General comments

I have read with interest this paper, which describes the consequences of vegetation change and erosion processes on SOC dynamic and stocks.
It is an interesting research objective, and the purposes of this work would fall within the aims of this journal. In general, I think the paper is interesting and has potential. However the manuscript needs some improvements, outlined in the specific comments, but its main shortcoming is outlined below.
The study is based on the comparison of toposequences under forest and grassland and the assumption that the soils under these different vegetations were identical or at least very similar before the vegetation change. However, the paper gives almost no information on these soils, either from a chemical or physical aspect. Some parameters, such as texture, have a strong link with the dynamics and stocks of organic matter. How can we be sure that the very large decreases in C stocks observed under pasture is indeed due to deforestation and the erosion it induces, if we do not know that the soils are really comparable? A presentation of the main characteristics of the soils (if only in the supplementary material) is necessary before we can put forward the hypotheses set out in the discussion.
This manuscript, after the necessary improvements and corrections, would be acceptable for publication.

Specific comments

Abstract

Lines 17-18: the time span allowed by the δ13C to study the past dynamic of soil carbon ranges from years to millennia (rather than centuries)

Line 20: the SOC is low, not extremely low.

Line 23: "…which show typical profiles under C3 vegetation, with a slight increase with depth."

Line 30-31: "…suggesting a recent expansion of grass vegetation, and/or that the valleys are depositional areas from organic matter eroded from the hillslopes."

Lines 31-33: "Our approach, based...determine changing vegetation cover". This is true, but it has already been done in different parts of the world and published in many publications in the last 40 years. As this sentence is written, it sounds like a new approach.

Introduction

Lines 87-90: "The stable carbon isotope ratio…show a different degree of isotope fractionation". It is necessary to cite references

Materials and methods

Line 101: the rainfall is not very high; many tropical regions have average annual rainfall between 1500 and 3000 mm or more.

Line 103: "the mean annual temperature varies between 18 and 24°C" Really? Not the mean monthly temperature?

Line 118-120 AND Figure S3: the length and the gradient of the hillslopes are different under forest and grassland. Could this have an effect on erosion processes?

Line 123-126: Why is there such a large distance (about 60 km) between the soil profiles under the forest and those under the grassland? Were there no adequate situations for the grassland soils closer to the forest?

Important information about the soils is missing, which could be in the supplementary material: are the soils under forest and under grassland really similar, in chemical and physical terms. One of the objectives of the paper is to assess the effect of vegetation change on carbon stocks. Several soil parameters, such as texture, can influence organic matter stocks, so it is important to know whether the soils are similar.

Results

Line 207-208: The description of the C profiles is too brief and even wrong! For example, for the F1UM profile the SOC content varies from 60 to 200 cm, between 0.3 and 0.9 %, not 0.1 and 0.2 %.

Line 210-211: The description of the δ 13C profiles is too brief.

Line 218-219: It would be better to say that in the first few decimeters, these two profiles have lower SOC values than the other profiles.

Line 236: The sentence “However, the cumulative…on the GLP hillslope” is unnecessary.

Figure 3c: THIS IS NOT THE GOOD ONE!

Discussion

Lines 253-277: All these explanations of the evolution of δ 13C values under C3 forest vegetation are excessively long. Since the end of the 80's, many articles have detailed this. This does not provide decisive information to answer the objectives of the paper.

Lines 293-295: I do not agree, in the topsoil (what depth exactly?), the C3 contribution is much lower than 70%! See the figure 6.

Lines 297: for GLP-V the δ13C value **increases** between the surface and 50 cm.

Lines 356-358: repetition of the lines 348-350

Line 380: “…, while the outputs include CO2,…” or “…, while the outputs include CO2 **emissions**,…”?

Line 397-398: It is not true that all the studies cited found strong differences in SOC stocks between savannah and forest situations. Moreover, the stocks are not calculated and commented on.

Lines 400-401: That is true, but what does it add to the discussion, at this point. It would be better to delete this sentence.

Line 411: “The δ13C values of the forest profiles increased with depth, which is expected for soils developed for soils developed under C3 vegetation”. It would be better to say that these 13C profiles are typical of soils under C3 vegetation for a very long time.

Lines 417-418: you cannot say that organic carbon input from the new grassland vegetation is not significant: it represents almost a **third** of the carbon stock!

Line 429: “This indicates that the response time to deforestation depends on the rate of depletion of the old C3 pool.” What does this sentence mean?

Technical corrections

Introduction

Line 42: Voarintsoa et al., not Voarintsoa and Cox

Materials and methods

Figure 1a: in the caption, it is written "dotted black line", but it is a "dotted white line".

Line 121: The supplementary material S3 does not show vegetation

Line 148: in the equation, δ13C, not δ13.

Line 197: for D(i), the unit of measurement is missing.

Results

Figure 2: in the caption: “middle” not “middles”

Line 207: the topsoil samples are 0-5 cm not 0-10 cm

Line 209: “…between -25.5 and - 27.1‰ …”

Discussion

Line 225-226: verify the profiles which show gradual decline: GLP-B, GLP-UM, GLA-T (not GLA-M)

Line 240: “at different depths” appears two times

Line 280: “…values of -20 down…” The symbol ‰ is missing.

Line 350: In the references, Brosens et al. is indicated as published in 2022.