

Second report egosphere-2025-3722

Summary of changes

I would like to thank the authors for their detailed responses and the revisions made to the manuscript. They have addressed most of my previous concerns, particularly by improving the consistency of terminology, clarifying the methodology (remote sensing and statistical analyses), and strengthening the discussion. I especially appreciate the additional figures, which are very helpful for visualizing such a large dataset.

While these edits have greatly improved the overall clarity of the study, I have included a few final minor comments, mainly regarding the classification of degradation status based on seasonal parameters. At this stage, it is also important to ensure that the data and code are shared in an open and FAIR repository.

To facilitate the review, I have provided all my follow-up comments in green. My responses are placed directly under the authors' previous answers to ensure the continuity of the discussion is easy to follow. The text has been copied from <https://doi.org/10.5194/egosphere-2025-3722-AC2>.

Reviewer 2

Many to the reviewer for the helpful comments

This study investigates the use of satellite-derived vegetation indices to identify soil degradation in Eastern Kenyan grasslands. The authors classify fields into three categories (equilibrium, transition, and degraded) and validate these classifications against a large set of laboratory soil analyses of topsoil samples collected from a subset of the sites. Due to limited initial findings, a PCA and k-means clustering approach was applied to all the soil data, identifying two primary clusters which represent degraded and non-degraded soils. While the development of rapid, remote-sensing-based degradation assessments is of great practical importance, there are significant concerns that need to be addressed. The major points are outlined below under "General comments" with more details provided in the "Detailed comments".

General comment

While the authors' objectives and overall workflow are clear, the manuscript suffers from a lack of general clarity and technical detail.

There is an inconsistency in the terminology used throughout the paper, particularly regarding the degradation and soil property classifications (see detailed comments). Furthermore, while the authors provide an extensive list of laboratory-measured soil properties, reasons for their selection and the analysis remain unclear and confusing for the reader.

Thanks for this comment. We have added justification for the inclusion of the soil parameters into the text of the 'soil sampling and analysis' section

Clear now, thank you.

The description of the remote sensing methodology is incomplete. Vital information is missing regarding the selection (e.g. specific sensor details, image selection criteria, total number of images used, reasons for the temporal coverage) and the pre-processing (e.g.

cloud masking is likely crucial in the humid tropics) of satellite imagery.

Following the comments of referee 1 we have now significantly improved the section on the remote sensing approach.

The revised wording helps understand the remote sensing methodology. It would be helpful to know which R / RStudio versions were used and to cite the R package.

Statistical analyses also require further explanations and details: It is unclear on what data the tests were performed (e.g. was there data splitting by site? What were explaining and response variables?) and whether model assumptions were verified before performing ANOVA. Additionally, the authors utilized pairwise t-tests to compare three levels of a factor (degraded, transitional, and equilibrium); a post-hoc test such as Tukey's HSD could be more statistically robust and appropriate?

We thank the reviewer for the helpful comment! We agree that the section would benefit from further details on the testing procedure, for which we have revised the paragraphs in sections "Statistical analysis of field data". We also agree that some improvement is needed for the testing procedure. Therefore, we made the following changes.

The analyses in this study were done on the data from Kuresoi and Nyando respectively. Initially, we used ANOVA to test the difference between degradation classes for all variables. We have now replaced the ANOVA with a non-parametric test (Kruskal-Wallis) when the samples failed the normality test (Shapiro-Wilks). This only applies to a few variables, but it does result in two more variables (H₂ONO₃, Inorganic_P) being significant for Nyando. To investigate the details of the differences, we initially used the two-sample t-tests. This is because we considered the follow-up comparison between two classes to be no longer a multiple comparison problem, as the inflation in Type-I error caused by multiple comparisons has already been taken care of by ANOVA or its non-parametric equivalent. However, we agree that the Tukey's HSD test and its non-parametric equivalent would be a better choice for reporting the result in Table 2, due to the consistency between them and the first stage of the analyses. Therefore, we have updated the letter-based display with the results from Tukey's HSD test or pairwise Wilcoxon test for the non-parametric case.

Thank you for the explanation and the revisions. It makes a lot more sense now.

The results section generally lacks structure. Several results mentioned in the text are not supported by data in the manuscript or the supplementary material (see detailed comments below). Some visualizations would help the reader to better understand the results (e.g. boxplot and score plot; see below).

We address these comments below

The discussion section requires major rewriting. It is currently a mix of introduction and a repetition of the results, also often drifting into unrelated topics. Furthermore, the discussion does not cite enough literature (in my opinion, 10 references in the entire discussion section is not enough to discuss the results obtained (there are numerous high-quality articles published in the field)).

Ultimately, the study demonstrates that while laboratory analysis successfully identifies soil degradation status, the proposed remote sensing method does not. Given that the efficacy of laboratory analysis to identify soil degradation is already well-established, the value of this paper lies in a deeper exploration of why the remote sensing approach underperformed. The authors should focus their discussion on comparing their remote sensing results with existing studies to identify specific challenges or limitations.

Detailed comments

L24: Not all soils across Sub-Saharan Africa are degraded! Please rephrase.

We don't actually say this, but we have rephrased the opening line to add clarity.

Now reads as 'Soils across sub-Saharan Africa are exposed to extensive degradation processes,

which can reduce their ability to produce crops and support livestock. ‘
Thank you for the clarification.

L30: What is equilibrium, transition, and degraded soil? The reader has absolutely no idea what is meant. Please introduce the terms.

We have clarified this in the abstract as follows:

‘classify grasslands as productive grazing lands (equilibrium), unstable and unproductive (degraded) and grazing lands that followed a variable trend in vegetation productivity (transition),’

This makes a lot more sense now.

L40: What are stable and transient soil variables? Please explain.

We have clarified this in the abstract:

‘into stable (those that are slow to change) and transient (those that change rapidly in response to a changing pedological environment) soil variables’

The rephrasing makes it easier to understand this description.

L125-158: It would be more useful to have a description of the sampled sites instead of the hydrological catchments / basins. Also, since topography is a major factor influencing soil degradation this would be crucial to add.

Given the number of sample sites it is impractical to provide a description of each site.

Figure 1 illustrates the topography using a 30 m DEM. We have added elevation ranges and mean slopes derived from the DEM to the field area description (L125-158).

It would somewhat be interesting to know the range of slopes. But otherwise, I appreciate the edits.

L156-158: The degradation classification should be properly introduced, and the terms should be kept consistent throughout the manuscript. It is not clear to the reader what equilibrium, transition, and degraded soil really means (see above).

We agree that there is a lack of consistency in the manuscript and that the terms could have been better introduced. We have now reworked a paragraph (below) to introduce the terms we have decided to use and have checked the document for consistency. We believe our choice of productive, transition and degraded as the classes will have more resonance with SOIL’s readership.

‘Approach to degradation classification

Our approach to defining degradation states following the concepts developed in the ecological literature (Briske et al., 2003). Productive grasslands are those where biomass productivity is and returns rapidly following dry seasons. On the other hand degraded grasslands are those with lower peak biomass and only slowly recover following drought. Those grasslands which exhibit characteristics of both productive and degraded grasslands and termed transitional. These states are defined using an analytical approach using remote sensing images of both study sites which is set out in the following sections This spatio-temporal analysis covered a period of 5 years (2013 – 2018).’

Great, thanks

L198-204: It is not relevant which parameters were calculated if they were not used within this study.

We are not sure what the reviewer is getting at here. The parameters are used in table 1. Examples of model parameters used include maximum index values (MV), and End of Season Values (EoS).

«... the following seasonal parameters were estimated: Seasonal Amplitude (Amp), Start of Season (StoS), End of Season (EoS), Function value at Start of Season (SoSv), Function value at End of

Season (EoSv), Season Length (Len), Base level, Mid of the Season (Mid), Largest data, Maximum Value (MV), Left Derivative/greening rates (LD/GR, and Right Derivative/browning rates (RD/BR), Large Seasonal Integral and Small Seasonal Integral. For definitions of seasonal parameters and further explanations see (Eklundh and Jönsson, 2017).”

- a) Table 1 does not show all these parameters (e.g. Amp, StoSt, EoS, Len, ..)
- b) It is still not clear why these additional parameters were estimated (or how they were used in the models)

L171-225 and Table 1: It is not clear to the reader what kind of models were trained. It is also unclear why models were compared or what their underlying parameters are. Moreover, I recommend moving any data or outcomes presented in this section to the results (if it is relevant).

The difficulty with moving these results to the results section is that we subsequently use the model to decide where we are going to sample, a procedure that we need to describe in the methods. We therefore propose to leave this section where it is as it will be easier for the reader. However, if the Editor is not happy with this we will move it.

I understand the authors' rationale for keeping this table in the Methods section. However, the first part of my previous comment remains unaddressed: the specific technical details regarding the models presented in the table are still missing. While I understand that thresholds of seasonal parameters were used for the classification of degradation status, the functional differences between Models 1 through 5 remain unclear.

Please provide these specifications to ensure the methods are fully reproducible. If these models are based on specific functions/code in R, please provide further details here or provide a direct reference to the code repository.

L253: I only see five seasonal parameters (SoSv, EoSv, MV, GR, and BR)

L235-236: This visual inspection seems arbitrary.

We agree the visual inspection might seem arbitrary. However, we assessed the benefits of including several model outputs from the time-series analysis in the sampling decision process and determined that models without landscape consistency (e.g., classifying large forest-patches as productive/equilibrium) did not provide additional insights for the landscape classifications before field sampling.

This makes sense, thank you for the clarification.

L241-243 and L246: What is “the status of the locations” and “land use history check”?

Please add more details on this. The long-term history of the grassland is crucial to understanding soil degradation.

The sentence with ‘status of locations’ now reworded to ‘and use history check’ refers to the removal of land that had been previously tilled (see above). We have now edited this section to make it clearer for the reader.

‘Locations that coincided with recently cultivated areas (<10 years) and/or road tracks were removed following examination using Google Earth (2008 - 2018). Locations that had signs of recent cultivation or tillage lines were excluded. In October-November 2019, the locations were visited to remove sample locations that were inaccessible or when landowners denied access. In total, 90 sites were selected for study (Figure 2).’

Thank you for adding this information. This adds much more clarity to the description.

L255–310: The soil sample analyses section is currently disorganized and lacks critical information. Sample processing (e.g. drying, sieving, grinding) and citations for laboratory methods are missing. To improve clarity, the authors should remove any variables that were

not further investigated within this study. I recommend presenting the measured soil properties in a table, for example categorized by their subsequent classification (stable/transient).

We are a little confused by this comment. Our soil methods section follows the standard approach for methods in soil science papers, see for example the recent paper in SOIL (<https://soil.copernicus.org/articles/12/55/2026/>). Almost all the methods are referenced, if not then the machine used is referenced and more detail given. The paragraph 'Description of data' clearly identifies the stable or transient variables and the reasons why they were considered transient or variable. We have also added this information to Table 2.

We can put all this information in a table if the Editor requires it, but it would be unusual to do so.

I believe there may have been a misunderstanding regarding my previous comment. It remains unclear to the reader how the soil samples were physically prepared prior to certain analyses. Specifically, please state whether the soil was oven-dried (including the temperature), sieved (specifying the mesh size), or ground. While the methods for the wet chemistry analyses are quite elaborate, this fundamental information on initial sample processing is missing and is essential for reproducibility.

I appreciate the additional information and table provided regarding the stable and transient variables. This inclusion makes the manuscript much easier to follow. Thank you for these additions.

L312: Which soil variables?

Not clear what the reviewer means here. We have added 'soil' in front of variables to make it clear that we are talking about soil.

Clear now, thank you.

L314: Why and how were variables classified and what does transient and stable mean?

The information is missing completely. Literature is needed to justify such a classification too.

We disagree that this information is missing completely since there is an entire section devoted to it in the paper. However, we have now re-written this to add clarity and introduced some new references.

'For testing and clustering analysis, we focused on a total of 28 soil variables measured from the soil samples collected from the 0-0.1m depth in Kuresoi and Nyando, respectively. These variables were grouped in relation to their rate of change in response to degradation as either stable (changes over multi-year time periods) or transient (changes over seasonal time periods) soil variables

Bulk density and soil hydraulic properties change over multi-annual timescales (Berisso et al., 2012), as can contents of C (Tully et al., 2015), N (Sun and Chen, 2025) and P along with pH (Tully et al., 2015), and thus were considered stable. Other soil physical variables (sand percentage, silt percentage, and clay percentage; and aggregate stability) were also considered stable. Although there is little literature evidence to support this, we reason that textural changes will be in response to weathering or prolonged soil erosion, both processes which take place over multi-year periods and that aggregate stability is strongly related to soil texture and organic matter (Kemper and Koch, 1966).

In contrast, soil biological parameters, including enzyme activities, microbial biomass, and rates of nutrient mineralisation, respond rapidly to change in response to seasonal changes environmental conditions (Cordero et al., 2023) and therefore soil enzymes (PHO, GLC, NAG, XYL, CBH, PER, POX, URE), water extractable NO₃, and KCl-extracted NH₄, microbial C, 'microbial N, total dissolved C, organic dissolved C, mineralisation and nitrification were considered transient.

Kemper, William Doral, and Elbry James Koch. 1966. *Aggregate stability of soils from Western*

United States and Canada: Measurement procedure, correlations with soil constituents (Agricultural Research Service, US Department of Agriculture).

Sun, S. , and S. S. Chen . 2025. “ Extensive Decline of Soil Nitrogen and Its Drivers in the Lake Victoria Basin of Tropical Africa (1996–2015).” *Land Degradation & Development* 36, no. 17: 5911–5926. <https://doi.org/10.1002/ldr.70045>.

Skinner, R.J. and Todd, A.D. (1998), Twenty-five years of monitoring pH and nutrient status of soils in England and Wales. *Soil Use and Management*, 14: 162-169. <https://doi.org/10.1111/j.1475-2743.1998.tb00144.x>

Tully K, Sullivan C, Weil R, Sanchez P. The State of Soil Degradation in Sub-Saharan Africa: Baselines, Trajectories, and Solutions. *Sustainability*. 2015; 7(6):6523-6552. <https://doi.org/10.3390/su7066523>

The revised paragraph is much clearer now, thank you!

L325: Why are there missing observations? Specify. Number of samples should be added to the overview table (see comment above).

The number of samples has been added to the overview table as requested.

The reasons are multifaceted. Much of the work was carried out during the covid pandemic resulting in problems with access to laboratories, samples becoming expired and in some cases lost or damaged in transit. We do not think the reader needs to know reasons , rather what is important is how this situation was dealt with which is explained.

I agree that the underlying reasons are not relevant to the reader, and I acknowledge the helpful edits made to the table.

L330-337 and Table 2: It is not clear to the reader how the models were trained (which data, which explaining and response variables). Also, have the model assumptions been met? The dataset is unbalanced, has this been considered? Why was a t-test applied to a factor with three levels (degradation status). A Tukey’s HSD test might be more appropriate in this case..?

For the analyses of the field sampling data with the degradation classes, we first split the data by areas, Kuresoi and Nyando. Then, for each area, ANOVA was applied to each variable using the degradation class labels as the explanatory variable. We have added some explanations to the main manuscript. We have also updated our testing procedure by including the non-parametric tests (Kruskal-Wallis and Wilcoxon) and the addition of Tukey’s HSD test. We have provided a detailed response to this in the general comments above.

Here we would like to provide some more discussion on the use of non-parametric tests as opposed to ANOVA when some of the model assumptions (e.g., normality) are violated, and explain why we did not replace ANOVA for non-parametric Kruskal-Wallis tests on all variables. In our case, running the Shapiro-Wilks test suggests that the majority of the groups pass the normality test (21 out of 28 for both Kuresoi and Nyando). Similarly, the majority of the soil variables have similar variances across groups, but some show larger differences. Overall, there are some violations to the classic assumptions of ANOVA in some soil variables, but the majority are still suitable for a classic ANOVA. Non-parametric tests, such as the Kruskal-Wallis test, do have advantage in situations where the normality assumption is not met, but a loss of information when we convert the observed values into ranks is inevitable. In addition, there are different opinions with respect to how robust

ANOVA is when there is violation to the assumptions, for example, Lantz (2013) and Blanca et al (2017). So far there hasn't been a conclusive answer to this question (as many of the analyses on the robustness of ANOVA were based on simulation studies, not theoretical proof). Therefore, the new approach we take here is to apply ANOVA to variables that passed the normality test, and to apply Kruskal-Wallis test only on those who do not pass the normality test.

Lantz, B., (2013) The impact of sample non-normality on ANOVA and alternative methods. *British Journal of Mathematical and Statistical Psychology*, 66, 224–244.

Blanca, M., Alarcón, R., Arnau, J., Bono, R., Bendayan, R., (2017) Non-normal data: Is ANOVA still a valid option? *Psicothema*, Vol. 29, No. 4, 552-557

Clear now, thank you.

L337: What is the “content of the mean differences”?

This refers to how the degradation classes differ for each variable. This essentially means pairwise group comparison. We have revised text in the main manuscript for clarification.

Thank you for the clarification.

L343-344 and L349-360: Why was the Gaussian mixture model compared to the k-means clustering? What does an application of a model “for reference” mean? This is again very arbitrary, and the entire paragraph is irrelevant and should be removed. L355-360 sounds like a justification for why k-means clustering was selected in the end. In my opinion this is not relevant.

As this clustering analysis is exploratory, that is, we do not know the truth, nor do we take the ERU labels as the “truth”, we applied different clustering methods to the data sets, to see how their results differ and whether one result makes more sense than the other (in terms of separating the sites). We agree that this information needs not to be presented in the main content. We now have removed the related content.

Thank you. This part is much easier to read now.

L384-386: It is not clear to the reader how the t-test was used to determine the separation of populations.

The two-sample t-test was carried initially to obtain additional information on the mean difference of different soil variables between the two clusters. It is used when we determine the better clustering method in this case. As the comparison of clustering methods has been removed from the main manuscript, we removed this part as well.

Clear now, thank you.

L388-391: It is not necessary to describe the used R functions. The R packages and their versions should be sufficient.

We have now removed the details of R functions.

To improve reproducibility, it would be very helpful to have information on the R package versions.

L 394-395: It would be helpful to the reader to visualize the results of the variables which differ significantly between the degradation classes in a graph (for example in a boxplot).

Thanks for the suggestion! We have added the boxplots overlaid with observations to the manuscript. See below

I would like to thank the authors for providing these graphs; they are very helpful in illustrating the data. To further improve clarity, I suggest the following minor adjustments:

- ensure the labels for soil properties in the figures match the specific terms used in the text

- it would be helpful to the reader if the different study sites were clearly distinguished (e.g. different colors)

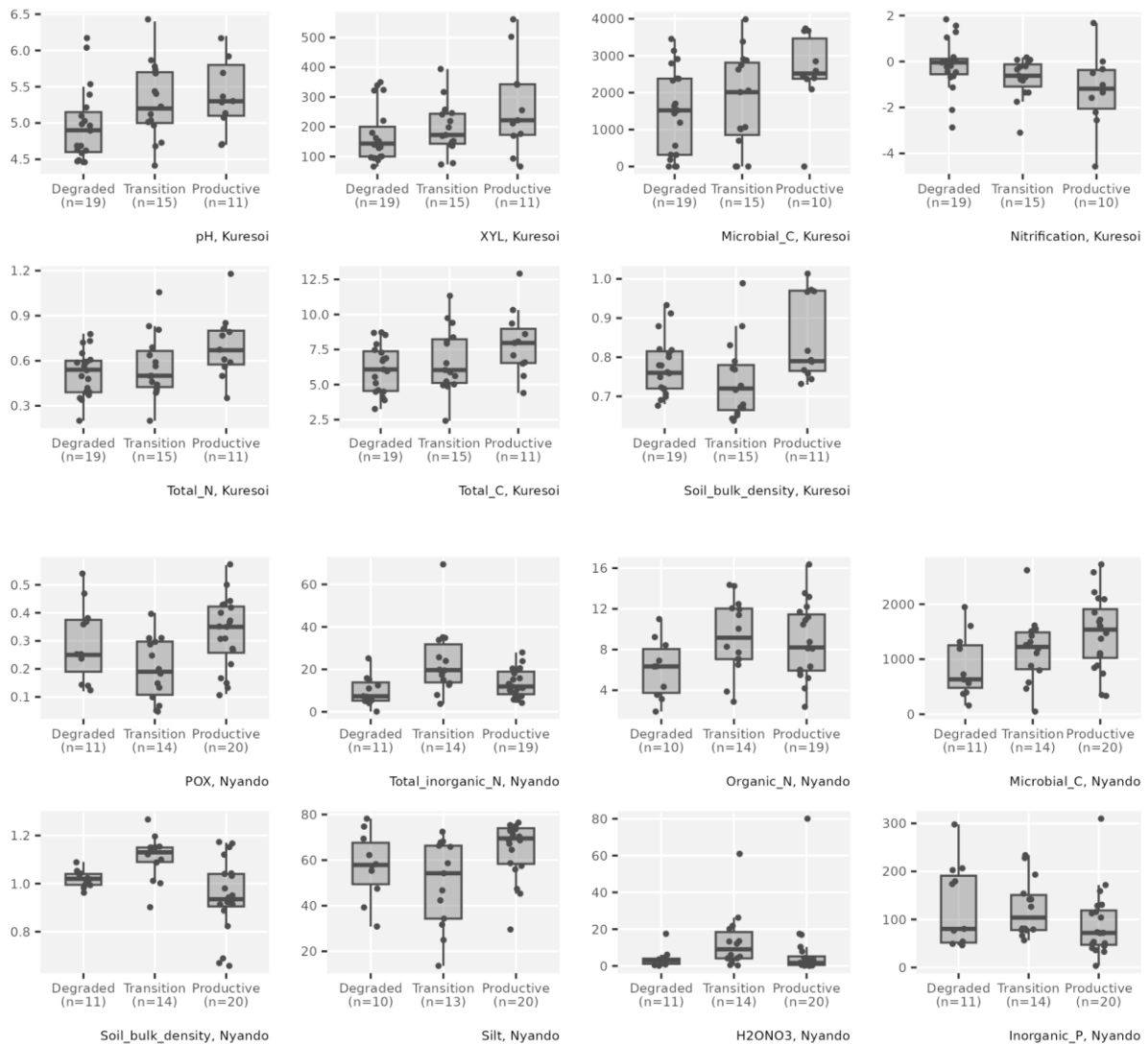


Figure **: Boxplots of the soil variables with significant difference between degradation classes based on ANOVA or Kruskal-Wallis test from Kuresoi and Nyando respectively. The dark grey dots are the observations overlaid on top of the boxplots.

Please correct line numbers (they should be continuous but randomly restart here).

L10-30: Numerous soil properties which are reported in this section are not presented in the manuscript (for example inorganic N, organic N, pH, clay, bulk density). When you report results, always refer to the table or figure where the reader can find these results (this is completely missing with one exception of the density plots). Moreover, it would be nice to have a graph showing the results of the principal component analysis and the k-means clustering.

Thanks for the advice! We have strengthened the link between the tables, figures and the text.

Thanks!

For the result of k-means clustering, we would like to point to Figure 3 and 4 and Table 3 for

the presentation of the clustering results as density curves and in numbers. The reason we only presented the density plots of a few selected variables is because, although all stable or transient soil variables were considered in the analysis, not all of them are well separated by the K-means method. Hence we only presented the result of five selected variables that are well separated by the clustering algorithm. As for the principal component analysis, it is used for the purpose of dimension reduction, so that the result from k-means clustering will be more stable. For this reason, the features (for example the variances and the loadings) of the principal components themselves are not of particular interest to the clustering analysis. Therefore, we decided not to present the details of the principal components in the main manuscript.

I thank the authors for the clarification; the explanation is well-received. Figures 4 and 5 in the revised manuscript are particularly helpful in visualizing the clustering results.

Table 3: It would be interesting to see which sites have been correctly classified and which didn't (for example using a confusion matrix). This table is otherwise irrelevant.

Yes, that is good advice. We have now extended Table 3 to also include the count of sites in the two clusters under different degradation classifications.

I thank the authors for the extended table. To further aid the reader's interpretation, it could be beneficial to indicate which clusters represent productive versus degraded sites (Table 4, Figure 4, and Figure 5 in the revised manuscript)? Such indications could additionally improve the quick understanding of the clustering results.

Table 3: Number of stable and transient sites allotted to two clusters at Kuresoi and Nyando using K-means clustering.

	Kuresoi		Nyando	
	Cluster 1	Cluster 2	Cluster 1	Cluster 2
<i>Stable variables</i>				
Degraded	8	9	2	3
Transition	8	7	4	6
Productive	2	7	4	12
Total	18	23	10	21
<i>Transient variables</i>				
Degraded	4	10	2	8
Transition	6	9	8	6
Productive	6	3	5	13

L23-25, Figure 3, and Figure 4: Use the same terms for soil properties in figures and text. Otherwise, the reader cannot easily relate the text to the figures. In these two figures it is not clear what the p-value indicates. Also, the number of samples in each cluster needs to be added.

We have updated the x-axis labels in the density plots in Figure 3 and Figure 4 to be consistent with the variable names in Table 2. We have also removed the p-value from the plots. The p-value is the result from the t-test used to compare different clustering methods. Since the comparison has been removed from the main manuscript, we also removed the pvalue information in the plots.

I have already addressed the Figure axis labelling in a previous comment. All clear regarding the p-values, thank you.

L26: What are degradation labels?

L26-28: This belongs to the discussion and a reference is missing.

We agree that this should be in the discussion

Thank you.

L44-53: This is a repetition of the introduction.

We have changed the wording to avoid repetition

Thank you for the rephrasing.

L71-72: Completely unrelated sentence to the topic.

We disagree. Soil C and N status is often used as a degradation indicator. However, this could be worded better and has been revised. Now reads:

'Apart from microbial biomass C, there was little consistent agreement between the remotely sensed classification with field-based soil variables (Table 2). . Some variables considered good proxy indicators for soil health and are correlated with other important soil functions (Lal, 2016), such as C and N concentrations, C:N ratio and pH, were statistically significant for one site, but not the others.'

Thank you for your clarification.

L75: This is the first time rainfall appears and seems very arbitrary.

We discuss erratic rainfall in the introduction. We are discussing problems with remote sensing soil degradation so it seems reasonable to discuss it here.

Yes, I agree. This makes sense. Thank you for clarifying.

L85-96: This is repetition of your results. This does not belong to the discussion.

Agreed. Deleted

Makes sense now, thank you!

L97: Salinisation? Again, very arbitrary.

Not at all. We are discussing high pH values and the possible reasons for this.

Thank you for your clarification.

L105: Since when were these ratios used? These properties have not been introduced and occur the first time.

The properties are given in table 2 and although we do not go through every non significant property we do mention the significant ones. Here we point out that there is a lack of corroboration from the C, N P ratios which seems reasonable. We added a reference to Table 2 to help the reader.

Thank you for adding the ratios to the table. This is clear now.

L96-108: Some discussion, but no reference at all.

Reference added.

Thank you.

L108-109: Repetition

Repetition deleted

Thank you.

L110-117: This part of the discussion seems interesting. But again, literature is missing and it needs to be extended.

We will extend the discussion.

I thank the authors for extending the discussion on this topic.