporated into our discussions. While we prove that P supply increased the desorption of organic C, they show that the solubility of PO_4 increased with increasing SOC content, indicating a higher competitive capacity of SOC for sorption to binding sites relative to P. These differences could be attributed to differences in P rates utilized, soil types, their chemical/physical properties, and other climatic conditions that can shift the SOC-Fe-P interactions.

Comment

Fig.7. Also here, why can you assume its all driven by Fe bound and not a mix of Fe and Al (hydr)oxides?

Response

As stated earlier, because Al-oxide concentration was very low in our soils, we based our assumption on the fact that if Fe oxides dominate over Al oxides in soils, NaOH-P will mostly reflect Fe-bound P.

Comment

l. 614 the statement needs to be made more precise. All soils are capable of binding anions to some degree

Response

We have clarified the statement to be specific to soils rich in Fe with a high capacity to bind organic carbon.