Comment on egusphere-2022-209
Anonymous Referee #2

The preprint entitled “Relationship between the stocks of carbon in non-cultivated trees and soils in a West-African forest-savanna transition zone” address an interesting issue as it is the evaluation of the impact of different land uses in are climate type and world region where limited research on the subject has been carried out. It also presents a valuable dataset, coming from a carefully planned field sample which might be of interest to the research community. The search for a land use dynamic model that could provide a scientific framework to their measurements it is also a relevant contribution of the manuscript. As such, the subject and objectives of the manuscript fits into the scope of SOIL and it is of potential interest to the field, particularly because it could contribute with a case study on the impact of different grazing strategies for a more sustainable use of grassland areas.

Despite this, the manuscript still presents some minor issues, in my view, which limits its relevance and preclude that the manuscript could be accepted for publication. Some of these issues are conceptual, while others (less relevant), are related to editorial issues.

I list below the conceptual issues.

1- The discussion part is too concise and does not depend into the fact that landscape/soil conditions is a major driver in determining land use, and in some extend management within the same land use. As part of the general improvement of the discussion section, I suggest to address this issue in more detail.
2- The dataset presents, as it is usual in this kind of large surveys, a large variability. Since the hypothesis to test depends on the ability of sampling to represent the soil properties of the plot I wonder how much of this variability for the land uses of density on non-cultivated trees (e.g. annual cropland) comes the variability in soil OC and TN that should be related to the distance to the tree, which according to the sampling method (see line 110) might be a relevant factor. This might be worth discussing in the manuscript, particularly when the higher variability in the regressions (see Figure 6) tend to appear in the land uses with lower non-cultivated tree density.

3- One of the land used (forest) has a very small number of samples (n=2) which might limit the statistical power of the analysis made on this land use. It will be a good idea to include some caveat on this in the result and discussion section, and comment its possible implications.

4- The authors are right, in my view, to claim that their hypothesis is validated for some land uses. In some areas of the world more skilled farmers or shepherds, tend to be also paying more attention to other positive landscape elements, like non-cultivated trees. Perhaps the authors want to consider this as potential underline factor in their discussion, particularly since this been true a more careful management of the grassland and cropland might be taking place.

5- Line 286. “... mainly driven...” I wonder if the authors want to qualify this statement. They have demonstrated that it is a major driver, given some of the other factors involved perhaps they want to qualify this statement.

6- The authors have developed a conceptual model to explain the dynamic evolution of the model that also might have implications for large scale exploratory analysis of SOC stock in the region. I wonder also if they also want to include this issue in the discussion and conclusions of their study.

There are other lesser questions regarded to editing issues that list below:

1- Line 64. There are also many studies in Mediterranean type of climate as compared to tropical regions in Africa.
2- Line 103. “main crop”. Does this mean that the non-cultivated tree has some use, e.g. wood occasionally? Please clarify

3- Line 110. Was proximity to the non-cultivate treed considered somehow in the land uses with low non-cultivated tree density?

4- Line 120. Indicate also the number of samples, not only the percentage.

5- Line 123. Some reference (or results of internal test) to validate this assumption?

6- Line 125- It is always better to indicate the chemical name not the commercial one.

7- Line 131 add regression \( y=mx+n \) in Figures S2, S3 and S4 to allow the reader to see possible biases as compared to the 1:1 line.

8- Line 143-145. It is a bit confusing. Did you use bulk density from measurements and coarse fragments (frag) from Hounkpatin el al. (2018) or both from Hounkptin et al. (2018)? Please clarify. Please revise titles of Table S2 to indicate this.

9- Line 154. I guess that Figure S4 should be Figure S5. Please revise.

10- Line 189 Table S3. Indicate what the numbers mean in the Table caption. I have another question. Why not statistical analysis has been carried out on Fire, Erosion...

11- Line 171. Where the conditions for normal distribution and variance tested for proper use of ANOVA? Please indicate, or correct of needed.

12- Line 192. What do you mean by restrictions? That sampling could not be carried out because a rocky horizon was found? Please clarify. In Table 2 indicate n (number of samples) at each depth which can provide a glimpse on how many times this happened for any given depth.
13- Line 231, Section 3.5 An additional Figure showing the cumulate SOC and TN stock with depth should be included (it could go in Supplementary material or in Figures). It can help to provide a complementary view of your results and clarify the presentation. It should include an analysis of the statistical significance of differences in mean values of SOC and TN stock which are not shown now.

For those reasons my recommendation to the editor is that this manuscript should be considered for possible publication in SOIL after the issues raised above have been addressed.