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Comment on soil-2022-13

Anonymous Referee #3

Referee comment on "Tropical Andosol organic carbon quality and degradability in relation to soil geochemistry as affected by land use" by Sastrika Anindita et al., SOIL Discuss., https://doi.org/10.5194/soil-2022-13-RC3, 2022

The submitted study examines the relationship between organic carbon storage/stability and land-use change in soils of the tropics. The chosen approach pursues the idea that the soil geochemistry changes due to the history of land use and that this causes a change in the stabilization of the soil organic carbon. The authors base this assumption on the fact that changes in soil geochemistry have an effect on the soil mineral phase, which in turn indirectly affects carbon stabilization. The connection of the mineral phases with their characteristic properties and their changes by exogenous factors is a key point for the prediction of soil carbon stabilization under a changing environment. Six sites under three different land uses were selected for the study. andic cambisols (primary forest and pine forest land use) were described at two sites and aluandic andosols (pine forest and horticulture land use) at the other four sites. Despite the global distribution and the high agricultural relevance of Andosols (land-use change), there are many gaps in knowledge, especially with regard to the behavior of the mineral phases in tropical regions. Although the approach of the study is very intresting, there are some ambiguities in the methodological approach and evaluation of the results.

The novelty of the study is justified by the fact that an effect on the soil mineral phase is assumed to be triggered by land use change. This assumption is based on a single case study that, however, covers fundamentally different soils. Furthermore, an own study is given, which has not yet been published. This makes it very difficult for the reader to follow the chosen approach of the submitted study. Perhaps the integration of data from the other study would be useful for a better understanding, since otherwise the present study cannot stand alone.

This would possibly also invalidate another point of criticism, which relates to the locations used. Their description shows that not all land uses have received a comparable parent material for soil formation through the prevailing volcanism (e.g. "The proportion of primary minerals is higher in the younger NF soil as it is closer (within 1.5 km) to the crater of Mt Tangkuban Perahu and received ash more recently."). It can be assumed that

possible changes in the properties of the soil mineral phases due to changes in land use are already superimposed by differences in the parent material. This makes the provision of detailed mineralogical data all the more important for understanding the sites. Furthermore, this should also better allow for a clearer link between existing and suspected historical geochemistry of the soil and its impact on the properties of present-day mineral phases.

Due to the objective of the study, the mineral phases should be characterized as precisely as possible and what changes occur with longer-lasting agricultural use. The description of the fractionation scheme used gives me the impression that there can be overlaps at least between S+A and s+c fractions. It is conceivable that mineral-associated OC is contained in aggregates of the S+A fraction, which could not get into the s+c fraction due to the relatively low application of 22 J/mL. Which does not allow an evaluation of the mineral phase characteristics and their effect on the OC stabilization. Provideing the database for deriving the selected energy level would support the results shown in the study and the conclusions derived from them.

For the reasons listed, I recommend that the submitted work be rejected. However, due to the high relevance, I recommend that the authors resubmit the study after it has been thoroughly revised.