

Interactive comment on “Variations in soil chemical and physical properties explain basin-wide variations in Amazon forest soil carbon densities” by Carlos Alberto Quesada et al.

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Received and published: 29 September 2019

We thank the referee for the comments. Most comments were dedicated to style and structure of the manuscript. We are working to incorporate the comments in the best way possible to improve the manuscript readability.

Here we respond to each comment separately:

General comments: Comments on manuscript length and readability:

Referee 2: “A heavy revision and resubmission of the paper is recommended in order to condense the paper, make the writing more straightforward and direct, and to address

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a few important questions addressed below.”

Response: We have made efforts to make the text more direct (especially in terms of breaking up and shortening sentences in the Results Section) and have also attended the specific points raised by both reviewers. However, we do not agree with the view that short papers are necessarily the best or only way to communicate results. In our view, a detailed and comprehensive analysis, when well supported by data, becomes a definitive reference and thus reaches out to broad audiences. Short papers can be good, but not necessarily are, the opposite also sometimes being the case. As noticed by Reviewer 1, our personal style is somewhat ‘expansive’, which so far has led us to establish a successful publishing record. For instance, previous work published on soils of the Amazon by the lead author in Biogeosciences (Quesada et al. 2010, 2011, 2012) has had an even greater number of final pages published, but this did not stop these papers to receive over 300 citations each. We feel that the points raised by the reviewers have indeed helped to make the paper clearer and we have made changes to break up longer sentences and phrases into smaller units, but hope that there is no need for further shortening of the manuscript just for the sake of it.

Referee 2: “There are also too many figures with too many colors and shapes, which both dilutes the impact of individual graphs and muddles the findings”

Response: We respectfully disagree. Our figures follow the same style and standards of our previous papers on the soils as references above, to which the manuscript presented here is somewhat a continuation of such. There is no actual gain in simplify the “too many colors and shapes”. In truth, we believe just the opposite. Such colors and shapes reflect individual soil types and thus allow the reader to understand that pedo-genetic level is an important factor in controlling SOC. This adds to the impact of the paper since readers can relate the results to their own soils of interest, and also help them to perceive the importance of common chemical and morphological characteristics to shape SOC.

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Specific comments from Reviewer 2:

“Introduction is far too long and spends too much time on general soil chemistry processes (i.e. lines 70-124). Much of this is background knowledge (i.e. line 105 “The extent to which DOM precipitates is largely influenced by soil pH”). This is not a process-based paper but a broad geographic survey of soil carbon concentrations, and the introduction should reflect the breadth and focus of the paper”.

Response: We have shortened the introduction. However, giving the general interest in the Amazon forest from researchers with a wide range of scientific backgrounds, we purposely aimed to provide a text that communicates to broader audiences and therefore it seems important to keep some background information to aid the understanding of our results. Thus we believe that everything remaining in the introduction is relevant to facilitate the understanding and interpretation of the data. Also, given that this is the first time that Al/OM interactions is reported in Amazonia, we feel that background information in the chemistry of this process may be really important for scientists working on that region.

“Be clear about what work was completed in this paper. Parts of the methods and results include references to previous work, which should belong in the discussion section (Line 295: “based on a previous analysis of a subset of sites”).” Response: We thank the referee for spotting this and have clarified all issues on the text.

“Results section needs to be heavily revised. Rather than saying “Figure X shows. . .”, state the findings and put the figure at the end. I.e. for Section 3.2 (Line 316-319), the length of these sentences could be halved if the writing was instead: “Mineralogy of LAC and HAC soils were distinctly different based on PCA analysis (Figure 3a).” Response: This has been changed and text is now more direct

“The 2nd biggest issue is this work heavily relies on a previous paper’s work. In the methods, Section 2.2 (Line 179), the author writes as if the soil classification was performed in this study. However, in Section 3.1 (Line 294), it turns out this clustering

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was performed by a previous study.” Response: We thank the referee for spotting this confusion. Soil classifications and clustering were performed in this study and the text has been changed to make it clearer.

“Terminology needs to be consistent and clear. Use specific p-values. Do not switch between “less weathered” and “low activity clays” to define for instance the 1st group. Those two descriptors do not mean the same thing.” Response: We accept the comment regarding terminology and have changed the text accordingly. Where we consider p values critical to the arguments presented they have been specifically presented in the text.

“Some QA/QC reporting for the soil density fractionation is needed. Why that particular subset of samples? And what was the recovery of soil mass and soil carbon? Were there reps? SDF analysis is often coupled with a reporting of sample recovery in the Appendix.” Response: Details on the selection of subset of study sites and the C recovery after fractionation have been added to the Materials and Methods.

“Remove the analysis of temperature and precipitation controls. The paper is stronger without it. Temperature is not highly variable in this region. Rather precipitation is. However, the focus on the paper is on the mineralogical controls of SOC preservation and stabilization. Precipitation and temperature can be brought up in the discussion, especially for the Arenic soils.” Response: Apart from the Tables of Kendall’s τ do not undertake any analysis of effects of temperature and precipitation. Rather we simply test for any model biases as affected by climate by looking at regression residuals in the Appendix, which we believe is appropriate.

“Be upfront about the number of soils in each group. It appears the numbers are not consistent based on Fig. 1. How is this accounted for statistically to ensure against bias.” Response: Table 1 shows clearly the soil types for each group. Our statistical approach does not require a balanced design, but when this had to be taken in consideration, i.e. with Kendall taus, the number of observations and its influence on

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significance tests has been very clearly stated.

“Lastly, land-use change is an equally important threat to this region!! What is the situation in terms of deforestation and how is that expected to change in the future? Consider this alongside climate change in both the intro and discussion.” Response: We consider that this is a topic beyond the scope of this paper and, with the paper already being longer than is typically the case, we have purposely avoided discussing what we consider to essentially be diversions from the main theme of the manuscript.

L25 – are you sure all of these soils are “pristine”? “Minimally disturbed” might be more appropriate. Response: this has been corrected accordingly.

L31-33 – Omit beginning part of sentence “SOC fractionation studies further showed that. . .” – let the focus of the sentence be on the findings Response: Done

L36-38 – run-on sentence and complicated phrasing “and with this mechanism enhanced by.” Sentences need to be more straightforward and direct. Response: We accept the comment and have changed the text accordingly

L24-45: Title uses the word densities, but abstract focuses on soil carbon concentrations and soil carbon stocks. Keep language consistent Response: We accept the comment and have changed the text/title accordingly

L45: Rather than end on a negative result (“aboveground biomass nor precipitation. . . were found to exert any influence. . .”), end on a more powerful positive finding. . . i.e. soil and litter qualities are more powerful predictors! Response: Again, this a matter of style and we are happy to leave this as is.

L49-60: What is the focus of this paragraph? This is a list of facts that don’t work together towards an overall point such as soil carbon stocks being equally as important as biomass carbon in Amazon forests. Response: We accept the comment and have deleted this paragraph for shortening the text

L70 & throughout the paper: Try to use a more active voice than passive. I.e. “clay

mineralogy controls specific surface area” in line 70. Response: Again, is nothing more than a matter of personal style and we see no reason to change anything in this respect.

L79-80: Mention anion exchange capacity. Response: Done

L73 and L76: Be consistent in your terminology – “Hydrous Fe and Al oxides” vs “Iron and Al hydrous oxides” – pick one and refer to it consistently Response: Done

L122: Do not assume reader will know why Fe-associated co-precipitation is considerably less important than Al precipitation Response: We have adjusted the text accordingly

L126: Edaphic factors involve physical (temperature, precipitation) and biological factors, but the previous paragraphs have focused on chemical factors. There is a large leap from chemical stabilization to all edaphic factors without an explanation for why. Response: We typically use the word ‘edaphic’ in accordance with its dictionary definition viz. “of, produced by, or influenced by the soil” and by our reckoning this does not include the current day climate. Although, of course, we accept the critical role of climate in pedogenesis sensu Jenny. The text has been changed to avoid misunderstandings.

L132: Wade et al. 2018 is a more recent citation. Response: We have looked for this reference and two articles could be found for Wade et al. 2018, but none seemed relevant. As the full reference was not given by the referee it is possible that we did not find the right one.

L140: Be precise, not vague. What does “more similar weathering levels” mean, and how is that different from chemical and mineralogical characteristics? Aren’t those two descriptors collinear? Response: We have removed the ‘weathering levels and/or’

L142-143: Instead of “less weathered,” can you say more rich in X, Y, Z? Response: Bearing our sometimes non-specialist audience in mind we have simply changed this

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to 'generally younger soils'

151: Omit "may" Response: Done

154: Be succinct! Instead, say "Here we explore the climatic. . . . Response: Done

158: Make the main action verb ("associated") more clear Response: Done

170: "Usually" is not a scientific word! Please omit Response: Done

164-176: Study site needs more information. How many different soil orders were covered by these 147 plots? This is a big omission that should not be left to a table or figure but succinctly described in the paragraph. Additionally, line 142 has information that should be included in the section under study site not in the introduction. Bring the reader up to speed. How accurate are the classifications for this region as well? Response: Indeed, we thank the referee for spotting this. There are 14 soil orders and this information was added to section 2.1. We take the opportunity to clarify that soil classification was performed as part of this study.

165: "Primary forest plots" means something different than "Pristine forest plots" (Line 25). Be consistent Response: Done, it was corrected accordingly

176: WHY are only the top 30 cm reported?? This needs to be explained and the entire paper needs to better reflect this. For instance, mention this depth in the Abstract. Response: Done, this was added to the abstract and explanation for the reason added to material and methods.

212: How was this subset of sites chosen to ensure a representative subsample? Response: This information was added to material and methods.

232: Omit the sentence about leaf litter lignin estimates. That should be included in section 2.1 when the previous studies are mentioned. Response: Indeed it was out of place, but rather than 2.1 we decided to move it to the section 2.3.1, which deals with chemical analysis.

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259-271: Was it bulk soil XRD (2 mm) and the clay fraction, or just bulk soil? Response: Bulk soil. This was made clear in the methods.

275: What does SRTM stand for? Response: This was made clear in the methods.

294: Begin the results section with the findings, not a description of a figure. It is confusing whether this cluster analysis was completed in this study or in previous work. Response: Done, things were made clear in the text

298: Need to define low activity clays and high activity clays in the Introduction – this seems to be an important part of the paper Response: Indeed. This was made clear in the introduction

306-313: What were the ranges in CEC and clay %? Were the average values for these 3 groups significantly different? Response: This comes later in the text

315: For the mineralogy, it is important to know what fraction of these soils are clay sized. Response: we are not at all sure as to what the Referee wished us to address here.

330: Be precise, what does varying proportions mean? Response: Means that the proportion of sand varies.

343: Give exact p-value(s) Response: In our view given the many comparisons involved in Fig 4 this would only serve to unnecessarily clutter the text accompanying what is essentially a ‘background setting the scene descriptive diagram’

348-350: Confusing sentence, I don’t understand it at all. Response: Text has been improved for clarity

355: An appropriate way to demonstrate higher variability is to give the coefficient of variation for both groups Response: We believe that in this particular case a simple ‘eyeballing’ of Fig. 2 is all that is required for the different variabilities to be appreciated.

378: Instead of all the acronyms, which are hard to keep track of, can you say cation

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exchange capacity instead of IE? Response: Done

403-406: Knowing the clay fraction of these two soil groups is essential to interpreting the data. Response: The different clay fractions of the different groups is shown in the proceeding Fig. 4 and with clay fractions also being the independent variable in all three panels. Thus we are not at all sure as to what the Referee wished us to address here.

430: 3.5 Soil carbon/mineralogical associations is not an appropriate subtitle when the association between leaf litter and C storage is explored: Response: Correct, subtitle has been changed

534: Is 0.49 a percentage, a mass?? Response: It is a fraction (values vary between 0 and 1), text has been adjusted to make it clearer

656: The discussion subheadings need to be consistent. I.e. if you use low activity clays for one heading, use high activity clays rather than less weathered soils for the following heading. Also, why the change to “retention” for 4.1.2? Response: Correct, subtitles have been changed

661: Rather than lumping other studies into the same sentence as your results, separate their findings into a different sentence. Response: We see no issue and have chosen to leave as is.

690-691: I don't understand why the sand and aggregate fraction is lumped together. In soil density fractionation studies, it is often the light fraction (free organic matter), occluded light fraction (organic matter bound up in aggregates), and the dense mineral fraction (residual organic matter bound to minerals). This latter group can be subdivided into clay+silt, and then sand. But to clump sand and aggregate fraction together makes no sense. Response: This is a consequence of the methodology used. Zimmermann et al. 2007 initially separates DOC, clay and silt, from the remaining coarse particles (>63 μm). This coarse fraction contains POM, sand and aggregates that have

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not break down in sonication. POM is later separated by density. As many tropical soils have very strong aggregates, such coarse fraction often will have clay aggregates remaining. This issue was made clear in the methods.

767: I would be very careful with such a statement. I'm sure this process of AI/OM interactions has been studied elsewhere. Focus instead on your findings, which are interesting in and of themselves! Response: We are not aware of any study that has reported AI/OM interactions in forest soils of Amazonia, with the possible exemption of particular studies on podzolization, which is a completely different matter.

833-834: Do you mean biomass carbon inputs impacting soil carbon stocks? It feels like some key adjectives are missing here. Response: Text has been adjusted for clarity

838-840: This is a classic example of this paper's poor writing. Be direct, be concise! For instance, "Our findings do not negate the possibility that future climatic changes will have a significant impact on soil carbon stocks in the Amazon Basin." Response: Text has been adjusted for clarity

Fig. 1 – This is not a systemized, randomized design across the Amazon Basin. These plots are clustered by geographic area. I think a more appropriate statistical approach would be to cluster these sites by location and compare between groups. This is essential for considering the effects of precipitation and temperature. Within and between group variability needs to be addressed. Response: Given its' geological and climatic complexity, a systemized, randomized sampling design across the Amazon Basin would be, for all practical purposes impossible. We agree there is some geographical clustering of sites, but lumping these together for any statistical analysis would not work as in many cases there are (purposely) different soil groups (and considerable variability within the groups themselves) at each geographical location. We do accept of course that there is a lack of independence between clustered sites in terms of precipitation regimes, but as precipitation is not a predictor variable in the

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models presented this is not an issue in our view. The residual plots of Figure A1 do not suggest systematic biases according to precipitation regime in any case.

Fig 11. – I do not understand this graph at all, which is unfortunate because I can tell it has important results! Response: The y axis legend was adjusted and the figure caption expanded. We hope it is clear now!!

Interactive comment on SOIL Discuss., <https://doi.org/10.5194/soil-2019-24>, 2019.

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