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 #2

This paper describes 11 cores from four peatland types in Costa Rica (montane, alluvial, coastal palm swamp, and mangrove). The authors applied radiocarbon dating to construct an age-depth curve (BACON), loss-on-ignition, carbon and nitrogen content, and macrofossils as well as FTIR (Fourier-transform infrared spectroscopy). FTIR is increasingly used to evaluate dominant types of macromolecules in peat (cellulose-like vs lignin-like) as part of a way of assessing either inherent recalcitrance or degree of decomposition. The study provides correlation diagrams for mangrove, riverine and one of the bog sites. Basal radiocarbon dates ranged from about 11,000 to about 500 calibrated years before present, with oldest basal dates in the montane peatlands and the most recent inception date in the coast palm swamp near Limon (Caribbean coast).

The descriptive work here has unambiguous and lasting value and the manuscript therefore makes a worthy contribution to the literature. As the authors state, the peatlands of Central America remain poorly known so this information is fundamentally valuable. I do not see any critical flaws. The manuscript is currently somewhat difficult to read, partly because it does not quite follow the standard division into methods vs results and does not articulate an alternate structure. The sequencing is often surprising and makes the information hard to absorb and retain. My feeling is that the manuscript could be greatly improved by establishing a clear, possibly different, structure; and that the resulting text could and should also be significantly shortened. The text in most of the figures also needs to be larger to be easily legible at printed size. I have therefore recommended major revisions, although this would require no new analysis or reinterpretation. The bulk of my review is focused on helping to prepare the manuscript for publication.

Thank you for the constructive feedback and positive reception of our study! We address the points raised here in the point-by-point comments below.

General comments

1. The text through the Introduction is good. I finished the Introduction excited to read the rest of the paper.

Awesome!

2. The Introduction and Discussion are well grounded in the literature, notwithstanding some suggestions in the detailed comments below.

Noted.

3. The Introduction seems to half-commit to a hypothesis-testing framing regarding preservation of tropical peat. I think it would be better to abandon this because the hypothesis cannot really be tested with the data collected, which are anyway valuable.

Agreed, and done. Reviewer 1 had a similar comment. We removed the hypothesis pertaining to the preservation of peat, as it was the only part of the Intro with a hypothesis.

4. I suggest shortening the Methods and moving most of what is currently presented as site description to the Results, leaving in the Methods only the general approach to identifying and characterizing sites, and the technical methods (coring, FTIR, etc.).

We created a separate section for the Study area and field site descriptions, which we now present into distinct sub-sections to improve readability. The Methods section is thus reduced to presenting the sampling strategy (a short section) and the techniques employed.

5. The site descriptions would be easier to absorb if they were more structured. The observations are good but characteristics that are compared across sites would be better presented in a table, so that the description of each site in the text contains only essential qualitative information.

We split up the 4 peatland types into sub-sections. We already have a table that presents what we argue are the most important site features (peatland type, coordinates, peat depth, etc). Since those four peatland types are quite different, and the description we provide are aimed at presenting those differences but also explaining the geographic context of our own study sites, we don't think another table is needed.

6. Description of sites in section 2.2 is valuable but comes off as meandering and it is not clear what material is important or why. It also seems a mix of methods and results (but this depends partly on the positioning of the paper; I think actually these sections are part of the novelty and contribution?). Restructure this to make its purpose clear. Putting 2.3 before 2.2 might help?

Good idea. We moved what was called section 2.3 (general sampling approach) before the site description (which used to be section 2.2). This change helps the flow of information. As for the information within the site description section, we have restructured the information such that we provide (1) site-specific information about the coring sites, and (2) a somewhat broader overview of the existing knowledge of those study areas.

7. Motivation for methods generally unclear; why do the ANCOVA? Papers in top journals can provide useful examples; it is often useful to structure sections of the Methods like this: "To quantify / evaluate / assess X, we measured Y".

That is a good advice that we will retain for future publications, too. We have updated the start of each sub-section with an active 'motivation'. For example: "The estimate the degree of peat decomposition, we… "; "To determine peat bulk density, we used…"; "To characterize peat composition, we…".

8. The Results are essentially descriptive but not strongly propelled by a clear narrative motivation.

We have revamped the Results section by incorporating Discussion elements (as suggested by Reviewer 1). This new structure is likely to address this comment.

9. Interpretation is sprinkled into the Results in places; distinguish these clearly. I would reduce the number of adjectives in the results ("expected", "incongruent", ...) unless a point can be made that any reasonable person would find these results expected or incongruous, in which case it should be made clear why.

As mentioned before, we have revamped the Results and Discussion section (and combined them). This update results in a shorter and less repetitive paper.

10. Interpretation: what explanations can the authors suggest for the transition to mangrove at RM?

We agree that this is a counterintuitive and unusual sequence. Typically, one expected to see a progressively drier environment as the ecosystem evolves. With that said, a few sites are known to have transitioned from drier to wetter classes in the Caribbean (see Rabel et al. 2024 for a review). In many cases, those transitions were linked to local changes in hydrology, including flooding due to local land subsidence that was caused by earthquakes or large storms (hurricanes). Since we do not know the reason for our site, we chose to avoid speculating on this matter. For example: McCloskey TA, Liu KB. 2012. A sedimentary-based history of hurricane strikes on the southern Caribbean coast of Nicaragua. Quaternary Research 78(3):454-64.

11. I feel that the Discussion and Conclusions could be more concise and should be shortened significantly.

We have combined the Results and Discussion, making both sections less redundant (and overall shorter). The Conclusion section has been improved as well, though not shortened (it is only 15 lines).

Specific comments

1. P1, "ultimately to clarify the role of recalcitrant material in tropical peat accumulation.": If the results have in fact clarified the role of recalcitrant material, consider replacing this statement with what was learned? Otherwise, consider removing it?

Removed.

2. P1, "and is critical to advancing knowledge of the structure of tropical peatland systems": Consider removing this statement? The abstract is just as strong without it, and It is not obvious how it is justified by the findings of the study.

Done.

3. P2, "largest tropical peatland complexes in the world": Does the Gumbricht et al map suggest this? It clearly suggests a very large total peatland area in the neotropics, but does it suggest exceptionally large individual peatland complexes (rivaling those in the Congo)?

Yes, the Gumbricht et al. paper does suggest this for both wetland and peatland area. From their abstract: "Tropical and subtropical wetlands estimates reach 4.7 million km2 (Mkm2). In line with current understanding, the American continent is the major contributor (45%), and Brazil, with its Amazonian interfluvial region, contains the largest tropical wetland area (800,720 km2). [...] Unlike current understanding, our estimates suggest that South America and not Asia contributes the most to tropical peatland area and volume (ca. 44% for both) partly related to some yet unaccounted extended deep deposits but mainly to extended but shallow peat in the Amazon Basin. Brazil leads the peatland area and volume contribution". Left as is.

4. P2, "beyond Changuinola, there is little knowledge on where peat is found, how deep, old, and carbon-rich it might be, and what factors control long-term peat accumulation processes": Is this a fair statement in view of the following references? Already cited: Hastie, A., Honorio Coronado, E. N., Reyna, J., Mitchard, E. T. A., Åkesson, C. M., Baker, T. R., Cole, L. E. S., Oroche, César. J. C., Dargie, G., Dávila, N., De Grandi, E. C., Del Águila, J., Del Castillo Torres, D., De La Cruz Paiva, R., Draper, F. C., Flores, G., Grández, J., Hergoualc'h, K., Householder, J. E., et al. (2022). Risks to carbon storage from land-use change revealed by peat thickness maps of Peru. Nature Geoscience, 15(5), 369–374. https://doi.org/10.1038/s41561-022-00923-4 Draper, F. C., Roucoux, K. H., Lawson, I. T., Mitchard, E. T. A., Honorio Coronado, E. N., Lähteenoja, O., Torres Montenegro, L., Valderrama Sandoval, E., Zaráte, R., & Baker, T. R. (2014). The distribution and amount of

carbon in the largest peatland complex in Amazonia. Environmental Research Letters, 9(12), 124017. https://doi.org/10.1088/1748-9326/9/12/124017 Lawson et al. 2026 Not cited: Kelly, T. J., Lawson, I. T., Roucoux, K. H., Baker, T. R., & Honorio Coronado, E. N. (2020). Patterns and drivers of development in a west Amazonian peatland during the late Holocene. Quaternary Science Reviews, 230, 106168. https://doi.org/10.1016/j.quascirev.2020.106168 Kelly, T. J., Lawson, I. T., Roucoux, K. H., Baker, T. R., Jones, T. D., & Sanderson, N. K. (2017). The vegetation history of an Amazonian domed peatland. Palaeogeography, Palaeoclimatology, Palaeoecology, 468, 129–141. https://doi.org/10.1016/j.palaeo.2016.11.039

Yes, our statement that: "… there is little knowledge on where peat is found, how deep, old, and carbon-rich it might be, and what factors control long-term peat accumulation processes…" stands because it refers to Central America and the Caribbean. The references provided by the reviewer (Hastie et al. 2022; Draper et al. 2014; Lawson et al. 2026; Kelly et al. 2017, 2020) all come from the Peruvian Amazon.

5. P2, "(Rabel and Loisel, 2024": It is not clear without referring to Rabel and Loisel (2024) and Rabel et al (2025) how the work in those studies relates to the work here.

We added the following: "…(for a review, refer to Rabel and Loisel, 2024)". As for Rabel et al. (2025), it is not referenced within this section of the text, so we are omitting this comment.

7. P3, "the findings of this study shed light on biogeochemical processes that underpin the formation, development, and resilience of tropical peatland ecosystems, thus providing knowledge to scientists, landowners, managers, and policymakers.": I would put something concrete here, or cut it.

Removed.

8. P3, "For instance, the accumulation of recalcitrant compounds": There isn't enough context here for a reader to understand these statements or evaluate the results. Is more given in the discussion? Is it necessary to include this section at all? Would it make more sense to explore these questions in another paper that works with a larger dataset?

This sentence (which refers to a hypothesis) has been removed to comply (rightfully so) with a comment from reviewer 1. And in response to the comment about exploring these questions in another paper: yes, absolutely!

9. P4, ", (g) g , ( ) Photo credits: Hannah Mitchell and Patrick Campbell.": The pictures are great. It is helpful to have something or someone for scale if possible but appreciate that such photos may not be available.

Agreed. We will say that jungle pictures are particularly challenging to take, and those are the best ones that are available to us. We will keep in mind to add someone for scale in the future.

10. P4, "The country-wide Costa Rica map,": Would it be possible to also show the peat distribution as far as it is currently known (basis for the inset) in the map? I think readers may expect this.

Yes. The peatland map has been added to Fig. 1.

11. P4, "Inset map": The text in the inset will be too small to read comfortably at print size for many readers. Usually 6 pt, or at the very smallest, 5 pt is about as small as one can go and still reach most people.

Agreed. We have increase the font size in every figure.

12. P5, "the Atlantic Ocean": the Caribbean Sea, no?

Updated.

13. P5, "a few meters away": This surprised me! What keeps this small stream in its channel? Are there large mineral-soil levees on either side of the stream?

Us too! We amended this statement to "~ 100 m away". But still, the difference in soil substrate (and vegetation community) is astounding, and when combined with the fact that the red mangrove site was a palm swamp in the past (as revealed in the paleo record), this site's historical development is indeed intriguing. To answer the reviewer's question, there were no breaks or levees that could explain this difference. We speculate that a relatively recent rearrangement of Rio Gandoca (i.e., a change in meandering dynamics) might be responsible, but we do not know the cause of said change.

14. P5, "Table 1": This table looks like results; see general comments.

Agreed, but it is routine (in paleo papers) to show radiocarbon results in the Methods section. Left as is.

15. P6, "50 cm long,": The chamber is 50 cm long, but I think this description might confuse some people who are not familiar with the device (how can one core to 2 m with a 50 cm corer?). Rephrase?

Rephrased as: " … using a Russian-style peat borer equipped with a 50-cm long, 5-cm diameter chamber".

16. P8, "in some cases, such as for mangrove roots, the distinction was safe to make": How should we interpret the results? Should we consider samples specified as containing root material as definitely containing roots, and others as possibly containing roots, or?

Updated to: "… but in the case of mangrove roots, the distinction was safe to make".

17. P9, "distance": Bray-Curtis dissimilarity, technically not a distance (not a distance metric) in that it does not satisfy the triangle inequality (with three samples A, B, and C, the dissimilarity between A and B may be greater than the dissimilarity between A and B plus the dissimilarity between B and C).

Updated to: "… Bray-Curtis dissimilarity…".

18. P10, "Figure 2.": Text is again very small.

Updated.

19. P11, "We offer an explanation of this incongruent result in sect. 3.1.3 and 3.2.1.": Good

Thanks.

20. P13, "Repeated flooding may have increased clay content, confounding the carbohydrate signal, thereby also explaining why our high carbohydrate abundances are synchronous with high bulk density values, which is also a phenomenon that we observed at our mangrove site": Interpretation; try to reserve for Discussion to the extent possible.

This section has been updated and this comment does not apply anymore.

21. P13, "the record may have been altered,": What is meant by "the record may have been altered"? Is it hypothesized that the core was affected by some secondary processes?

We agree that this statement was misleading. Updated to: "There is then a long-time interval (from 5500 to ~ 2500 cal. yr BP; from 75 to 55 cm) during which a two-fold order-of-magnitude slowdown in apparent rate of peat accumulation is recorded (0.007 cm/yr)".

22. P15, "Age-depth g g p i i model,": Comment briefly in the caption on the error envelope near the top of the core, or refer to a specific section in the text?

Updated. The most likely reason is that the near modern age close to the core surface forces the age-depth curve to substantially change its accumulation rate and thus the slope of the relationship. In Bacon, a constant rate of peat accumulation is assumed, so the grey area is indicating a section of greater uncertainty. A note was added to the legend.

23. P16, "Figure 6": Series labels are obscure; expand in legend or in caption?

We have now spelled out the names of each site in the caption (rather than only presenting the acronyms in the figure legend).

24. P20, "base level,": Define

Updated to: "… the relatively recent inception and expansion of peatlands along the Caribbean coast (~1000 cal. yr BP) was probably 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…".

25. P20, "pre-colombian": pre-Colombian

Updated.

26. P21, "perpendicular to the ocean.": perpendicular to the shore?

Updated.

27. P22, "%C is about half the OM content, similar as the relationship found in the northern peatland database for non-Sphagnum peat (0.514; Loisel et al. 2014), supporting the idea that OM content and %C are closely related in peatlands across biomes": It would be good to make a comparison to other tropical peats as well. See, for example, Warren, M. W., Kauffman, J. B., Murdiyarso, D., Anshari, G., Hergoualc'h, K., Kurnianto, S., Purbopuspito, J., Gusmayanti, E., Afifudin, M., Rahajoe, J., Alhamd, L., Limin, S., & Iswandi, A. (2012). A cost-efficient method to assess carbon stocks in tropical peat soil. Biogeosciences, 9(11), 4477–4485. https://doi.org/10.5194/bg-9-4477-2012

The study mentioned by the reviewer relates bulk density and %C; we are relating %OM and %C. While both methods are acceptable in their own right, we cannot compare our findings directly with those from Warren et al. (2014). Left as is.

28. P24, "60% more dense than their northern counterparts": Compare also to bulk density from other tropical peatlands. See, for example, Shimada, S., Takahashi, H., Haraguchi, A., & Kaneko, M. (2001). The carbon content characteristics of tropical peats in Central Kalimantan, Indonesia: Estimating their spatial variability in density. Biogeochemistry, 53(3), 249–267. https://doi.org/10.1023/a:1010618807469. Consider distinguishing Sphagnum peats from woody peats in the comparison.

We added the following statement: “We also note that other authors have documented the higher density of tropical peatland soils before, including palm swamps in Colombia (0.19 g/cm3; Winton et al. 2025)”.

29. P27, "A key finding is that bulk density values tend to be greater in tropical lowland peatlands than what is typically reported for extra-tropical peatlands": This statement is too broad as a finding from a study of Costa Rican peatlands.

Updated to: “… bulk density values tend to be greater in Costa Rican peatlands (average = 0.24 g/cm3), and perhaps across tropical peatland lowlands, than what is typically reported for extra-tropical peatlands (average = 0.1 g/cm3)”.

30. P28, "spectra": spectrum

Updated.

31. P37, "The data will be made publicly available through Zenodo upon publication of this study.": Normally data are made available to reviewers at the review stage. This is not always an issue but in this case the figures are quite difficult to read.

Noted. The data will be made publicly available on Zenodo shortly.