Reviewer 1

In this paper, the authors tested the impact of aggregation of soil texture data to coarser resolutions on model simulations, and the sensitivity of vegetation and soil carbon to soil texture in three popular Terrestrial Biosphere Models (TBMs). They demonstrate that vegetation and soil carbon are, for the most part, insensitive to soil texture. Data aggregation to coarse resolutions from TBMs hinder most of the soil texture spatial variability.

First, I would like to acknowledge the importance of studying the influence of soil texture in models. I believe it's a subject that hasn't received as much attention as it should lately. I was surprised to see the lack of sensitivity of models to soil texture, and I'm glad this paper is bringing this up.

We would like to thank reviewer #1 for their positive evaluation of our work. We agree that soil texture (and belowground processes in general) does not receive as much attention from the vegetation modelling community as it should and hence this study. Please find below our reply to the comments.

<u>Major comments:</u>

My most important concern is regarding the relevance of the aggregation of soil texture input data. I think the point the authors make is valid, however, it's an issue that models have with most data input, as the scale at which they work is coarse. What I mean is that although is good to point this out, I don't think it's groundbreaking or surprising. Therefore, I'm not sure it's worth having this result as the main finding of the paper. I think the other result about the sensitivity of the vegetation and soil C to soil texture is much more important and should be the focus of the paper. While the soil data aggregation could be a secondary finding. I liked the suggestions provided in the discussion on how to address this issue in models. However, in the abstract and conclusions, I think the framing should be more directed towards a place where models could improve by proving some measure of uncertainty with the selection of soil texture.

We totally agree with this comment. Indeed the impact of the soil aggregation of soil textural data is not the most important finding of the paper and the second aspect (the lack of sensitivity of most model outputs to soil texture) is much more interesting. We propose to address this comment by putting much more emphasis on what used to be the second objective in the previous manuscript version. This includes: deleting the statements about soil aggregation in the abstract and conclusion, rephrasing and re-ordering the objectives of the paper, and shifting/re-balancing some of the results and discussion.

Regarding the same topic, I think it's worth reinforcing in the discussion or the abstract why it's important to consider the uncertainty in the influence of soil texture in vegetation activity and drought, particularly for large scale/ecosystem level studies.

In the next version of the manuscript, we will better explain why it is so important for large scale level studies to consider the uncertainty of soil texture in vegetation activity and drought in the abstract and discussion, e.g. by referring to the recent paper of Yang et al. (2022) in PNAS (https://doi.org/10.1073/pnas.2101388119) where they show that the pattern of AGB changes in tropical forests due to extreme drought events is primarily associated with drought severity, duration, <u>as well as with soil clay content</u>.

About the biomass, GPP and soil carbon simulations' comparison with observed data: can you provide more information in how this comparison was done? The methods don't provide much detail on what was done and it's difficult to understand exactly what you did. I think it was a correlation analysis, but I'm not completely sure that was it.

We agree that the methods were not fully detailed on this aspect in the previous version of the manuscript. Also because the comparison with observations was not thoroughly performed (we mostly compared the spatial and absolute distributions visually). We will reinforce that part by adding correlation analyses that we will describe in the new version of the material and methods.

Minor comments:

I provided some minor comments in the attached pdf file.

We would like to thank the reviewer for this detailed feedback, which is very useful. We will implement all the suggested changes.

Reviewer 2

In this study, the authors perform a sensitivity analysis to different soil texture properties (from the global SoilGrids250m dataset) on the carbon cycle in three Terrestrial Biosphere Models (TBMs), namely LPJ-GUESS, ED2 and ORCHIDEEv2.2. They evaluated the aboveground biomass spatial distribution, ecosystem Gross Primary Productivity (GPP), soil carbon content and drought stress simulated by the three models over the Amazon rainforest region, using model default pedotransfer functions. They found that the model outputs were mainly insensitive to soil texture change, showing the poor representation of the soil-vegetation coupling in the TBMs.

Overall I find the topic very interesting and important to produce accurate simulations in the land surface models. There are some points that I think need to be improved/clarified in the manuscript to be suited for a publication.

We thank the reviewer for the positive evaluation of our work and the detailed feedback below.

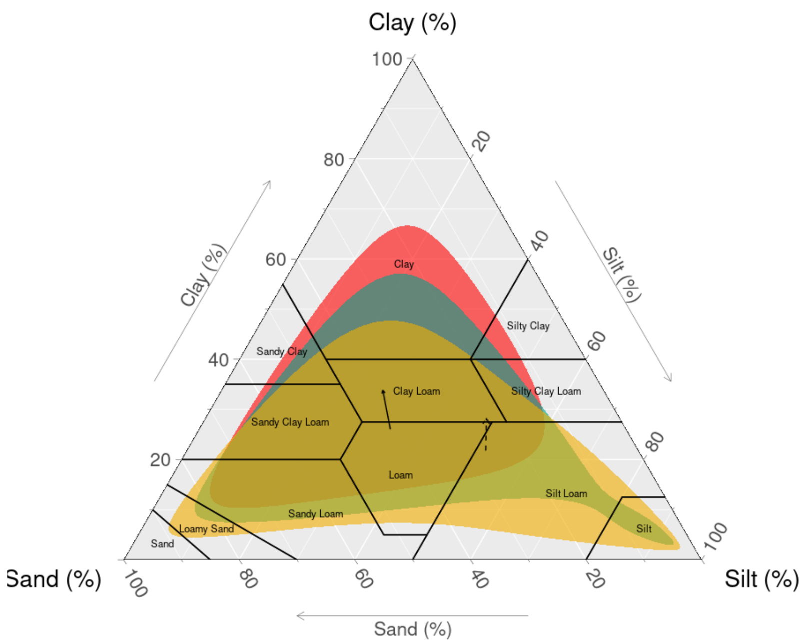
Major comments:

1. I think the soil texture is also connected to the land use / land cover over a region. Even more, the root depth plays a key role in the water uptake and therefore in GPP and ET processes. Given that the Amazon basin has gone through deforestation activity for more than three decades, it is important to take into account the land cover, the root depth and the soil texture to have a realistic effect in the change in biomass.

We totally agree with the reviewer that to realistically model spatial (and temporal) patterns of biomass and GPP, land cover (and land use changes) need to be taken into

account on top of soil and root properties. However, the main purpose of our paper was really to test the intrinsic sensitivity of all three investigated models to soil texture.

Yet, the comment of the reviewer inspired us to do additional analysis. We have indeed found a few references that show a link between land use and soil texture, see e.g. Veldkamp et al. (2020) in Nature (https://doi.org/10.1038/s43017-020-0091-5). Even though there is not a clear, strong agreement on the exact impact of land cover change on the soil texture, the literature suggests that the impact of land cover change on soil texture is of the same order of magnitude as the changes that we applied in our soil scenarios. We thank the reviewer for pointing out this important aspect and we will now include in the revised manuscript the literature body on the deforestation (and land cover changes in general) in the Amazon. Additionally we will adapt Figure 1 so that it compares the changes of soil texture we considered in the scenarios with the observed changes of soil texture due to land cover changes:



where the solid arrow is the average change between the Min. and Mean clay scenario and the dashed arrow illustrates the impacts of land cover on the soil texture from existing literature.

Regarding the rooting depth, while we fully agree with the reviewer that this is a key model parameter in all three models that is interconnected with soil texture and land cover change, investigating its impact was beyond the scope of this paper focusing on the effect of soil texture. Since root parameters are also linked to plant hydraulic representation, assessing the sensitivity to such parameters would also require an in-depth analysis of other plant functional type parameters, which will complexify the analysis and dilute the key message of this study. We propose to extend the discussion with the importance of including root (and below-ground) traits in future analyses.

2. I think the change in soil clay should be carried out with a recalibration of other land surface-related parameters in the TBMs to have an accurate representation of land surface processes. Maybe the low sensitivity observed in the simulations is in part related to this non parameter recalibration?

While we agree with the reviewer that any model structural change should be accompanied with a recalibration of the model parameters to match the observed behavior of the modelled biome, here we show that the model is (almost) completely insensitive to soil texture. This means that recalibrating the models for each of the soil texture maps would lead to (almost) the same sets of parameters. We think that the low sensitivity observed in the simulations is mostly due to the model structure that oversimplifies the belowground component of the land surface rather than a model calibration issue. Yet, we will include and leave the two options open in a new paragraph in the discussion.

Minor comments:

1. I suggest in the introduction to include also relevant literature in regards with Amazon deforestation. I think deforestation and subsequent land cover change has an effect in soil texture variability that is worth to mention.

The references mentioned above about the link between Amazon deforestation and soil texture variability (and more) will be added in the introduction. We will also include them in the discussion

2. The authors cite literature about ORCHIDEE and ED2, but not about LPJ-GUESS. I suggest to include recent work done with this model.

The most recent literature about LPJ-GUESS will be added in the manuscript

3. Line 96 → Please specify sensitivity of what property/variable from the three TBMs to soil texture?

See next comment

4. Line 96-98 → Please rephrase this idea "which occupy different positions along the vegetation representation abstraction continuum". Perhaps do the authors mean something like the three models have different levels of complexity?

The sentence will now read:

"In this study, we explored the sensitivity to soil texture of important model outputs (e.g. GPP, soil carbon) of three state-of-the-art TBMs with different levels of complexity"

5. Line 107 → Could you please explain your reasons to select the cohort mode over the other two options and the related implications?

Cohort mode is the default vegetation representation for simulations using LPJ-GUESS. Population mode is inherited from the original LPJ-model, which is essentially a big leaf model (Smith et al., 2001). For simulating this vegetation representation we would

recommend to use a recent development of the LPJ model instead (e.g. von Bloh et al., 2018).

On the other hand, the individual mode in LPJ-GUESS shares the vast majority of its code with cohort mode in the current version of the model. While individual mode would allow for variations between different members of a cohort (e.g. in establishment parameters), this is not currently implemented. Therefore we expect that the impact of choosing cohort mode over individual mode would not have any influence on the outcome of our study (except for computational time!).

6. Line 108 → I believe the acronym "PFT" has not been properly introduced nor explained so far.

The acronym will be defined after its first use

7. Line 110 → Which meteorological drivers? And where do you get this input data from?

The order of M&M will be changed according to one of the next comments of the reviewer, so that it is clear from the beginning where the input data come from and which met drivers are used for each model

8. Line 117 → "Soil moisture in the top two layers (20 cm) is available for surface evaporation". Does this mean that deeper soil moisture (>20 cm) is not available for evapotranspiration processes? I believe this is not an accurate representation of the soil water uptake by the Amazon rainforest (see doi: 10.1038/372666a0). How can this affect the interpretation of your results?

We believe that there was a confusion here between the soil evaporation, and the overall evapotranspiration. While only the top layer is contributing to the former, plants can access both top and bottom layers for transpiring. We would not modify this sentence which explicitly refers to surface evaporation and not plant evapotranspiration.

9. Line 117 → “Only two larger percolation layers are defined”. Larger than what?

This refers to the previous sentence in the manuscript, where we describe that the soil profile is divided into 15 soil layers of 10cm thickness each. However, for simulating percolation, the model currently uses two larger (than 10cm) layers of 50cm and 100cm. This is a remnant of a previous version of the model, which only used these two larger layers for all soil processes. The 15 layers in the current version are mainly used for a more detailed calculation of soil temperature.

10. Lines 121-122 → About the soil water content per grid cell, how does it change in time? Do you give soil moisture as input data to the model to compute the water content at filed capacity and at wilting point?

In all three models, soil water content is a dynamic state variable that is tracked during the water cycle resolution during the simulations. Hence, soil moisture is not provided as an input, but is a prognostic variable computed based on the different in- and outflows of water in the soil compartments. The water content at field capacity and wilting point are computed from soil information according to pedotransfer functions, as described in the manuscript.

11. Lines 133-134 → I suggest to remove this information or move it to the introduction, as I do not see its relevance for the methods nor for the results.

We will remove that sentence

12. Line 137 → Similar to my previous comment, where do you get the meteorological forcing from?

We will reorganise the methodological section according to one of the reviewer’s comment below so that the origin of the drivers will be more clear.

13. Lines 152-153 → "Simulated sites are characterised by vertically uniform soil texture and hence hydraulic properties over the entire soil column". Did you mean: Simulated sites are characterised by vertically uniform soil texture and hence uniform hydraulic properties over the entire soil column?

We will rephrase to:

"**In the simulations, sites were assumed to be characterised by vertically uniform soil texture and hence uniform hydraulic properties over the entire soil column.**"

14. Lines 15-156 → I am confused here. If this model can use the vertically integrated soil water from the deepest soil layer (which I believe is 8 m depth based on line 148), how can you compare the results obtained from this model (ED2) with those obtained from the previous model (LPJ-GUESS) that only uses water available from the first 20 cm of the soil column? Also, the third model (ORCHIDEE v2.2) has 2 m depth soil profile, so the same question would apply for the results from the third model.

Indeed, the three models have different soil depths (and number of soil layers, and formulations of water uptake, and etc.). We are aware of those differences that we tried to summarise in Table 1 and this is the main reason why our study focuses on the sensitivity of those models to soil texture rather than model performances. The key aspect of such model intercomparison exercises, as commonly performed (see e.g. the LBA-MIP project or doi: 10.1111/gcb.13442), is precisely the fact that each model represents the same processes with different approaches and hypotheses. While we expected clear differences in sensitivity because of their structural differences, the fact that all three models exhibit a very low sensitivity to soil texture is in our opinion the key finding of this study.

15. Line 162 → For a given vegetation

Will be corrected.

16. Line 164 → I see that here you define for the first time the acronym PFT, but it was used several times before.

Will be corrected (see comment above)

17. Line 166 → A reference for Richards equation would be good here.

We will add a reference for Richards equation.

18. Line 185 → With "current PFT distribution" do you mean a global ESACCI land cover map from 2015? From 2021? From 2022?

The land cover map corresponds to the year 2015 to be consistent with the maps from Avitabile et al. (2016) and MODIS data (2006-2016). This information will be added in the revised manuscript.

19. Line 194 → What do you mean with "the most default"? Is it that you varied only few parameters from Table 1?

It means that we did not re-parameterize anything compared to the most recent version of the model, corresponding also to the model version commonly used by default in impact assessment or scientific studies that does not require new parameterizations. We will rephrase to:

"Similarly, we did not change any model parameters (with the exception of the soil textural information) compared to the model default parameterization for the tropics."

20. Lines 206-207 → I do not understand the sentence. Perhaps do you mean that you average either (i) the last ten years of the historical period (2006-2016) or (ii) the last year of the historical period (2016)?

We will rephrased to:

"All the results from the vegetation model simulations presented below are either the averages of the ten last years of either the spin-up or the historical period (2006-2016) or the averages of the very last year of the historical period (2016)."

21. Lines 215-217 → The information of where to find the code is already in section Data and code availability, therefore, I suggest to remove it from the methods section.

Indeed, we will remove it here and keep it in the code availability section.

22. I think Section 2 should be reorganized in a more straightforward way for understanding. For instance, you could start with a first subsection that contains a description of the study area (coordinates, land use/land cover, climatology, etc.). A second subsection could be the models' description. A third subsection could be a brief description of all datasets used in the study. In a fourth subsection you could integrate the simulation protocol, the soil scenarios and the model parameterization. And a fifth subsection could be the analyses.

We thank the reviewer for this suggestion. We will re-organize this section accordingly.

23. Figure 1B is not really showing the difference between the intra-grid cell and the inter-grid cell variability but both in the same plot. I would suggest either to rephrase this part of the caption (to something like inter (black) and intra (red) grid cell variability...), or to really plot just one line that shows the difference between the two lines. Moreover, why does the legend include sd (mean) for the inter-grid cell variability and mean (sd) for the intra grid cell variability? What is "sd"? I presume is standard deviation, but the acronym is not defined.

We will rephrase that part of the caption as:

"**Subplot B shows both the intra-gridcell (i.e. mean of the standard deviation (sd) of the clay content) and the inter-gridcell (the standard deviation (sd) of the mean clay content) variability as a function of the spatial resolution**"

24. Caption from Figure 1C. I suggest to remove the last sentence ("showing a clear shift toward larger clay contents in the Max. clay scenario"), as this should not be part of the caption, but part from the main text describing the results.

We will remove the last bit of the last sentence as it should indeed not be part of the caption.

25. Lines 238-239 → The sentence "The three soil scenarios were built on this intra-gridcell variability in soil texture...within each gridcell" is not part of the results but part of the methods. You should move this to the methods section.

We will remove this sentence as it was already explained in the material and methods section.

26. Line 245 → Perhaps do you mean supplementary Figure S2?

Indeed, it will be corrected.

27. Figure 2 and Figure S2 → I strongly recommend the authors to change these Figures. Instead of plotting the mean values of each model in the second row of the Figures, you could plot (for each model) the difference between the model output and the reference data. It perhaps will be interesting to see in which specific regions there is better/worse performance of the models. The way the Figures are displayed right now makes it very hard to compare overestimation/underestimation of the models in regards with the reference data. Moreover, why is Figure S2 in supplementary if its results are as important as the results from Figure 2?

We agree with the reviewer and we will re-draw those figures according to the reviewer' suggestion. We will also move supplementary Figure S2 to the main body.

28. Line 264 → The first sentence does not specify differences in what.

We will add:

“**Large differences *in all investigated model outputs* existed between models for the same soil scenario.**”

29. Line 266 → Which correlation coefficient did you compute? The methods section does not mention any correlation analysis, so this came as a surprise in the results section.

This was derived from the quantile regression analysis. We will add in the results:

“..., as illustrated by the quantile regression analysis (Figure 3)”.

And in the material and methods sections, we will also add the reason why we choose to perform a quantile regression analysis:

“We used a quantile regression given the nature of the vegetation productivity response to SDI: for SDI close to 1, the GPP variability is high (other resources can limit GPP) while for SDI to 0, the GPP is necessarily low.”

30. Line 266-271 → Can you please explain how can we infer these numbers from Figure 3? Or is this information not shown in the Figures and you computed it elsewhere?

We will clarify those sentences by (i) moving the references to Figure 1 to its proper location (section 3.1), and adding references to the vertical and horizontal boxplots of Figure 3:

“Across the three scenarios, we observed that increasing clay content slightly increased drought stress (i.e. decreased SDI) by 2.6, 0.7 and 1.5% (change of the drought stress index from the Min. clay to the Max. clay scenario) for ORCHIDEE, ED2 and LPJ-GUESS, respectively (horizontal boxplots in Figure 3). This increase in simulated drought stress was accompanied by a decrease in productivity for all three models, respectively by 2.7, 1.9 and 3.2% (vertical boxplots in Figure 3).”

31. Line 272 → Please indicate the location of these gridcells and the reasons to select them.

The location of these few grid cells is spread over the Amazon basin and differs for each model. We do not think that adding their location would bring any additional information on the model functioning. Given the already high complexity of the figures, we would prefer not to add the location of these gridcells in the maps.

32. Line 277 → You mention here Soil moisture index. Is it the same of soil drought stress index? If so, you should refer to it in the same way throughout the document.

Indeed, we meant the Soil drought stress index, which we will correct throughout the document.

33. Line 278 → After the sentence “aboveground biomass” add: (supplementary Figure S3).

We will add this reference to supplementary Sigure S3.

34. Line 284 → Did you perform any significance test to say this? If not, you should write “We observed some substantial impacts…”

We did not, so we will use the word “substantial” rather than “significant”.

35. Line 290 → Change from “most important” to “the most important”.

Will be corrected.

36. Figure 6 → Could you provide the R2 for the goodness of fit and the slope of the fitted line? Also, I strongly recommend to change the markers for the scenarios, it is really hard to differentiate one from the other.

We will add the r^2 and the equation of the fitted lines and change the markers for the different scenarios to better differentiate them.

37. Line 304 → I would omit the cross-reference here of Figure 1 as I do not see a strong reason to use it in the context of the sentence.

We will remove the cross-reference to Figure 1.

38. Lines 315-316 –> Explicitly indicate that the supplementary Figure S6 is from Poggio et al.

2021. Perhaps something like: see supplementary Figure S6 from Poggio et al. 2021.

We will modify this sentence as suggested.

39. Figure S5 → Explicitly indicate in the caption that Ksat is saturated hydraulic conductivity.

Will be added.

40. Overall, I think that the discussion section should not present cross references to Figures. All the Figures (both from the main text and from the supplementary material) should be properly described in the results section and the main message derived from them can be included in the discussion. Specially, Figure S5 is mentioned for the first time in the discussion, so please move it to the results section.

In the next version of the manuscript, all figures will be described in the results sections. So the results and discussion sections will be re-organized

Non exhaustive list of References that will be added to the manuscript

- Eleftheriadis, A., Lafuente, F., and Turrión, M.-B.: Effect of land use, time since deforestation and management on organic C and N in soil textural fractions, Soil Tillage Res., 183, 1–7, https://doi.org/10.1016/j.still.2018.05.012, 2018.
- Veldkamp, E., Schmidt, M., Powers, J. S., and Corre, M. D.: Deforestation and reforestation impacts on soils in the tropics, Nat. Rev. Earth Environ., 1, 590–605, https://doi.org/10.1038/s43017-020-0091-5, 2020.
- Smith, Benjamin, I. Colin Prentice, and Martin T. Sykes. 2001. “Representation of Vegetation Dynamics in the Modelling of Terrestrial Ecosystems: Comparing Two Contrasting Approaches within European Climate Space.” Global Ecology and Biogeography 10 (6): 621–37. https://doi.org/10.1046/j.1466-822X.2001.t01-1-00256.x.
- von Bloh, W., Schaphoff, S., Müller, C., Rolinski, S., Waha, K., and Zaehle, S.: Implementing the nitrogen cycle into the dynamic global vegetation, hydrology, and crop growth model LPJmL (version 5.0), Geosci. Model Dev., 11, 2789–2812, https://doi.org/10.5194/gmd-11-2789-2018, 2018.