Response to Reviewers' Comments

Mitchell et al.: Long-term patterns of peat accumulation and organic matter decomposition in Costa Rican peatlands (manuscript egusphere-2026-1209).
Biogeoscience

The reviewers' comments are presented in grey, bolded text.

The responses to those comments are presented in black, indented text.

Reviewer #1

Mitchell et al. reports an impressive dataset of Costa Rican peatlands geochemical analysis enabling a carbon store estimation, successional and peat accumulation histories, while attempting to establish links between peatland types, accumulation and organic matter quality. Given the dearth of field-based peat studies in the Central Americas, this study lays important groundwork relevant for a range of scientists and policy makers to understand the value of tropical peatlands in Costa Rica.

The introduction is very well written, outlaying current paradigms and challenges of tropical peat biogeochemistry and carbon stores, peatland extent and uncertainties on their inceptions. Especially the regional importance is well highlighted. The methods could benefit from some clarification (Major comment 3). The results and discussion could benefit from restructuring, the authors spend a lot of the manuscript describing results and could benefit from reducing this (Major comment 1&2) and spending this on placing their results in wider literature (expand discussion). That being said, the manuscript presents some impressive results which should be highlighted better in the text. To move towards a better understanding of tropical peatland dynamics and extant globally, papers like Mitchell et al. are exactly what our field requires. I strongly recommend publication, pending appropriate modifications. This is thorough and impressive work that makes several important contributions to the discipline and will serve as a foundation for understanding carbon sequestration and transformation in tropical peatlands.

We thank reviewer #1 for their encouragement and constructive comments.

Major comments
1. Figures and redundancy
Almost all figures (excluding Fig. 11) are too low resolution for readers and contain too small text features (axis labels, legends etc.). Furthermore, many figures convey several iterations of the same data (see minor comments on figures) which is unnecessary and inflates the

number of figures and panels to the detriment of the reader and dilutes the impact of the data. The same applies to the main text, the authors present different versions of the same data, which should be presented in a single succinct section, see minor comments. I would encourage the authors to convey their story in less figures and strategically choose and incorporate data to highlight. Currently the text and figures do not easily link. The minor comments on the figures and text suggest several alternations that could aid the reader.

Agreed. We address the issue of many figures showing different versions of the same data in detail in the sections below. As for figure resolution, it could be an issue pertaining to the PDF that is created for reviewers, though we will make sure that all our figures are all saved and uploaded at 600dpi.

2. Structuring of results and discussion

Currently, the manuscript contains discussion in the results and results in the discussion (see minor comments for specifics). When splitting actual results and discussion, I would argue that that balance is off. The majority of the manuscript is outlining results (sometimes redundantly), and the manuscript could reach higher impact if focusing more on the wider context of tropical peatlands, drawing from other studies and bringing the story back to some of the larger themes outlined in the introduction.

Agreed. We address the issue of redundant and/or misplaced information between Results and Discussion by streamlining and re-combining those 2 sections (and using sub-section titles that help bring the data and interpretation together + help integrate our findings in a broader context). Details are found in our replies below.

3. Clarification on FTIR-processing methodology

Does the author utilize the R-script published in Hodgkins et al. (2018) or is it a custom iteration? Are the dotted lines in Fig. A1 the peak heights as included in the study? If so, this would be a different interpretation than the approach presented in Hodgkins et al. (2018) as it is not baseline corrected per included peak height. I question this approach, especially for the aliphatics. In Figure A1, you can see the that the aliphatic peaks are situated on a larger peak of unidentified absorbance. Therefore, the signal of this broader peak would be added to the interpreted signal of the aliphatics. The advice in Hodgkins et al. (2018) is to use the baseline-corrected peak heights normalized to spectral area, so I wonder why the authors have chosen a different approach. Does the relative abundance refer to the peak height post-correction? Overall, I would advise to expand the method paragraph on FTIR to aid reproducibility and publishing the custom R-script with the paper. It is difficult for me to assess which processing the authors have actually conducted. I would also advise to employ the R-script from Hodgkins et al. (2018), since the study publishes a transfer function that allows for %carbohydrates and %Klason lignin determination. This would help contextualize the results as currently any change seems relative and cannot be easily compared to the wider literature. I am aware such transfer function for the aliphatics and acids does not exists, therefore the current approach is (besides the uncertainties surrounding base-line correction) acceptable. Furthermore, the manuscript often highlights the role of silicate

mineral interference to overinflate the proportion of carbohydrates in FTIR. This is a known effect and flagged in Hodgkins et al. (2018) and the pipeline includes a test (norm.silicate780) to assess whether or not data points need to be discarded due to silicate mineral interference. Perhaps it is possible for the authors to engage a similar method? To construct a threshold after which we should not overinterpret the carbohydrate signal. The relative abundance concept is also not entirely clear to me. Would the overinflation of carbohydrates indirectly cause a decrease in aromatics due to a normalization step before? If this is the case, any datapoint with enough evidence for silicate interference should be presented separately.

Yes, we use the excellent R-script published in Hodgkins et al. (2018). The difference is that we manually corrected the baseline of each FTIR spectrum prior to applying the R-script, as specified in lines 235-236 of our original manuscript. With the baseline corrected only in regions of the spectra that lack features arising from bond vibrations in the sample, the raw peak heights normalized by spectral area provide the most unadulterated measure of absorbance at a given frequency, providing an inclusive maximum of the Gaussian/Lorentzian function giving rise to that feature in the spectrum. This is the most analytically sound way to compare the intensities of different features both within a given spectrum and across spectra from a sample set, by measuring their respective intensities from the same baseline. Otherwise, drawing a somewhat arbitrary line tangent to the bottom of a perceived valley gives an equally arbitrary measure of absorbance intensity of a given band, with a baseline that varies with the intensities of any overlapping peaks. Neither method entirely avoids contribution from overlapping signals, but measuring absorbance intensity from the consistently zeroed out baseline provides the best means of standardization across spectra from different samples, as the baseline will not vary with the absorbance of other, non-target components. We added a clarifying message to make our analytical procedure clear.

It is true that with overlapping bond vibrations in any FTIR spectrum, there may be contributions from non-target bonds. However, it is a particular problem for the C-O band, which overlaps substantially with the Si-O band. Thus, we have reevaluated the consideration of carbohydrate relative abundances in this manuscript. The relevant changes are addressed in more detail below.

We refrained from applying Hodgkins's transfer function because we did not reproduce their wet chemistry experiments on our instrument. We use the peak heights as measurements of relative abundances to allow for comparison of trends within our dataset. We added a sentence to explain this choice.

4. Silicate interference on FTIR results

The influence of silicate mineral interference in the carbohydrate peak in the FTIR is a reoccurring theme throughout the paper, discussed in at least four separate paragraphs. I would encourage the authors to deal with this earlier, dedicating a section fully on this

phenomenon. Especially, because it is such an important theme, perhaps it is more accurate to detach "carbohydrate" from the 1050 cm-1 peak height and refer to the FTIR signal as the 1050 cm-1 band instead and add the interpretational link to carbohydrates where it is appropriate. (I would suggest to alter this in all figures as well). If the silicate interference (through relative abundance calculations) also affects the other FTIR signal (which I cannot currently decipher based on the methods) I would advise to use the wave lengths instead of the interpretation throughout the manuscript as well. The paper could benefit especially from a dedicated section to convince me that the authors are not solely interpreting silicate minerals proportions instead of the actual peat organic matter. Could this interference be deconvolved semi-quantitively? Although this has not been attempted before (as far as I know) and papers usually disqualify samples. However, the latter approach would be a shame for such an amazing dataset.

It would be a potentially revealing analytical challenge to deconvolve the band at 1050 cm^{-1} into its component C-O and Si-O functions. However, for the purposes of this manuscript, we have chosen to relabel the carbohydrate data as suggested by the reviewer, to emphasize that the relative abundances indicated by the intensities measured at 1050 cm^{-1} reflect silicate (kaolinite) contributions in addition to carbohydrates. We have added a statement to the Methods section to this effect. We have also deleted the carbohydrate/aromatic ratio plots from the figures, as this ratio does not provide meaningful information.

We have also revised the text throughout the manuscript to de-emphasize any assertions regarding the carbohydrate data given the interference from silicates in so many of our samples. Revised segments can be found throughout the revised document.

In fact, we have decided to use the FTIR data from our sample set as a kind of "cautionary tale" to demonstrate how extreme care must be applied in the interpretation of FTIR spectra from peat samples with high clay mineral content to ensure that the band at ~1050 cm^{-1} is not taken to represent carbohydrates alone when there are clay minerals present. We believe this is an important caveat for the peat geochemistry community to be more aware of, and we emphasize this in the revised version.

Given these comments, I think that minor revisions would result in a fine manuscript of value to the community. However, I have suggested major revisions, as these (especially a stronger treatment of the FTIR data and greater discussion relative to the results) could yield a truly foundational paper.

Thank you for all the constructive feedback!

Minor comments text:

Line 9: sentence does not flow, perhaps remove "these ecosystems"

Agreed; updated.

Line 10/277/279/283/347/351: number ranges work better with an en-dash: "100–300 gigatons"

Updated.

Line 11/12: Split the sentence (remove : , replace by full stop), ensure the list does not include "Panamerican region"

Updated.

Line 22: replace "of" with "on"

Updated.

Line 28: "multifold and" is redundant

Updated.

Line 29: Replace "/" with "and"

Updated.

Line 30/31/32: Is there basis to assume that Panamerican tropical peat is essentially different? Could the author clarify and reference why estimating carbon density on extra-tropical region is problematic. Same is true for the next sentence. Why would Southeast Asian peatlands have essentially different carbon densities? To a certain extent we always extrapolate data for carbon estimations, why would geographic region matter more than, for example, vegetation type?

The point we argue in the text is that using peat data from other regions is "adding unquantifiable uncertainties" (original line 31) to any assessment of the Panamerican peatlands. This is because peatlands worldwide are composed of different plant assemblages (i.e., the inputs to the peat column are different), those peat soils are affected by different temperature and hydrological conditions, and the organic material is decomposed by different fungal and microbial communities (i.e., the outputs to the peat column are also different). It is fair to assume that altogether, those differences may lead to regional variations.

We made an update to the text to better reflect what we meant: "…, adding unquantifiable uncertainties that pertain to differences in vegetation assemblages, fungal and microbial communities, and environmental conditions."

Line 36: Any references for this paradigm? I suppose that "tropical" depends on your definition. Lowland (climatically) tropical, yes, but high-elevation tropical, perhaps not so much. Perhaps the author can define/clarify its meaning for tropical earlier, especially given that the study includes some high-elevation sites.

Updated. We now specify that the paradigm pertains to the tropical rainforest biome.

Line 38: replace "special" with "specific"

Done.

Line 43: Please reference mechanism (2) as well

Done.

Line 44: Please clarify the wording; are stems, branches, wood not composed of recalcitrant compounds? Perhaps "woody tissues" or "lignin-rich tissues" can replace this summation. I would also clarify that this is relatively recalcitrant to tissues associated with northern peat accumulation.

We added "relatively" and replaced the summation with "lignin-rich tissues".

Line 47: Are there any other studies that have implied this as well?

Maybe, but we are already citing 7 studies at the end of the sentence (spanning from 1997 to 2018 and from multiple regions of the globe), which we think is enough.

Line 49-62: The purpose of this paragraph seems to be to cement the lack of field-based carbon storage dynamic peat studies in Central America. Perhaps introducing a world map insert into figure 1 might be more powerful displaying the coverage of field studies? Perhaps then the paragraph can be consolidated more effectively.

While we agree that a global peatland map is often a good figure to have and would make the point of poor representation of LAC, we choose to not include one because we argue that the data gap is clearly explained in the introduction. Figures are best used to show our results. Also, there currently exist many global-scale maps that do suggest the presence of peat in Costa Rica (and Mesoamerica more broadly), but none of those map products have been ground-truthed, so we would rather not show them in the context of our study.

Line 64/69: spell out “nine”

Done.

Line 66: remove “, if any”

Done.

Line 68: The line “We also discuss the differences that were found across our sites” is repetition of the earlier defined goal of “temporal differences” and therefore redundant.

Deleted.

Line 70: “can be used” seems cryptic, will the author do it? Perhaps remove?

Haha! Removed as this was indeed useless in the context of the sentence.

Line 71-73: switch to active voice for consistency with the previous sentences.

This sentence is already using active voice: “… we identify shifts in organic matter quality…”.

Line 74-76: These hypotheses feel arbitrary, given that no hypotheses are presented for any other objectives. If included, the literature can be used to back up the hypotheses as both have been observed before.

That’s a good point. Those hypotheses are introduced and discussed later on (in the Results and Discussion), so we have removed them from the Introduction.

Line 75-76: Are roots associated with a high carbohydrate relative abundance? As far as I understand, aerobic degradation is fairly ambivalent to aromatic/carbohydrate proportions, therefore bypassed roots should have similar carbohydrate-aromatic ratios to extensively aerobically degraded peat litter, however roots are often “woody tissue” and may have higher recalcitrance?

The sentence in question was removed (see previous comment/reply) so this comment does no longer apply.

Line 82: As peatlands cover 3% of the worlds surface; Wouldn’t that make Costa Rica a “peat-average” country?

Updated to “peat-bearing country”.

Line 82: remove “With that said, “

Done.

Line 89: Could the author clarify if these peat types are representative for Costa Rica's peatland cover

We added the following: "as well as regional representativity of Costa Rica's peatland cover".

Line 102: Please include the acronyms in the photo panel description too

Done.

Line 106-107: Perhaps it is helpful to introduce the "full core"/"bottom core" concept here already, since it is included in the table

Good idea. We added the following to the caption of Table 1: "Full cores consist of the entire peat column, from the peatland surface to the peat-to-mineral interface. Bottom cores consist of the deepest 50-cm drive that was collected at the site and they include the peat-to-mineral interface. Those bottom sediment cores are primarily used for basal age determination".

Table 1: Perhaps including temperature range and precipitation is more informative than elevation given the scope? Could the authors reorder the table to the site description order?

Table 1 has been updated as suggested by the reviewer.

Line 112/123/134/153: Full stop after (n=x). i.e. "(4) Montane peatbogs (n=2). These sites…"

Updated.

Line 112: Be consistent with using either "these" or "those" describing field sites

We have revised this section and improved our consistency (using "these").

Line 119: "The" instead of "Those"

Updated.

Line 120: Could the authors briefly describe the relevant information that these studies contain? Equivalent to Lines 131-132

Updated as follows: "… the exception of prospective work in the 1980s and 1990s that provided preliminary peat depth and calorific values and was aimed at assessing the potential economic development of these peat deposits as fuel resources".

Line 123: replace "Mangroves" with "Mangrove"

Updated.

Line 123: Please approximate the distance (e.g. ~5m)

Updated to: "… ~ 100 meters away…"

Line 164: The information in section 2.3 might be presented better first (as section 2.1)?

Done.

Line 184: Please introduce the acronym LOI earlier in the manuscript

The acronym is now presented.

Line 198-199: Perhaps this statement is better suited in the results?

Removed the sentence in question and moved it to the Results section.

Line 263: Is this abovementioned mineral interval the peat basement? Could the author clarify which mineral interval? I do not observe it in Fig. 2, so I assume it is omitted from the graph?

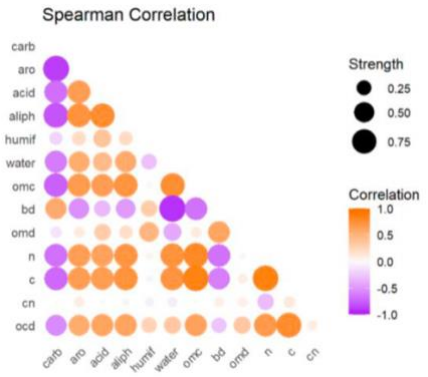
Correct. We clarify as follows: "with a sharp increase that corresponds to the abovementioned mineral interval found underneath the peat (not shown on Fig.)".

Line 263-266: Since these are relative abundances, what kind of variations are low and which ones are high? Could the author clarify what variations are significant? Furthermore, the trends in %Carbon in Fig. 2 seem to correlate well with both Carbohydrate and Aromatic, where a low %Carbon corresponds to a high proportion of Carbohydrates and Aromatics being a perfect anti-correlation to that. Especially in the 15-35 interval the %OM, %C, carb and aro show the same signal. I wonder if this could also be mineral interference as the author invokes in later sites, especially since the %C is rather low, likely the rest of the material would contain some silicates.

The relative abundances are derived from peak heights from baseline-corrected spectra normalized by spectral area. They are not absolute concentration measurements, and as such they have arbitrary units. To address this, we have removed the numbers from the x-axes in the relative abundance depth profiles in each of the figures, and we have

relabeled the axes to indicate that the units are arbitrary. We look therefore at overall trends in abundance with depth rather than claiming that certain abundances are high vs. low (we have made significant changes in the text of the manuscript throughout the Results and Discussion to avoid overinterpreting the relative abundance data).

Indeed, the correlations of aromatic abundance and %C and OM are very strong, as shown in Fig. A6 (original version), which we now brought into the main document.



As we mentioned above, we have made significant changes here and throughout the manuscript to address the issue of mineral interference in the FTIR spectra.

Line 275: This sentence is redundant, perhaps integrate the next 2 sentences. Also, this seems like a point for in the discussion instead?

Agreed. The text: "As a point of reference, other mangrove studies in the region have described these ecosystems as mineral-rich, with 5-8% OM (Rodgers and Horn, 1996). In contrast, our RM site harbors OM% in the order of 25–71%" was removed and placed in the Results and Discussion section.

Line 280-285: Could the authors explain why the model discarded that reversal instead of the date at 45cm? Perhaps the authors could reword this section as currently the wording anthropomorphises the model and treats it as a black box (e.g. "deemed too young")

Updated to: "… but did not fit the rest of the curve per the principle of superposition and the assumption of a constant rate of accumulation used in the Bayesian approach".

Line 289: em dash instead of hyphen to separate the sentence

Done.

Line 297-300: These lines are better suited in the discussion of 3.2.1, where the authors could draw from all the sites, making the signposting here redundant

Agreed. Removed.

Line 313: The age model in figure 4 seems continuous with radiocarbon points in the indicated hiatus (Fig. 4A)? Could the author clarify?

We identify a probable hiatus, where the age-depth model presents an extremely slow slope, akin to a vertical line in panel 4a. The hiatus is recognized where a lot of time (thousands of years) is represented within a very small amount of peat (only a few cm). The idea is that the peatland might have accumulated much more peat during that time interval, but secondary decomposition processes due to natural and/or anthropogenic conditions taking place much later in time (e.g., drought, drainage, fire) may have led to a second wave of peat decomposition down into this older peat section. This is a recognized process that has been documented in peatlands from across the world (see Young et al. 2019 for examples). To reiterate, while the radiocarbon ages are continuous in the age-depth model, they are spanning an unusually long time period that is contained in very little peat (relatively speaking).

We have updated the original text as follows: "There is then a long-time interval (from 5500 to ~ 2500 cal. yr BP; from 75 to 55 cm) during which a two-order-of-magnitude slowdown in apparent rate of peat accumulation is recorded (0.007 cm/yr). This interval coincides with high bulk density values (> 0.4 g/cm3), humic peat (10 on the von Post scale), and peaks in aliphatics that suggest the presence of herbaceous peat. These conditions may indicate secondary decomposition processes (i.e., reactivation of diagenetic processes following a disturbance) and/or a drastic change in the net carbon balance of the peatland during that time interval that has resulted in little to no net peat accumulation for two millennia."

Line 317-323: Could the author perhaps restructure or swap out the interpretation with RM to introduce this earlier in the manuscript? However, the split peaks indicates that the silicate signal is equivalent to the carbohydrate peak, which it does not have to be to significantly alter the carbohydrate height due to the width of the peak. Could that the same mechanism drive most FTIR trends in RM and GAN as well, regardless of the bulk density trends?

We have restructured the text such that we now address the silicate overprint of the spectra in a single section rather than twice (for RM and MQ). As for the interpretation itself, we have modified the text throughout the manuscript to de-emphasize any assertions regarding the carbohydrate data given the interference from silicates in so many of our samples (especially in RM and MQ but in the others as well). There are 9 text updates related to this point in the revised document. We have also relabeled all of the figures to indicate that the carbohydrate signal includes contribution from silicates.

Line 327: Could the author add a reference that establishes a link between aliphatics and herbaceous peat?

Our previous work demonstrated positive correlations between herbaceous macrofossils and aromatic abundance in Patagonian peats (Leri et al. 2025). We also found similar correlations with herbaceous macrofossils and aliphatic abundance, although we did not go into that in detail in the previous paper. We now reference the aromatic-herbaceous correlation in the revised text.

Leri AC, Loisel J, Parke I, Pavia AP, Ravel B, Northrup P, Liu Y. 2025. Long-term peat organic matter stability influenced by botanical composition and oceanic bromide inputs. Geoderma. 464:117633.

Line 343: "shortest but longest" is confusing. Could the authors either remove the lead-in or specify their meaning e.g. "spatially shortest but temporally longest".

Ha! Definitely confusing. We updated to: "…is the shortest but oldest record…".

Line 343-344: Could the authors specify what observations lead to the interpretation of "appears to have started as a herbaceous-dominated peatland". This sentence could benefit from restructuring as the current 4 sub-sentences are not linked coherently.

Updated to: "The ecosystem started as a herbaceous-dominated peatland…".

Line 348-349: Could the authors specify at what depth ("top of profile")?

Added: "… towards the top of the profile (top ~ 20 cm)".

Line 349: remove "in fact"

Removed.

Line 349: Could the authors explain the two outliers in OM% at ~18 and ~75cm? (Fig. 5h). They do not seem represented in any other parameter.

We have no explanation for those 2 outliers.

Line 352-353: Could the authors reference and provide context for the link between aliphatics and herbaceous peat?

We have added a citation to our 2025 Geoderma paper in the updated version. We found the aliph-herb correlation in Patagonian peat but did not show it explicitly in that paper, instead going into detail about the aromatic-herbaceous correlation, which is essentially similar.

Line 354: Could the authors briefly explain what chemical/spectral reason is behind this feature?

In the spectra from samples at the surface of the core, the two aliphatic C-H bands at 2850 and 2920 cm^{-1} are less resolved than in all the other samples (Fig. A5, top sample), likely indicating comparatively higher content of carbohydrates and other labile organic components that become degraded further down the core. We have modified the lines in question to clarify this feature.

Line 367-368: While I am not certain what transformations the authors applied to the spectral data, I wonder if the normalization step essentially makes carbohydrates and aromatics into a ratio of each other, therefore these correlations are autogenic.

We believe that the methodological clarifications we made above address this concern. The correlations are not autogenic.

Line 369-370: Is this correlation positive or negative? I wonder if it is actually the dearth of silicates that drives this correlation.

We have modified the language here to clarify. The directionality of each correlation is also laid out in Fig. A6.

Line 372-374: This interpretation is likely, however, the inverse relationship can also indicate the proportion of silicate minerals increasing instead. It would be excellent if the authors could present a signal independent of the proportion of the 1050cm-1 peak heights.

We have made the language much more careful here, as we cannot entirely rule out silicate contribution to the signal @ 1050 cm-1.

Line 387-388: Could the authors refer to papers that state these mineral FTIR wavelenghts?

We added the following reference: Kloprogge JT. 2018. The Kaolin Group: Hydroxyl Groups. In: Kloprogge JT, Spectroscopic Methods in the Study of Kaolin Minerals and their Modifications. Springer Nature, pp. 41-96. https://link.springer.com/chapter/10.1007/978-3-030-02373-7_3

Line 388-390: Do the authors have any indication about the flooding frequency or sediment load of the floodings? The %OM still suggests rather minor flooding/sediment loads otherwise the peatland would not incept?

Unfortunately, we do not have information on flooding at those sites. We agree with the reviewer that flooding must be relatively minor given the high OM% and the presence of peat.

Line 382-392: This section seems to be a repeat of Line 317-323 with added markers for mineral interference and discussion. Could the author restructure the manuscript to deal with

the silicate mineral effect in one sharply written section and contextualize the interpretation? Perhaps prior to the site-specific interpretation. (as section 3.1 instead). This would enable the reader to understand the nuanced FTIR signals earlier

Done. Thank you for this comment, it helped limit repetition between different sections. We now clearly describe the silicate mineral effect on FTIR results in a single part of the Results.

Line 397-398: This lead-in seems not to link to the rest of the paragraph? Perhaps the author can justify their use of ordination to disentangle inter site variability.

Rewritten as follows: “Peat composition and properties vary between and across the four sites”.

Line 402-403: Is this information not found in Fig. 8? If this figure is more key that figure 8, perhaps these figures can swap?

There was a typo in the main text; we meant to refer to the different panels of Fig. 8 but instead referred to Fig A7 (because we had originally placed some of those images as Annexes). Updated.

Line 409: Could the authors do a statistical test to prove this?

The correlation matrix, which was shown in the Appendix originally but that is now included in the main text shows the statistical relationships.

Line 411-414: See comment for Line 382-392; it would be beneficial to condense all the lines of evidence for silicate mineral interference in one section.

Addressed.

Line 418: Does the combination of peat type and site not encompass the whole dataset? It is difficult to attribute these properties to peat type and site if those overlap significantly. There could be many covariance with uncharacterized parameters. Perhaps the authors could just highlight this.

Agreed. We added: “With that said, there could be covariance with uncharacterized parameters within those sites.”

Line 420-423: Could the author show the statistical tests for these?

The statistical relationship that corresponds to this statement: “Dispersion was shown to be significantly different across sites, with the mangrove (RM) and the riverine (MQ)

sites displaying greater heterogeneity, but not significantly different among peat types." is shown in in the Appendix (Table A). We added a reference to it.

Line 441: "tend to cluster" can the author show this statistically?

We modified to "… and cluster on the basis of peatland type / geomorphological setting (Fig. 2)". This assertion is not based on statistics, but rather on visual analysis of the diagram (Fig. 2), with clear data clusters. Note that this section has been moved to the start of the Results section, as suggested by this reviewer in the next comment.

Line 440-446: This section contains results and should be moved to the results

Updated.

Line 447: ", a common theme …" to "which is a common theme"

Updated.

Line 448: I would argue that elevation cannot be a driver of peatland inception (peat is ambivalent to vertical location), it is the climatic conditions associated with elevation that are the main drivers. Same goes for coastal sites, but that has a different driver.

Agreed, but temperature in this case is unlikely to be driving peat formation. Elevation is used as a proxy for several conditions that have enabled early peat formation. The same goes with "proximity to the coast" as a proxy for sea-level stabilization, climate, geo(morpho)logy, etc. We maintain that 'elevation' and 'proximity to the coast' best encompass the main differences in our peat-forming environments.

For accuracy, we updated the text as follows: "Overall, the spatiotemporal pattern of peat inception primarily relates to landscape-scale conditions, with high-elevation sites being the oldest and coastal sites being the youngest".

Line 452: Could the author clarify what "base level" entails?

Base level refers to a horizontal line where water velocity decreases, causing sediment deposition rather than erosion. Fluctuating base levels activate rivers and makes them erode and accumulate; those conditions are not conducive of peat formation. The latter requires stable water conditions.

We already explain this in the main text but now added a few words for clarity: "… linked to increased stability in base level, which combines stable sea level conditions as well as lowered variability in freshwater and sediment inputs from the inland rivers that feed the coastal lowlands, providing conditions that support sediment deposition and peat formation".

Line 453-454: Does the author have any references to back up this hypothetical chain of events?

Added: "(Dommain et al. 2014)".

Line 457: "it seems [sites] lay within a lowland area that used to be flooded" Could the author ensure and remove the doubt from the sentence?

Good idea. We removed "it seems that".

Line 458: Does the author mean Fig. 1?

Thanks for catching the typo. Updated to Fig. 1.

Line 470-497: This section contained new data and contains no citations at all. I would urge the authors to either move this to the results and place these results in relevant literature.

Agreed. We moved those paragraphs in the Results, in a section dedicated to the 'bottom cores' / peatland inception (section 3.1).

Line 470-483: Should be presented in the results, perhaps a section dedicated to the bottom cores would be valuable?

Agreed and done (see comment above).

Line 487: "that is an order …" replace by "which is an order …"

Replaced.

Line 485-490: This information was presented in the results and is repetition here. If the authors want to reinforce the uniqueness of the fast and slow accumulations, perhaps drawing from wider literature might make a stronger argument.

Agreed and deleted.

Line 490-497: Every sentence is this section would benefit from relevant citations. I question most statements made in this paragraph. Perhaps the author could present a structured approach to the factors that offset the balance of accumulation and degradation with citations

No longer relevant, as this info has now been deleted (see comment above).

Line 494: Remove: "with all that said"

No longer relevant, as this info has now been deleted (see comment above).

Line 498-512: Move to the results. I encourage the authors to condense all similar statistical tests in one comprehensive section in the results.

Moved to Results (section 3.1).

Line 507: Could the authors not conduct these tests in a smaller subset?

We are not sure what the reviewer means. We conducted statistical tests to test the effect of peat type (herbaceous, ligneous, bryophyte) on the peat properties (bulk density, organic matter content, etc.). The goal is to test the whole set, not subsets. Left as it.

Line 510: Could the authors further investigate this relationship, as the slope is the same, but the intercept might not be. Why does this relationship does not hold in Sphagnum peat?

Good point! The intercepts are indeed different: ~ 9 in Costa Rica (our new data) vs. ~ 25 in northern peatlands.

We added this finding to the main text as follows: "The slope (0.51) of the linear regression indicates that %C is about half the OM content, similar as the relationship found in the northern peatland database (0.49; Loisel et al. 2014), supporting the idea that OM content and %C are closely related in peatlands. With that said, the intercepts differ between this new dataset and the one from the northern peatland database (9 vs. 25, respectively), pointing to a generally higher C% in northern peatlands".

Table 3: This table is redundant with the box plots presented in Fig. 10. I would encourage the authors to omit this table or move to SI

While it is true that the dataset presented in the Table vs. Figure is the same, we argue that the Table format is quite useful to the readership, who can quickly access those data rather than having to read the whole text. Left as is.

Line 521-522: Could the author cite a study where this data is "directly usabl[e]" as an example?

Added: "…with direct usability for regional carbon analysis and modeling (e.g., Dehaen et al., 2025; Swails et al., 2026; Fig. 12)".

Line 524-525: Does the author have suggestions why the Costa Rican bryophyte peat has a lower %OM?

This observation applies to the northern *Sphagnum* peat as well. We are not sure why, but it may relate to the botanical properties of *Sphagnum*.

Line 525-538: In this section, the authors compare every parameter to northern peats. I would encourage the authors to include the northern data in Fig. 12 and not highlight every difference or commonality, instead find 2-4 common themes to highlight, discuss, and place in wider literature context.

We added the northern values to the Table and removed the least important comparisons from the text.

Line 544: I would argue that the approach in this study is "simple" as well given the uncertainties in peatland depths or spatial transferability of the parameters. Perhaps more powerful would be to highlight strengths of this study's carbon store estimations.

We improved this section and highlight the benefit of our new dataset as follows: "An important improvement brought about by our new dataset is that previous peat-carbon store estimates often use 0.1 g/cm3 as their bulk density in combination with an assumed 50% carbon content, yielding an organic carbon density of 0.05 gC/cm3. The results for Costa Rica suggest an organic carbon density of 0.072 gC/cm3 (0.24 g/cm3 * 30%), about 30% greater than the value previously used in carbon assessment (e.g., Yu et al., 2010)".

Line 558-559: Since this study does currently contain the best available carbon estimations, I would encourage the authors to contextualize the carbon store (compare to other estimations) and provide a probabilistic range to their estimation as well. These could be helpful to stir further research and to engage to a wider (policy-focussed) audience. Overall, this section could be expanded for higher impact.

Excellent idea! We added a short paragraph that reads: "At the plot scale, Costa Rican peatlands were found to store 1080 Mg C/ha on average (150cm x 0.17 g/cm3 x 30%C), which is a massive concentration of belowground carbon that also accumulated relatively quickly (less than 1000 years in the case of the coastal peat swamp sites). The riverine site (MQ) was characterized by the greatest carbon stock (1652 Mg C/ha), followed by the peat-rich mangrove (RM; 1164 Mg C/ha), the (young) peat swamp (GAN; 706 Mg C/ha) and the montane peat bog (BOG70; 646 Mg C/ha). The averaged value of 1080 Mg C/ha is also 5-10 times greater than the typical carbon stock of a tropical rainforest (e.g., Baker et al., 2002; Malhi et al., 2009). When compared to other peatland carbon stores, the Costa Rican peatlands rival with the Brazilian veredas (~1200 MgC/ha; Verona et al., 2026), the Peruvian Amazon lowland peat swamp forests (~234-1067 MgC/ha; Bhomia et al., 2019) peatland pole forests (~1133 MgC/ha; Honorio Coronado et al., 2021), and open peatlands (~ 655 MgC/ha; Draper et al., 2014). Similar results have been reported from the Colombian lowland peatlands (490-1230 MgC/ha; Winton et al., 2025)."

Line 571: Does the author mean figure 1?

Yes, thank you. Updated.

Line 573: I would encourage the author to incorporate this caveat earlier and end the paragraph on a positive note.

Updated. That last sentence ("Unfortunately, there is a lack of paleodata from the lake to corroborate with the peat core") was removed since this information is already mentioned in the text beforehand.

Line 576: Remove "The montane peatland presented a different story"

Removed.

Line 583: Based on the size and extent of the peatland, I would not presume to pick up any palynological changes in a regional lake. What would the authors most likely speculation be? Could it be an autogenic development? (i.e. fen-bog transition).

We expanded this section to present potential reasons for the recent *Sphagnum* appearance at our montane site: " A transition from herbaceous-dominated fen to moss-dominated bog (or poor fen) only requires the peatland surface to become isolated from the groundwater (e.g., Hughes & Barber, 2003). It is possible that regional changes in hydroclimate such as an increase in rainfall or fog have allowed for *Sphagnum* peat to colonize this site where it quickly developed dense moss carpets that promote their own sustenance (van Breemen, 1995). Alternatively, we may be witnessing the autogenic development of a rain-fed (or fog-fed) system (dominated by *Sphagnum* moss) that is transitioning away from a groundwater-fed herbaceous through vertical peat accretion (Hughes, 2000)".

Line 591: Remove "The effect of peat type on the nature of the peat carbon compounds is worth discussing"

Done.

Line 593: Could the author clarify the challenge? FTIR generally functions under any level of humification

Removed. This sentence was an unclear and out of place.

Line 595-596: Or the silicate minerals overprinted the carbohydrates, which pushed down the relative abundances of the aromatics and aliphatics. Recalcitrance is also not solely controlling organic matter accumulation, I would argue.

We have softened the language in the revised version.

Line 598: Remove "The anticipated effect of depth (and age) on the carbon compounds was not straightforward"

Done.

Line 604-606: As the manuscript currently stands, I would argue this is the main factor controlling all FTIR-based signals, not the peat OM.

We believe it makes significant contributions to the signal @ 1050 cm-1, rendering the "carbohydrate" measurement problematic and often uninterpretable. We have revised this paragraph to reflect this reality.

Line 608-615: Do the authors have any data-driven insights into this mechanism? If not, I would encourage the authors to omit this paragraph.

Agreed. It is all speculation. Removed.

Line 617: The authors referred to some peat sediment studies in the methods before. Is the word "systematic" the key word in this sentence?

Rephrased. The only other analyses of peat sediments performed in Costa Rica were palynological, this the authors were not interested in the peat in itself but rather in its pollen and spore content.

We rephrased as: "This study offers the first detailed analysis of peat sediments for Costa Rica".

Line 618-619: I did not extract the gravitas of this finding out of the main text, perhaps the authors could highlight this unique feature to aid readers.

In reorganizing and expanding the section on peat carbon stock (section 3.1.2), we believe this finding is more clearly presented. We also added the following words in the Conclusion sentence that the reviewer brought up: " A key finding is that bulk density values tend to be greater in tropical lowland peatlands (average = 0.24 g/cm3) than what is typically reported for extra-tropical peatlands (average = 0.1 g/cm3)".

Line 620: The word "mineralized" is confusing with the silicate mineral interference and OM remineralization discussed previous.

Replaced by "humified".

Line 622: This was discussed but not proven with data. Could the vegetation input be recalcitrant instead?

We are adding the following nuance: "… suggesting that rapid and intensive decomposition within the uppermost portion of the peat profile can lead to long-term carbon storage; we note however that the recalcitrance of the parent plant material remains to be tested".

Line 623-624: The author earlier referred to this as a given fact (no citations), and here discussed as a major result. Perhaps I have missed the discussion about this?

We agree with the reviewer here. While aliphatic abundance appears in much greater abundances in herbaceous-dominated peatlands than the other types, suggesting that their presence could potentially be used as a signature for this herbaceous peat, which was also observed in Leri et al. 2025, we are not quite ready to make this claim. The sentence was thus removed.

Line 624-627: Could the author indicate the inception age range? (e.g. 500-10000 yrs or 102-104 yrs)

Done: "… a broad range of peatland inception ages were found across Costa Rica (~ 500 to 11,000 cal. yr BP)".

Line 628: Why does humification lead to linear models? Was this discussed previously?

We removed the following: "… likely due to the humified nature of peat".

Line 629-630: See previous comment, could the author strengthen this speculation with references earlier in the discussion?

This sentence was removed.

Minor comments figures:
Figure 1: Panel E is currently illegible. I suggest the author also label the included sites in panel E (MAN, BOG etc). Does this climate envelope also suggest there is a detailed probabilistic map for Costa Rica? Perhaps that would make for a better base for panel A? I suggest the author labels panels f, g, h, and i to include the acronym of the peatland as well. I would also suggest to remove the insert maps, as they currently do not convey extra information and perhaps include a global map to cement the dearth of field-based peat studies in the region. I also suggest the author to color-code each dot representing an included peat core by the four assigned peatland types in all panels? The additional suggestion would be to highlight the four sites that will feature with detailed geochemical characterization (e.g. bigger dots, thicker outline).

For all figures, we are increasing axis font size. For fig. 1, we also fulfilled almost all of the requests from the reviewer (labeled the sites on the climate space and on the photos, added the probabilistic map of Costa Rica, removed the insert maps, color-coded the dots on the map.

Figure 2, 3, 4, and 5: Could the author condense the panels in the figures to aid the visual appeal and avoid overcrowding? For example in Fig. 2; b,c,d have similar x-axis and can be color coded and integrated, same applies for the FTIR results (l,m,n). The figures could benefit from added grouping aids i.e. subtitle for l,m,n,o indicating they are from FTIR analysis, I,j,k from elemental analysis, b,c,d from macrofossil etc. These visual aids can enhance the readers ability to quickly interpret these complicated figures. Especially since every figure features a different selection of panels (i.e. figure 3 having mineral %). Alternatively, the authors could choose to intergrate several sites in a single panel, which would aid visual comparison. It is difficult for the reader to grasp the differences between sites because the data is split in several complicated figures. For the age model specifically, it would enhance the readers ability to compare the sites by plotting the 4 age models in the same plot.

We added subtitles as suggested. After condensing the panels by integrating the datasets, we found them to be too busy and reverted back to the original format.

Fig 6: This is an excellent figure, which enables direct comparison of the FTIR results between the sites. Could the authors clarify that the figure shows interpretated FTIR signals? i.e. change axis labels to "carbohydates from FTIR 1050 cm-1" or something similar. The authors could simplify figures 2,3,4, and 5 as the same data is now presented twice (Fig. 6b, excluding acids). Style suggestions: Could the authors assign a panel letter (a,b,c) to each individual plot? (i.e. 6a aromatics vs carbs, 6b aliphatics vs carbs etc.). Could the authors make the text and datapoints larger? It currently difficult to read. Could the authors enlarge the legend and add the full name of the site and peatland type as well.

It is true that the same data are presented twice, but they are presented in the 2 different contexts. In Figs 2-5, the FTIR data are compared with the plant macrofossils and other biological proxies from the individual sites, whereas in Fig. 6, trends in FTIR data are compared across sites. In Fig. 6, as suggested by the reviewer, we have added a panel letter to each individual plot. We also increase the datapoint and legend sizes. We added the full name of each site.

Fig 7. This figure is crucial for interpretation but would benefit from some visual enhancements. Could the author attach a higher resolution version and max out the signal on the Y-axis (no unit presented anyway, no need to)? Could the author integrate this with Fig. A1a, adding the bands for the compounds, perhaps even annotating the discussed mineral-related signals (kaolinite bands etc.)? The author could also add a shading band to indicate the entire range of spectra detected in each peatland so the reader does not necessarily has to look at Fig A2,A3, A4 and A5. While I personally like figures A1-A5, I would argue that they are redundant if the author publishes their raw spectra in the SI and enhances Figure 7 as

suggested. The figure caption could also be expanded to explain e.g. the missing Y-axis labels and other important details.

As suggested, we have optimized the y-axis (to max out the signal). We added Fig. 1A as an insert and annotated the mineral-related signals and updated the figure caption as such. The y-axis refers to absorbance and the units are arbitrary (label added). We will keep Figs. A2-A5 as the raw spectra are deemed important to present (in agreement with the reviewer).

Fig. 8: I would advise the authors to condense their figures. Panel a,b and c display the same information with 1 variable swapped out. It would enhance the understanding of the reader by omitting either depth or age (depending on which the story requires) and choosing perhaps inline labelling to convey this information. I.e. shape = Peat Type, colour = Core, label = Depth (cm). This would enable the plot to be larger and the text as well. Furthermore, please add a legend if the authors wants to minimize the text on the vectors (e.g. cn, should be described as C/N, aliph as FTIR-based aliphatics, no idea what n represents?, etc.). If keeping panel c, please add a unit to Age. Could the authors make the text and datapoints larger? It currently difficult to read. Could the authors enlarge the legend and add the full name of the site and peatland type as well. Then again, I would encourage the authors to condense the PCoA information in 1 panel.

Agreed. We are keeping panel A (the most important one) and are omitting panels B and C. We also added a legend. Datapoints are now enlarged. Full site names added.

Fig. 9: Given that all information of panel A is in panel B, I would suggest removing panel A. Could the authors add the site acronyms in panel B (labelled datapoints) and expand the legend. Could the authors also incorporate uncertainty (error bars) in the age dimension? Also, could the authors move the X-axis up to Age = 0 and condense the X-axis (range 0-250 cm).

Agreed. We have removed panel A (histograms). Site acronyms were added to panel B and the legend was expanded. Error bars were added to each data point. X- and Y-axes updated as suggested.

Fig. 10: Overall, I encourage the authors to omit this figure. If keeping: the histograms bins obscure the data. I would urge the authors to remove the histograms, present the box plots and remove the scatter plots as well. The scatter plots are redundant with the information presented in Fig 2, 3, 4, and 5. (although perhaps, with added lines, this figure could replace Fig. 2,3,4, and 5). Could the authors enlarge the legend and add the full name of the site and peatland type as well. Perhaps the authors could enlarge the x-axis label significantly to aid the reader.

Updated. We removed the histograms and scatter plots, and only show the box plots. The legend was enlarged and full names were added. Font size was increased along the axes and everywhere possible.

Fig 11: Excellent readability and the only graph for which the resolution is sufficient. Could the author present a transfer function, RMSE, r, n and specify the regression approach in the caption?

Good idea. The coefficient of variation (Spearman) and the number of data points were added to the graph.

Figure A1: Add references for the association of wavelength to compounds

Done. Hodgkins et al. 2018.

References:

Hodgkins, S. B., Richardson, C. J., Dommain, R., Wang, H., Glaser, P. H., Verbeke, B., Winkler, B. R., Cobb, A. R., Rich, V. I., Missilmani, M., Flanagan, N., Ho, M., Hoyt, A. M., Harvey, C. F., Vining, S. R., Hough, M. A., Moore, T. R., Richard, P. J. H., De La Cruz, F. B.,…Chanton, J. P. (2018). Tropical peatland carbon storage linked to global latitudinal trends in peat recalcitrance. Nature Communications, 9(1), 3640. https://doi.org/10.1038/s41467-018-06050-2.