nature portfolio

Peer Review File

Accretionary marsh levees act as emerging barriers to interior elevation gain

Corresponding Author: Dr Joshua Himmelstein

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Dr Himmelstein,

Your manuscript titled "Emergent edge microtopography as a barrier to platform accretion and marsh sustainability" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

Additionally, the following points need to be addressed in the revised manuscript:

* Please comment whether the results valid only for the specific setting of application or can they be extendible to a different tidal regimes and how.
* The two study sites (natural vs deforested) need to be comparable in term of landscape and hydrodynamics, for example. This requires further explanation by the authors.
* The language and phrasing require simplification of complex sentences.
* Clearly discuss the representativeness of the measurements, comparativeness of the two sites, and the accuracy of the measurements. What is the accuracy?
* Measurements and results should be fully capable of supporting the results, and not beyond it.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Claudia Zoccarato, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0002-3199-8681

Somaparna Ghosh, PhD
Associate Editor - Communications Earth & Environment
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Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This well-designed study makes an important contribution to understanding marsh dynamics under changing sediment regimes. The paired-watershed approach, combining remote sensing with high-resolution sediment cores, effectively demonstrates how increased sediment loading can paradoxically create prominent levees that inhibit interior marsh accretion.

The studied systems are microtidal but differ notably in tidal range (0.32 m deforested vs 0.52 m conserved). The stronger levee-interior decoupling observed at the deforested site could result from increased sediment loading, its lower tidal range making it more sensitive to topographic barriers, or both factors acting in combination. The Discussion should address this potential confound. Additionally, the authors should acknowledge that the mechanism of levees inhibiting interior accretion may be particularly pronounced in the most microtidal settings, where levee-interior elevation differences constitute a larger proportion of the tidal range. This would prevent over-generalization to macrotidal systems where spring-neap variability is substantially larger than typical levee heights.

Reviewer #2 (Remarks to the Author):

Interesting paper dealing with marsh accretion and edge processes. They make a compelling case that defenses to lessen or prevent marsh edge erosion may lead to enhanced levee formation that will reduce sedimentation rates and vertical accretion of interior marsh. Because of the importance of this finding with respect to proactive attempts to erect living shorelines and hardened structures, it would be beneficial to include a discussion of how their results might apply to marshes that experience substantially greater tidal ranges. While the marshlands of Louisiana have similar tidal ranges to NC, much of East Coast of the US and elsewhere in the world experience much larger tidal ranges. Would these trends still apply?

Much of the paper discusses different land-use classes and Deforested versus Conserved sites, but to the uninitiated, these classifications are somewhat abstract. Somewhere in the Introduction or elsewhere, it would be beneficial to the reader to have these classes well defined along with a description of their important attributes that are pertinent to this paper.

Labored writing, too many words to say simple statements. Examples given below:
Beyond watershed-derived inputs, sediment exchange at the marsh boundary itself can dominate local budgets. In marshes with migrating boundaries, lateral marsh edge erosion provides an additional source of sediment.
As efforts intensify to slow wetland degradation and coastal erosion through wave-dampening interventions such as living shorelines, the formation of prominent marsh levees due to ephemeral sediment loading may produce suboptimal edge micro-topographies that such
interventions would inadvertently preserve.
Instead of: “emergent microtopography,” how about some simple that many readers will immediately recognize: vertically-building levee, or accretionary levee

Some processes, rates, etc. are presented in the paper with little information provided as to how representative these measurements are. Also, we are not given the error bars associated with some of these measurements. For example:
1. How representative are your two transects? One transect for each of the Deforested and Conservative areas may be sufficient, but how do we know this?
2. Marsh edge erosion may provide sediment to the levee in the rollover process, but might the sediment also have come from the channel and flat during storm events?
3. How representative are the 210Pb age profiles. Are the computed rates affected by bioturbation?

Other Comments:
• At the Conserved site, why is there no signal of increased sedimentation caused by deforestation prior to 1959?
• Could you add a couple of sentences that explain how deforestation of a mostly flat landscape translates to increased sedimentation on the marsh.
• It would be useful to know more about the processes of sedimentation on the marsh surface and why the levees develop preferentially when suspended is more abundant. Are there different plant types, stem densities, difference in inundation durations, or is this strictly a product of decreased flow velocities related to grass baffling?
• How often and to what depth is the marsh platform inundated during neap and spring tides? This knowledge would help the reader evaluate levee development.
• Why are the scarps higher at the Conserved site and why are they eroding at a faster rate compared to the Deforested site?
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• One of the most important findings in paper receives very little attention. Every one of your six coring sites exhibits accretion rates that do not equate with the present rate of sea level rise. Forget about the influence of levees, your work

demonstrates that the marsh will be drowning. This point should more prominent.
• Missing the Hein et al 2024 reference.

Reviewer #3 (Remarks to the Author):

Review "Emergent edge microtopography as a barrier to platform accretion and marsh sustainability" submitted to Nature Communications earth and environment.

The manuscript describes a great effort in collecting and analysing sediment cores at two salt marsh study site, one site that is being conserved, while at the other site over the past decades deforestation took place. By selecting these two sites, driven by deforestation:
- sediment influx over the marsh scarp is assessed, and;
- the potentially imbalance between sediment accumulation at the marsh edge, compared with the middle and interior marsh is quantified.

Review highlights
I would like to start with congratulating the authors with collection of this long-term decadal dataset, where land use is combined with measured accumulation rates. I do understand the effort it takes to collect data in salt marshes. Apart from that I have three major points regarding the manuscript and storyline that is brought forward.
- by only reading the main body of text (without the methods section), it is difficult to understand the most important dynamics of the deforested and conserved site. This is necessary to compare both sites, e.g. relative elevation to the tide, tidal range, tidal inundation/frequency is not considered in the manuscript. When reading the methods section, it seems those parameters are not comparable between the two sites, which makes it difficult to understand the comparison and conclusions based on a comparison of both sites.
- I am not sure whether the deforested site can serve as proxy for sea level rise. Moreover, the results of the deforested site can also be interpret as follows: sediment load at the deforested site increased largely after 2001 at all measurement stations, although more at the marsh edge. Moreover, when comparing the deforested site with the conserved site, at all measurement stations SAR increased more at the deforested site. See also feedback regarding Lines 273-275.
- Finally, both in the introduction and in the discussion, it does not become fully clear for what salt marsh types and tidal characteristics (micro-, meso-, macrotidal) the conclusions are valid.
To me it seems the above points are very important for shaping the manuscript and storyline. In my opinion improvement is needed and perhaps a change of storyline to make the measurements and analysis support the conclusions and implications stated. More details regarding the above points, as well as other smaller comments can be found below.

Review details

Abstract
- Settings of the coastal landscape the MS is focusing on, is not fully clear to me in the abstract. Are the results shared valid for e.g. all marshes, marshes that have a cliff at the seaward edge, marshes with or without a intertidal flat at the seaward edge of the marsh? In addition is the analysis applicable to the full range of microtidal to macrotidal wetlands? In my opinion clarifying this is important, as it determines for which salt marsh and/or salt marsh development stage the analysis is relevant.

Introduction
- Lines 32-35: I don't think you can state that tidal marshes with low sediment inputs and narrow tidal ranges are landscapes in which to study the manifestations of sea-level-rise. As equal rates of transforming from vegetated marsh to bare mudflat, requires more sediment input under sea level rise compared without sea level rise. So, there will be more sediment trapped under sea-level-rise, resulting in different dynamics of the top layer of the substrate (e.g. seedling establishment and growth, consolidation processes, etc).
- Line 58: straight going to a description of levees, without any introduction, is confusing. This might need some introduction first, especially because it is presented as hypothesis ("may play a role in separating…")
- Lines 63-65: "prominent levees" and "larger levees" are difficult to put in context without any explanation or quantification.
- Lines 68-71: I fully agree that within certain timescales (i.e. decades), levees might grow with increasing sediment availability. However, 1) a certain type of landscape is assumed here, which is not clearly described, a marsh edge, i.e. an offset between vegetated marsh and bare tidal (or subtidal) area needs to be present. Either that can be a marsh cliff or a tidal channel directly neighboring a vegetated marsh platform. In addition, I think the hypothesis is not applicable for fringing marshes with a pioneer zone and a gradual transition from bare area to vegetated marsh; 2) in a healthy and dynamic salt marsh system, there are alternating phases between lateral expansion (i.e. growth) and erosion (e.g. retreat) of the marsh over decadal timescales[1,2]. Once in a retreating phase a levee might form at the salt marsh edge, because of the formation of a cliff. In this retreating phase, the fringing bare tidal flat allows for sediment accretion, resulting in the formation of a new pioneer zone. This process is actually partly described in lines 51-54. In this current hypothesis it is not clear how to distinguish between a marsh where a tipping point is reached and marsh expansion is not possible anymore, despite infilling/accretion of the area fronting the marsh, and a marsh with natural dynamics that is able to expand. So, some nuances and additional explanation is necessary.
- Lines 68-71: As mentioned in the introduction as focus site (deltaic wetlands of the Mississippi River), is this hypothesis

only valid for microtidal marshes?
- Lines 74-onwards: to me this comparison between the two sites feels a bit too simplistic and at least needs mentioning whether hydrodynamic energy and tides are similar, otherwise other mechanisms in addition to sediment loading might play a large role in sediment accretion of the marsh platform.

Results
- Lines 94-96: the manuscript states that bed resuspension depends on changes in water depth and hydrodynamic forcing. As I fully agree with that (also considering the importance of increased hydrodynamic energy in delivering sediment3), I am missing a hydrodynamic characterization of the two sites in the main body of text. To be able to only compare impact of sediment loads, inundation and hydrodynamic energy at both sites need to be rather similar.
- Figure 2: make sure fontsize is not too small
- Line 136: here “marsh scarp” is used, whereas at other locations, also for the definition of the measurements stations, “marsh edge” is used. In addition, it seems like “marsh edge” sometimes is also used to define the whole transect. Please be clear and consistent in using these definitions.
- Lines 173-176: it would be very insightful and helpful to understand dynamics, to show the differences in bed elevation of 1) the different measurement stations (edge, middle, interior) and 2) for both sites. To highlight how the offset in elevation changes between stations and sites. Can also be in supplementary materials.
- Figure 3: the white bars make it difficult to distinguish the three different symbols; to increase understanding the figure related to the results, perhaps add a horizontal line for the year 2001, as this is used in the manuscript as clear temporal boundary for analysis.
- Lines 209-211: The negative correlation between marsh edge and middle SARs at the deforested site are clearly shown, but that doesn’t necessarily imply a levee growth (edge) at the expense of accretion (middle). Figure 3a shows for the middle SAR around the year 2000 an initial increase after rapid deforestation, with a subsequent decrease and increase after increasing and decreasing edge SAR, respectively. So, sedimentation at the edge might be a driver. However, this explanation is too simplistic and should be nuanced, because 1) hydrodynamics are not considered here, perhaps energy and or inundation (sea-level-rise) changed over the same period and 2) at the deforested site the middle and interior SAR keep on average increasing, interestingly faster just after the peak of the SAR at the edge.
- Lines 217-218: In general SARs at the middle and interior increased on average more when comparing the deforested site with the conserved site. From figure 3a you could also conclude that after an initial change in SAR (year 2000), a new balance is settled (around year 2005) after the edge SAR peaked, the middle SAR recovered. So, the implications for sea-level-rise which is a by far slower process are not that clearly emerging from these results to me.

Discussion
- In general I miss for what specific marshes in terms of tidal characterization and types this study applies. And/or how these results transfer to meso- to macrotidal marshes, and marshes with more extensive mudflats where establishment of pioneer vegetation might be possible after increasing rates of SAR and further retreat of the current marsh edge. In my opinion this is essential for the study and for understanding and comparing dynamics at both sites.
- Figure 4: see comment above, how do tidal characteristics, relative elevation and tidal prism relate to these relations?
- Line 237: should “on” perhaps be “in”
- Lines 273-275: the results and translation of the results to sea-level-rise are not convincingly explaining how the levee is further build and the marsh interior drowns both by pulses of sediment load and sea-level-rise in my opinion. 1) for temporarily increased sediment loads, after a temporary decrease of mid/interior SAR, you might also say that accumulation there recovers and even increases due to the additional sediment available; 2) with SLR water levels might or might not exceed the elevation of the edge more often. Whether exceedance of this elevation increases or decreases really depends on accumulation at the edge. Figure 5 shows before 2001 an accumulation rate exceeding SLR at the edge (i.e. increasingly less inundation and potential accumulation at the interior), while after 2001 SAR is at the same level as SLR. So, this might imply an increased inundation and potential accumulation at the marsh interior when comparing before and after 2001 (without and with deforestation). Please don’t get me wrong, I am not in favor of deforestation, but I am not fully sure whether the measurements and results in the manuscript sufficiently support statements in the discussion.
- Lines 289-291: perhaps my fault, but I do not understand what is being said here.

Methods
- Lines 330 – 332: It is difficult to compare values between sites as the tidal range and also elevations of the cliff are not of equal height. Although I do agree that at the deforested site a clear pattern is visible after the period of rapid deforestation, the marsh scarp at the deforested site is lower, and tidal range is larger compared to the conserved site. Whis will result in different inundation times and frequencies at the measurement stations between both sites. So when comparing sites also other mechanisms (driven by inundation time and frequency) might play a role. In addition, at the conserved site it you might also be able to explain that a higher elevated marsh scarp/edge combined with a lower tidal range results in a suppressed signal of SAR, hence no clear visible pattern of edge surges after deforestation in 1995 and in general lower SAR rates compared to the deforested site. This really makes it difficult for me to compare the sites, perhaps this can be solved if first a basic analysis shows inundation times/frequencies and hydrodynamic exposure of the sites.
- Lines 336-338: perhaps good to refer to studies where similar methods were used in marshes.
- Lines 413-415: what representative samples were used? From different years and/or from both watersheds included in the study, or other watersheds?

Supplementary materials
- Figure S2a: from the picture it is not clearly ruled out that water will not reach the marsh interior from the creek at the back of the study site. Where the levee adjacent to the smaller channel might be lower than the marsh scarp that is studied

References
1. Bouma, T.J., et al., Short-term mudflat dynamics drive long-term cyclic salt marsh dynamics. Limnology and Oceanography, 2016. 61(6): p. 2261–2275. DOI: 10.1002/lno.10374
2. Singh Chauhan, P.P., Autocyclic erosion in tidal marshes. Geomorphology, 2009. 110(3): p. 45–57. DOI: https://doi.org/10.1016/j.geomorph.2009.03.016
3. Pannozzo, N., N. Leonardi, I. Carnacina, and R.K. Smedley, Storm sediment contribution to salt marsh accretion and expansion. Geomorphology, 2023. 430: p. 108670. DOI: https://doi.org/10.1016/j.geomorph.2023.108670

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Version 1:

Decision Letter:

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Dear Dr Himmelstein,

Your manuscript titled "Accretionary levees as emerging barriers to platform accretion in marshes" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment, provide you fully address all the remaining concerns raised by Reviewer #2 and Reviewer #3.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

Somaparna Ghosh, PhD
Associate Editor,
Communications Earth & Environment
Consulting Editor,
Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I thank the authors for having addressed my comments.

Reviewer #2 (Remarks to the Author):

Interesting paper dealing with marsh accretion and edge processes. They make a compelling case that defenses to lessen or prevent marsh edge erosion may lead to enhanced levee formation that will reduce sedimentation rates and vertical accretion of interior marsh. Because of the importance of this finding with respect to proactive attempts to erect living shorelines and hardened structures, it would be beneficial to include a discussion of how their results might apply to marshes that experience substantially greater tidal ranges. While the marshlands of Louisiana have similar tidal ranges to NC, much of East Coast of the US and elsewhere in the world experience much larger tidal ranges. Would these trends still apply?

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Labored writing, too many words to say simple statements. Example given below:

As efforts intensify to slow wetland degradation and coastal erosion through wave-dampening interventions such as living shorelines, the formation of prominent marsh levees due to ephemeral sediment loading may produce suboptimal edge micro-topographies that such interventions would inadvertently preserve.

Instead of: “emergent microtopography,” how about something simple that many readers will immediately recognize: vertically-building levee, or accretionary levee

Some processes, rates, etc. are presented in the paper with little information provided as to how representative these measurements are. Also, we are not given the error bars associated with some of these measurements. For example:
1. How representative are your two transects? One transect for each of the Deforested and Conservative areas may be sufficient, but how do we know this?
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Other Comments:
• At the Conserved site, why is there no signal of increased sedimentation caused by deforestation prior to 1959?

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• Figure 5 shows sediment accumulation rates for two time periods, along three transect sites for two study areas: 12 scenarios. Of these, 8 of the scenarios are pretty equal with only the remaining four exhibiting much greater rates and these are all for the period of rapid SLR. Could the incrementally slight increase in inundation time during the period of acceleration SLR really explain this, or is it due to other factor(s)?
• You talk about levee formation inhibiting sediment contribution to interior regions, but your accretion curves show that after the “accretion surge” accretion rates for the three sites become very similar.
• One of the most important findings in paper receives very little attention. Every one of your six coring sites exhibits accretion rates that do not equate with the present rate of sea level rise. While the influence of levees is important, your work demonstrates that the marsh will be drowning. You might consider making this point more prominent.
• Missing the Hein et al 2024 reference.

Reviewer #3 (Remarks to the Author):

Comments in the review report I provided are considered and thoroughly processed by the authors. Additional information is provided as suggested and more importantly the boundaries of the applicability of the studies are now clearly shaped and nuances are brought in.

In my opinion nuances can still be improved in the last section of the main body of text (line 504 in tracked changes document). As in this section shoreline hardening and living shorelines are being discussed in relation to the formation of levees. The latter is studied in the MS, but the relation with shoreline hardening is not being discussed and quantified. So, as this is part of potential wider implications, the right nuances need to be made. There are many possibilities for shoreline hardening, including methods that stimulate sediment accretion just seaward from the marsh scarp, building the full profile. So again, this needs to be nuanced and tied to the right environment and conditions.

Apart from this last comment, improvements are made as suggested and/or there is a reasonable explanation why choices are being made.

Response to Referees for manuscript entitled "Accretionary levees emerging barriers to platform accretion in marshes"

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This well-designed study makes an important contribution to understanding marsh dynamics under changing sediment regimes. The paired-watershed approach, combining remote sensing with high-resolution sediment cores, effectively demonstrates how increased sediment loading can paradoxically create prominent levees that inhibit interior marsh accretion.

The studied systems are microtidal but differ notably in tidal range (0.32 m deforested vs 0.52 m conserved). The stronger levee-interior decoupling observed at the deforested site could result from increased sediment loading, its lower tidal range making it more sensitive to topographic barriers, or both factors acting in combination. The Discussion should address this potential confound.

Good point. In order to address the impact of historic flooding on sedimentation rates sediment accumulation rates, we performed a new analysis and used historic sea-level measurements to calculate historic inundation frequency (ϑ – Supplemental Figure S5). This analysis revealed that levee emergence at the deforested site, where the tidal range is lower, indeed makes it a more effective topographic barrier. We now address this in the discussion "The emergence of a .10 m levee in a system with a 0.32 m tidal range represents approximately 30% of the mean tidal amplitude, suggesting that this microtopographic high can reroute or restrict flow for a portion of regular tidal oscillations. Indeed, following the emergence of the levee at the deforested site, the inundation fraction remains between 0-10%, consistent with fair-weather tidal exclusion, with only elevated water levels capable of overtopping the levee. During these periods, water and associated sediment is likely routed through microtopographic lows, such as feeder channels or lower-relief sections of the escarped marsh edge."

We also include further clarification on the tidal range discrepancies between the watersheds throughout the manuscript.

Additionally, the authors should acknowledge that the mechanism of levees inhibiting interior accretion may be particularly pronounced in the most microtidal settings, where levee-interior elevation differences constitute a larger proportion of the tidal range. This would prevent over-generalization to macrotidal systems where spring-neap variability is substantially larger than typical levee heights.

We now emphasize the importance of developing larger vertical relief in in microtidal systems beginning in the introduction, with multiple mentions of the microtidal setting in which this phenomenon is most relevant. Then to address the generalizability of this study in the latter half of the discussion, we state that “The findings are most applicable to scarp-bounded fringing marshes in microtidal systems, where rapid accretion on the order of 10 mm yr^{-1} can meaningfully modify hydraulic connectivity within the narrow range of tidal water levels. Although microtopography still structures flow in systems with larger tidal range (e.g. Temmerman et al., 2005; Belhadj-aissa et al., 2026) or in marshes characterized by distinct geomorphic stages – such as wave-dominated open-coast marshes, marshes fronted by extensive intertidal mudflats, or macrotidal systems where marsh–flat coupling dominates sediment exchange – the levee driven modulation of tidal exchange descried here is likely most relevant to marsh health microtidal settings like those studied here.”

Reviewer #2 (Remarks to the Author):

Interesting paper dealing with marsh accretion and edge processes. They make a compelling case that defenses to lessen or prevent marsh edge erosion may lead to enhanced levee formation that will reduce sedimentation rates and vertical accretion of interior marsh. Because of the importance of this finding with respect to proactive attempts to erect living shorelines and hardened structures, it would be beneficial to include a discussion of how their results might apply to marshes that experience substantially greater tidal ranges. While the marshlands of Louisiana have similar tidal ranges to NC, much of East Coast of the US and elsewhere in the world experience much larger tidal ranges. Would these trends still apply?

Thank you for comment. We added a paragraph right in the beginning of the discussion regarding tidal ranges and the systems to which these findings are most applicable (tracked-changes lines 321-328). We also frontloaded more details about tidal ranges (in first paragraph of results), and the microtidal nature of the systems we propose this hypothesis in (introduction).

Much of the paper discusses different land-use classes and Deforested versus Conserved sites, but to the uninitiated, these classifications are somewhat abstract. Somewhere in the Introduction or elsewhere, it would be beneficial to the reader to have these classes well defined along with a description of their important attributes that are pertinent to this paper.

We now expand our description of how deforestation leads to sediment loading in the introduction. Then, towards the end of the introduction when we define the distinct land use classes and why they are relevant to the story: "One watershed experienced rapid and extensive land clearing and deforestation, which is expected to elevate runoff and suspended sediment delivery. In this setting, our data show…." and then for the conserved watershed: "In a contrasting watershed, conservation efforts led to long-term reforestation, a trajectory associated with less fluctuation in sediment loading. There, we find…". This is meant to briefly distinguish the two watershed classifications, while in the discussion we begin with how deforestation elevates sediment supply by "by reducing root-mediated soil stability and increasing runoff" (tracked-changes line 371). We also draw attention to the National Land Cover Database definitions (tracked changes line 169-170) which our classifications are based around.

Labored writing, too many words to say simple statements. Examples given below:
Beyond watershed-derived inputs, sediment exchange at the marsh boundary itself can dominate local budgets. In marshes with migrating boundaries, lateral marsh edge erosion provides an additional source of sediment.
As efforts intensify to slow wetland degradation and coastal erosion through wave-dampening interventions such as living shorelines, the formation of prominent marsh

levees due to ephemeral sediment loading may produce suboptimal edge micro-topographies that such interventions would inadvertently preserve.

Reworded these sentences and others where wording felt a bit heavy.

Instead of: "emergent microtopography," how about some simple that many readers will immediately recognize: vertically-building levee, or accretionary levee

We have updated the title to "Accretionary levee" which is admittedly easier to understand. There is emerging discussion of microtopography as both an indicator and structurer of marsh health, thus the initial phrasing. We still use the term microtopography elsewhere in the paper, but accretionary levee where we are explicitly referring to this edge phenomenon.

Some processes, rates, etc. are presented in the paper with little information provided as to how representative these measurements are. Also, we are not given the error bars associated with some of these measurements.

In the revision we now explicitly mention the likelihood of spatial heterogeneity across marsh edges, and note "our observations are not intended to represent marsh-wide accretion rates, but rather to resolve cross-shore gradients in sediment accumulation that arise from channel-adjacent processes. The goal of these measurements is to test a local mechanism whereby increased sediment loads can temporarily restructure marsh microtopography, adding nuance to the assumption that increased sediment supply uniformly benefits marsh systems"

For any measurement derived from multiple values, we include error bars or +/- ranges and define how those were calculated either in the manuscript body, figure captions, or supplemental document.

For land use change, we report the percent of the watershed classified as each landcover type to the nearest integer percent. While these classifications are subject to uncertainty associated with image quality and classification accuracy, we didn't propagate formal error bounds as the imagery is relatively high spatial resolution (<2 m) and we aggregated it into just 5 classes. Errors from imagery would be exceedingly small relative to the observed change trajectories from <10% to ~50% for cleared land in both watersheds), thus would not affect our discussed interpretation of land-cover trajectories or their role in driving sediment supply.

Perhaps there is another area/method that we have overlooked uncertainty with, and are ready to address that as identified. We respond to specific comments about representativeness below.

For example:

1. How representative are your two transects? One transect for each of the Deforested and Conservative areas may be sufficient, but how do we know this?

While we can't be sure that our transects fully capture the range of variability across each marsh complex as we lack widespread sedimentation or levee morphology data, several lines of evidence suggest that the selected transects are not anomalous. Sites were selected for their average marsh width, were roughly halfway between the incised channel and the confluence of the watershed with the broader sound, and edge morphology at the transect locations (as evidenced by the digital elevation map used for shoreline mass flux calculations) is comparable to other reaches within each watershed. Additionally, the general marsh and watershed morphology has remained similar over the study period per our imagery.

We now briefly mention this motivation of sites and their potential representativeness in the first paragraph of results on accretionary gradients (across the transects) so that the reader has this information in the main text body (tracked-changes lines 214-215), then go into further details in the methods site description section (tracked-changes lines 527-529 and 533-537).

2. Marsh edge erosion may provide sediment to the levee in the rollover process, but might the sediment also have come from the channel and flat during storm events?

We agree – the channel bottom / subtidal flat are certainly reservoirs which source sediment during high energy events, especially when filled in with excess material from upstream loading or edge erosion. We allude to that slightly in explaining the lagged response to land use change (channel storage dynamics), but now re-emphasize the importance of these sources in the discussion. We include this as a new paragraph on autogenic recycling of sediment from the channel and flat in the discussion (tracked-changes lines 383-392).

3. How representative are the 210Pb age profiles. Are the computed rates affected by bioturbation?

We attempted to minimize bioturbation by moving away from the marsh scarp by +2 m (informed by our observations of burrowing at our sites). We collected companion cores split lengthwise for visual down-core analyses and didn't note any massive burrow tracks. We now include mention of bioturbation motivating our sampling in our methods as we had that in an early draft but had removed it.

As Appleby and Oldfield (1992) note, when using the constant rate of supply (CRS) / Constant Flux (CF) age-depth model, a mixing zone spanning 10 years would give a maximum error of just 2 years. To determine how long edge sediments may have spent in the surface active zone, we used (sedimentation rate (CF:CS) below * layer thickness) following the methods of citation. Our maximum mixing zone spans 6 years (2021-2015; top 5 cm in deforested-edge-

Pb210.csv). We in part selected an age-depth model that is resilient to mixing (the CF/CRS model) largely because the total Pb-210 inventory is unchanged by bioturbation, and it is inventory which drives our timing and the absolute magnitude of sediment and mass accumulation rates.

Appleby, P. G., & Oldfield, F. (1992). Applications of lead-210 to sedimentation studies. In Uranium-series disequilibrium: applications to earth, marine, and environmental sciences (pp. 731–783). Retrieved from https://inis.iaea.org/records/6r50m-60095

Arias-Ortiz, A., Masqué, P., Garcia-Orellana, J., Serrano, O., Mazarrasa, I., Marbà, N., et al. (2018). Reviews and syntheses: 210Pb-derived sediment and carbon accumulation rates in vegetated coastal ecosystems – setting the record straight. Biogeosciences, 15(22), 6791–6818. https://doi.org/10.5194/bg-15-6791-2018

Other Comments:

• At the Conserved site, why is there no signal of increased sedimentation caused by deforestation prior to 1959?

There is actually a slight increase in sedimentation and mass accumulation in the 1940s into early 1950s (when rates plateau for a period) at the marsh coring locations (Supplemental Tables). As the conserved site was selectively logged (Ballard, 2018), it never received the same rapid and non-fragmented deforestation that occurred at the deforested site, hence why we expect the sedimentation response isn't as pronounced. It was cleared more as a patchwork, which can be seen by the patchiness of forested/cleared in the land cover classifications (Figure 2). We now explicitly mention that in the results section on diverging land use change (tracked-changes line 157-161), as well as in the results text on sedimentation rates (tracked-changes lines 255-256).

• Could you add a couple of sentences that explain how deforestation of a mostly flat landscape translates to increased sedimentation on the marsh.

Beginning in the introduction, we added that "low-relief coastal plain watersheds, deforestation increases sediment delivery primarily by enhancing runoff and reducing soil cohesion. Exposed soils and compacted surfaces promote greater overland flow during rainfall, elevating suspended sediment loads which can be temporarily stored within channels and floodplains before being remobilized and delivered downstream on multi-year timescales. This lagged sediment signal has been shown to propagate efficiently through coastal plain river networks and to influence marsh sedimentation and extent for years to decades after land-use change (e.g. Slattery et al., 2001; Mattheus et al., 2009; Kirwan et al., 2011; Gunnel et al., 2013)"

Then in the discussion, we reiterate the point that deforestation can "elevate riverine total suspended solids (TSS) by *reducing root-mediated soil stability and increasing runoff*" (tracked changes line (371) which we follow with the discussion of weak stream power and channel storage leading to lagged propagations of the deforestation signal despite small watershed size (tracked changes line 383-392).

• It would be useful to know more about the processes of sedimentation on the marsh surface and why the levees develop preferentially when suspended is more abundant. Are there different plant types, stem densities, difference in inundation durations, or is this strictly a product of decreased flow velocities related to grass baffling?

Good point. We have now expanded our description of levee response to loading in the introduction which refers to the first text we know that refers to the phenomenon of disproportionate levee growth under higher edge sediment supply (Temmerman et al., 2004) and a more recent one which shows that under low sediment supply, levees do not form (Albernaz et al., 2020). We explicitly name vegetation / larger cross-sectional area which lead to reduced flow velocities and deposition. (tracked-changes lines 77-88)

• How often and to what depth is the marsh platform inundated during neap and spring tides? This knowledge would help the reader evaluate levee development.

Using tidal inundation fraction (as in Morris et al., 2021) we now include an analysis of inundation frequency through time (Fig S5) and refer to that in various areas of the results and discussion, and respond in more detail to this line of inquiry several comments below.

• Why are the scarps higher at the Conserved site and why are they eroding at a faster rate compared to the Deforested site?

There are several reasons we propose this may be due to the higher tidal ranges at the conserved site. Greater tidal oscillations will enhance pore-pressure fluctuations and drive hydraulic gradients between high and low tide, which can then promote seepage-driven undercutting and episodic marsh-edge failure. Also, the larger tidal range and smaller open water leads to a larger tidal prism per unit width, thus likely higher tidal velocities which more effectively flush fine-grained sediments and could enhance edge erosion or at least remove sediment which would otherwise shield the scarp toe. Also, with the sediment loading at the deforested site, we could (and do; Bost et al., 2023) see sediment additions to channel/bay bottoms, thus the higher scarps might reflect less sediment.

• Figure 5 shows sediment accumulation rates for two time periods, along three transect sites for two study areas: 12 scenarios. Of these, 8 of the scenarios are pretty equal with only the remaining four exhibiting much greater rates and these are all for the period of rapid SLR. Could the incrementally slight increase in inundation time during the period of acceleration SLR really explain this, or is it due to other factor(s)?

The available sediment at the deforested site likely helps potentiate the impacts of increased inundation (see the doubling in inundation fraction from 0.2 [~2000-2008] to 0.4 [2020] of interior location at deforested site) which explains the greater rates at the deforested site (three of the four scenarios which exhibit much greater rates), while the fourth scenario is the last 20 years measured at the edge core in the conserved site. There, we expect that as lateral erosion progressively allows the fixed core location draw closer to channel sourced sediments, the edge-benefit is realized. We think that the order of controls is (1) availability of sediment in channel (supply limited) (2) proximity to channel sourced sediments – controlled by lateral erosion (transport limited) and (3) inundation frequency (transport limited). So SLR and inundation is a factor, but not THE factor. We now include in the results where we mention acceleration of SLR and Sedimentation over the whole study period: "sedimentation acceleration at both sites was lower, indicating that over multi-decadal timescales inundation is increasing more rapidly than vertical sediment accretion." (tracked-changes lines 276-282).

• You talk about levee formation inhibiting sediment contribution to interior regions, but your accretion curves show that after the "accretion surge" accretion rates for the three sites become very similar.

It is perhaps the disproportionate growth of the levee at the same time as the interior regions show slowed accretion that is most interesting. Rates do indeed increase towards present. Our results for the inundation fraction seem to explain this. Under fair-weather tidal condition (MHW as high-water level), the inundation fraction towards present at the deforested site is ~0.1 (edge), ~0.2 (middle), ~0.4 (interior) (new Figure S5). Due to its height in the modern tidal frame, the edge location is least flooded, but most proximal to channel sourced sediment, thus some balance in sedimentation is achieved where lower inundation durations is compensated by higher suspended sediment concentrations at the channel-proximal locations. Because the effect of sediment limitation depends on continued levee prominence and thus may be ephemeral, I think it's fair to de-emphasize the influence of the emergent levee in reducing present day sedimentation rates. We now draw attention to this in the discussion, (tracked-changes lines 427-433).

• One of the most important findings in paper receives very little attention. Every one of your six coring sites exhibits accretion rates that do not equate with the present rate of sea level rise. Forget about the influence of levees, your work demonstrates that the marsh will be drowning. This point should more prominent.

We have increased the prominence of this point, including it as a direct comparison in the last paragraph of our results as well as a discussion of the relevance of levees as second-order control if marshes area already broadly drowning/failing to pace SLR. Results (tracked-changes lines 276-282) and Discussion (tracked-changes lines 214-215).

- **Missing the Hein et al 2024 reference.**

Added, now in references.

Reviewer #3 (Remarks to the Author):

Review "Emergent edge microtopography as a barrier to platform accretion and marsh sustainability" submitted to Nature Communications earth and environment.

The manuscript describes a great effort in collecting and analysing sediment cores at two salt marsh study site, one site that is being conserved, while at the other site over the past decades deforestation took place. By selecting these two sites, driven by deforestation:
- sediment influx over the marsh scarp is assessed, and;
- the potentially imbalance between sediment accumulation at the marsh edge, compared with the middle and interior marsh is quantified.

Review highlights
I would like to start with congratulating the authors with collection of this long-term decadal dataset, where land use is combined with measured accumulation rates. I do understand the effort it takes to collect data in salt marshes. Apart from that I have three major points regarding the manuscript and storyline that is brought forward.

- by only reading the main body of text (without the methods section), it is difficult to understand the most important dynamics of the deforested and conserved site. This is necessary to compare both sites, e.g. relative elevation to the tide, tidal range, tidal inundation/frequency is not considered in the manuscript. When reading the methods section, it seems those parameters are not comparable between the two sites, which makes it difficult to understand the comparison and conclusions based on a comparison of both sites.
- I am not sure whether the deforested site can serve as proxy for sea level rise. Moreover, the results of the deforested site can also be interpret as follows: sediment load at the deforested site increased largely after 2001 at all measurement stations, although more at the marsh edge. Moreover, when comparing the deforested site with the conserved site, at all measurement stations SAR increased more at the deforested site. See also feedback regarding Lines 273-275
- Finally, both in the introduction and in the discussion, it does not become fully clear for what salt marsh types and tidal characteristics (micro-, meso-, macrotidal) the conclusions are valid.

To me it seems the above points are very important for shaping the manuscript and storyline. In my opinion improvement is needed and perhaps a change of storyline to make the measurements and analysis support the conclusions and implications stated. More details regarding the above points, as well as other smaller comments can be found below.

Review details

Abstract

- Settings of the coastal landscape the MS is focusing on, is not fully clear to me in the abstract. Are the results shared valid for e.g. all marshes, marshes that have a cliff at the seaward edge, marshes with or without a intertidal flat at the seaward edge of the marsh? In addition is the analysis applicable to the full range of microtidal to macrotidal wetlands? In my opinion clarifying this is important, as it determines for which salt marsh and/or salt marsh development stage the analysis is relevant.

Added explicit mention of microtidal setting in which depositional disconnect is observed to abstract (tracked-changes lines 15).

Introduction

- Lines 32-35: I don't think you can state that tidal marshes with low sediment inputs and narrow tidal ranges are landscapes in which to study the manifestations of sea-level-rise. As equal rates of transforming from vegetated marsh to bare mudflat, requires more sediment input under sea level rise compared without sea level rise. So, there will be more sediment trapped under sea-level-rise, resulting in different dynamics of the top layer of the substrate (e.g. seedling establishment and growth, consolidation processes, etc).

We agree that sea level rise will alter the accommodation space in which sediment input must fill, and with low background sediment supply the marsh may fail to keep pace with sea level. As accretion accelerates under increased inundation, the sediment may be less consolidated if sparsely dewatered. The marshes studied here, however, don't exhibit an intertidal mudflat or a pioneer zone, and rarely show significant expansion at the seaward edge.

- Line 58: straight going to a description of levees, without any introduction, is confusing. This might need some introduction first, especially because it is presented as hypothesis ("may play a role in separating…")

Good point – it does read a bit abruptly. We mostly liked that framing as a direct response to the question posed at the end of the prior paragraph – but some more priming on sediment supply – deposition feedbacks is useful for the reader to understand why levees are there and may respond to deforestation. We now start with the literature that models increased levee prominence (elevation relative to marsh behind it) when sediment loads increase and are not evenly distributed across the marsh (e.g. Temmerman 2004, Albernaz et al., 2020). Then we define levees and a brief overview of why they exist before describing the influence of levee prominence (see below).

- Lines 63-65: “prominent levees” and “larger levees” are difficult to put in context without any explanation or quantification.

Changed “larger levees” and clarified use of “prominent levees” by explicitly defining them in terms of their relative elevation above the marsh interior and their proportionally increased position within the tidal frame (which results in more distinct inundation frequencies relative interior marsh and can lead to flow interception or re-routing).

- Lines 68-71: I fully agree that within certain timescales (i.e. decades), levees might grow with increasing sediment availability. However, 1) a certain type of landscape is assumed here, which is not clearly described, a marsh edge, i.e. an offset between vegetated marsh and bare tidal (or subtidal) area needs to be present. Either that can be a marsh cliff or a tidal channel directly neighboring a vegetated marsh platform. In addition, I think the hypothesis is not applicable for fringing marshes with a pioneer zone and a gradual transition from bare area to vegetated marsh; 2) in a healthy and dynamic salt marsh system, there are alternating phases between lateral expansion (i.e. growth) and erosion (e.g. retreat) of the marsh over decadal timescales1,2. Once in a retreating phase a levee might form at the salt marsh edge, because of the formation of a cliff. In this retreating phase, the fringing bare tidal flat allows for sediment accretion, resulting in the formation of a new pioneer zone. This process is actually partly described in lines 51-54. In this current hypothesis it is not clear how to distinguish between a marsh where a tipping point is reached and marsh expansion is not possible anymore, despite infilling/accretion of the area fronting the marsh, and a marsh with natural dynamics that is able to expand. So, some nuances and additional explanation is necessary.

We are primarily interested in the decadal timescales over which levees might emerge – especially in systems where long-term monitoring (e.g. Surface Elevation Tables) indicate interior marshes are failing to pace SLR despite. Across the east coast of the United States, this is common in transgressive marshes with marsh cliffs/scarps (good coverage in study by Weston et al., 2023). To address this, we specify the study setting more explicitly in the abstract (“microtidal system” and, “seaward edge of upland-fringing marshes”) and introduction (“focused accretion along scarped channel margins characteristic of eroding marshes”).

I think the distinction you address is an important nuance – and one we are still working to figure out. What amount of sediment loading, duration of loading, and locus of deposition is required to prevent the erosive marsh edges we observe in these fringing marshes? Indeed, we see lower rates of watershed erosion in the deforested site (which actually has a bit larger open water area and fetch), which may in part be due to increased subtidal infilling there. While we don’t think the design of this study is capable of finding that tipping-point, we now include discussion of the fact that sufficient sediment supply can and has allowed marsh expansion rather than erosion (introduction: tracked-changes lines 60-63 and in discussion: 420-422).

Weston, N. B., Rodriguez, E., Donnelly, B., Solohin, E., Jezycki, K., Demberger, S., et al. (2023). Recent Acceleration of Wetland Accretion and Carbon Accumulation Along the U.S. East Coast. *Earth's Future*, *11*(3), e2022EF003037. https://doi.org/10.1029/2022EF003037

- Lines 68-71: As mentioned in the introduction as focus site (deltaic wetlands of the Mississippi River), is this hypothesis only valid for microtidal marshes?

While similar mechanisms may be at play (feedback between levee-interior sedimentation) in meso and macro-tidal marshes, our study only tests microtidal systems and thus we now specify that we our hypothesis is with regards to "microtidal marshes" both in the abstract, introduction (line 65, 87, 94, 108) and the discussion, where "Our findings are most applicable to scarp-bounded fringing marshes in microtidal systems, where rapid accretion on the order of 10 mm yr^{-1} can meaningfully modify hydraulic connectivity within the narrow range of tidal water levels" (tracked-changes lines 321-328)

- Lines 74-onwards: to me this comparison between the two sites feels a bit too simplistic and at least needs mentioning whether hydrodynamic energy and tides are similar, otherwise other mechanisms in addition to sediment loading might play a large role in sediment accretion of the marsh platform.

The suggestion to include more about the elevation and tidal inundation in the main body of the manuscript is well received and prompted a new analysis of historical inundation fraction which we report in our results section (as it is derived from our sedimentation data), a new figure in our supplemental document (Fig S5), and a more robust description of tidal range and hydrodynamic setting early in the results section. Then in the methods we now report the open water areas (tracked-changes lines 524-529) and that the "areas of both systems are similar sizes and have a dominant axis trending northwest-southeast, perpendicular to the prevailing wind directions in the region (Giese et al., 1985)". While not a formal hydrodynamic analysis, the combination of this information and our formal tidal analysis show both watersheds are microtidal embayments with limited wind-aligned fetch. We also added in our method a comment on the potential representativeness of our measurement transects (tracked-changes lines 533-537).

Results

- Lines 94-96: the manuscript states that bed resuspension depends on changes in water depth and hydrodynamic forcing. As I fully agree with that (also considering the importance of increased hydrodynamic energy in delivering sediment3), I am missing a hydrodynamic characterization of the two sites in the main body of text. To be able to only

compare impact of sediment loads, inundation and hydrodynamic energy at both sites need to be rather similar.

As addressed above, we now include formal inundation analysis through the sediment record (Fig S5), showing that both sites are low marsh S. alterniflora marshes inundated between regularly by mean tidal oscillations. We are aware that the sites are not perfect mirrors, however, as the watersheds most proximal to the deforested watershed, which have even closer tidal range forcings, experienced the same land use change trajectories. Thus we used the nearest watershed with similar setting, watershed area, microtidal range, and divergent land use trajectory of reforestation.

- Figure 2: make sure fontsize is not too small
Updated Figure 2 to increase font size of tidal datums and axes labels.

- Line 136: here "marsh scarp" is used, whereas at other locations, also for the definition of the measurements stations, "marsh edge" is used. In addition, it seems like "marsh edge" sometimes is also used to define the whole transect. Please be clear and consistent in using these definitions.
We updated the manuscript to exclusively use marsh scarp when discussing the elevational prominence of the marsh above the adjacent mudflat. We also made changes throughout at avoid using "marsh edge" – opting instead for "edge coring location" or simply "edge, middle, and interior marsh")
- Lines 173-176: it would be very insightful and helpful to understand dynamics, to show the differences in bed elevation of 1) the different measurement stations (edge, middle, interior) and 2) for both sites. To highlight how the offset in elevation changes between stations and sites. Can also be in supplementary materials.

We enlarged the labels in Figure 2 and specified in the caption title that these profiles are measured, i.e. show the bed elevations where each of the cores were taken from as well as the levee prominence at the sites. The increased label sizes also increase the visibility of the horizontal tidal range lines (MLW, MTL, MHW). Renamed 2021 Profile to "Elevation Transect (2021) so the setting elevational setting in relation to tidal datums is clearer.

- Figure 3: the white bars make it difficult to distinguish the three different symbols; to increase understanding the figure related to the results, perhaps add a horizontal line for the year 2001, as this is used in the manuscript as clear temporal boundary for analysis.

After trying a variety of error bar representations and symbol sizes and line colors, we found the white bar offered the best tradeoff between communicating uncertainty and minimizing obstruction of the SAR measurements/lines. Added the horizontal line and labeled 2001 in Figure 3.

- Lines 209-211: The negative correlation between marsh edge and middle SARs at the deforested site are clearly shown, but that doesn't necessarily imply a levee growth (edge) at the expense of accretion (middle). Figure 3a shows for the middle SAR around the year 2000 an initial increase after rapid deforestation, with a subsequent decrease and increase after increasing and decreasing edge SAR, respectively. So, sedimentation at the edge might be a driver. However, this explanation is too simplistic and should be nuanced, because 1) hydrodynamics are not considered here, perhaps energy and or inundation (sea-level-rise) changed over the same period and 2) at the deforested site the middle and interior SAR keep on average increasing, interestingly faster just after the peak of the SAR at the edge.

We add nuance that this is a local effect and that "Salt marsh sedimentation is spatially heterogeneous. Our observations are not intended to represent marsh-wide accretion rates, but rather to resolve cross-shore gradients in sediment accumulation that arise from channel-adjacent processes. The goal of these measurements is to test a local mechanism whereby increased sediment loads can temporarily restructure marsh microtopography, adding nuance to the assumption that increased sediment supply uniformly benefits marsh systems" Line (tracked-changes lines 446-453).

And also in the discussion explicitly name the phenomenon you describe, with middle and interior rates increasing towards present "potentially due to transport limitation under reduced inundation frequency" (tracked-changes lines 427-433).

Although middle and interior SARs at the deforested site increase in absolute terms following the period of maximum edge accretion, they do so at rates insufficient to keep pace with contemporaneous sea-level rise. Thus, the negative edge–middle relationship reflects a spatial discrepancy in sedimentation during periods of elevated supply rather than a simple suppression of interior accumulation (like at the conserved site). We believe this decoupling remains a key finding, as it demonstrates that increased sediment supply does not translate with the same magnitude, yes, but also direction, across the marsh platform.

- Lines 217-218: In general SARs at the middle and interior increased on average more when comparing the deforested site with the conserved site. From figure 3a you could also conclude that after an initial change in SAR (year 2000), a new balance is settled (around year 2005) after the edge SAR peaked, the middle SAR recovered. So, the implications for sea-level-rise which is a by far slower process are not that clearly emerging from these results to me.

This is a fair summary of the data. We agree that our data stop short of showing a causative link between levee emergence and marsh sustainability under sea-level rise. However, we feel our inundation analysis undoubtedly shows the potential for rapidly emergent levees to reroute flow, tidal exchange, and thus porewater residence times in microtidal marshes. With other recent observational work (e.g. Qi et al, 2025) showing that soil saturation is moderated by elevation differences and also distance from creek, and that poor drainage in the interior is even "a more direct factor [in heterogenous marsh vulnerability] than differential accretion", we believe levee emergence is an important part of this story. Thus, we add to our discussion that our sampling approach integrates the net effects of interacting processes (drainage efficiency / porewater conditions / sediment delivery), "but does not isolate their individual contributions; accordingly, future studies should attempt to find causal associations between marsh levees and specific mechanisms through sediment transport measurements and targeted biogeochemical monitoring." (tracked-changes lines 362-366). Additionally, in frequently referring to the microtidal settings in which marsh interiors most commonly pond due to inefficient drainage, observations of edge survival and interior die off

Discussion

- In general I miss for what specific marshes in terms of tidal characterization and types this study applies. And/or how these results transfer to meso- to macrotidal marshes, and marshes with more extensive mudflats where establishment of pioneer vegetation might be possible after increasing rates of SAR and further retreat of the current marsh edge. In my opinion this is essential for the study and for understanding and comparing dynamics at both sites.

Thank you for drawing attention to the sparse use of tidal descriptions, which are indeed relevant here and an oversight of our initial submission. Beyond the tidal range visualization atop the measured marsh cross-sections in Figure 2, we moved our explicit mention of the tidal ranges from methods to the caption of this figure so the reader can directly note the microtidal ranges (tracked-changes lines 185-187). We also mention that the accretional disconnect between subtidal and marsh platform settings has been observed specifically in microtidal systems (tracked-changes lines 70). Then, in discussion, we clarify that our findings come from a microtidal setting (tracked-changes lines 322) and note that systems with larger tidal ranges may not behave similarly because.

Beginning in the abstract, we add mention that the marshes we study are microtidal and that the emergent microtopography is measured at the "seaward edge of upland-fringing marshes". Then, in the discussion where we have more space to extend our description of which type of marsh this is relevant to, we add that "The findings are most applicable to scarp-bounded fringing marshes and may not directly translate to marshes fronted by extensive intertidal mudflats, wave-dominated open-coast marshes, or meso-to-macrotidal systems where marsh–flat coupling dominates sediment exchange."

- Figure 4: see comment above, how do tidal characteristics, relative elevation and tidal prism relate to these relations?

Figure S5 now shows inundation frequency through time. Visualized with the down core SARs or (Fig 3) or MARs (Fig S4), there is not a strong positive correlation between peaking sedimentation rates and inundation fraction: our data suggest that increased inundation is not the main driver of higher SAR rates at the deforested site. In fact, inundation fraction is quite low in the 00s just as sedimentation is maximized at the edge core location in the deforested site. Inundation fraction (Morris et al., 2021) is based on MHW (not MHHW or meteorologically driven high water events), so likely misses a proportion of times when the marsh is underwater during which significant deposition is occurring (as evidenced by the high SAR rates).

- Line 237: should "on" perhaps be "in"

I think for timescales or approximate magnitudes, "on" is preferred.

- Lines 273-275: the results and translation of the results to sea-level-rise are not convincingly explaining how the levee is further build and the marsh interior drowns both by pulses of sediment load and sea-level-rise in my opinion. 1) for temporarily increased sediment loads, after a temporary decrease of mid/interior SAR, you might also say that accumulation there recovers and even increases due to the additional sediment available; 2) with SLR water levels might or might not exceed the elevation of the edge more often. Whether exceedance of this elevation increases or decreases really depends on accumulation at the edge. Figure 5 shows before 2001 an accumulation rate exceeding SLR at the edge (i.e. increasingly less inundation and potential accumulation at the interior), while after 2001 SAR is at the same level as SLR. So, this might imply an increased inundation and potential accumulation at the marsh interior when comparing before and after 2001 (without and with deforestation). Please don't get me wrong, I am not in favor of deforestation, but I am not fully sure whether the measurements and results in the manuscript sufficiently support statements in the discussion.

The elevation of the interior core location at the deforested site exists just above mean tide level (Fig 2), a theoretical lower bound suggested in marsh equilibrium models like Morris et al (2002) and observed for *Spartina alterniflora* (which occupies our study sites) by McKee and Patrick (1988) and Morris et al. (2013). The edge core location, which emerged especially quickly following the sediment loading, now sits essentially at mean high water, so I think your critique is fair that at least that location will have some buffer before it can be considered a proxy for sea-level rise. We do propose either site as a direct proxy for sea-level, but rather are interested in the mechanism whereby differential accretion reinforces microtopography with implications for how such an impacted marsh may respond to sea-level (i.e. by rerouting flow into the marsh through feeder channels and more circuitous paths).

While the deforested site does have a low tidal range, water levels at both sites are dominantly driven by astronomical conditions rather than meteorologically.

Thus we expect rising sea-levels to moderate interior elevations at the interior of the deforested site as well as the conserved site. The elevation of the edge core location exists right at modern (2021) mean high water level, while the

Morris, J.T., Sundareshwar, P.V., Nietch, C.T., Kjerfve, B. and Cahoon, D.R., 2002. Responses of coastal wetlands to rising sea level. *Ecology*, *83*(10), pp.2869-2877.

McKee, K.L., and W.L. Patrick. 1988. The relationship of smooth. cordgrass (Spartina alterniflora) to tidal datums: A review. Estuaries 11: 143–151.

Morris, J.T., K. Sundberg, and C.S. Hopkinson. 2013. Salt marsh primary production and its responses to relative sea level and nutrients in estuaries at Plum Island, Massachusetts, and North Inlet, South Carolina, USA. *Oceanography* 26: 78–84.

- Lines 289-291: perhaps my fault, but I do not understand what is being said here.

We rewrote several sentences in this discussion section to clarify that erosion of edge material is a continual source throughout the study period for marsh accretion at both sites. See (tracked-changes lines 416-426)

Methods

- Lines 330 – 332: It is difficult to compare values between sites as the tidal range and also elevations of the cliff are not of equal height. Although I do agree that at the deforested site a clear pattern is visible after the period of rapid deforestation, the marsh scarp at the deforested site is lower, and tidal range is larger compared to the conserved site. Whis will result in different inundation times and frequencies at the measurement stations between both sites. So when comparing sites also other mechanisms (driven by inundation time and frequency) might play a role. In addition, at the conserved site it you might also be able to explain that a higher elevated marsh scarp/edge combined with a lower tidal range results in a suppressed signal of SAR, hence no clear visible pattern of edge surges after deforestation in 1995 and in general lower SAR rates compared to the deforested site. This really makes it difficult for me to compare the sites, perhaps this can be solved if first a basic analysis shows inundation times/frequencies and hydrodynamic exposure of the sites.

It is true that the two sites are not equal: the mean tidal range and mean scarp height is 0.32 m and 0.55 ± 0.05 m at the deforested site while they are 0.52 m and 0.89 ± 0.23 m at the conserved site. That being said, both sites are microtidal, affected by the very similar wind fields, sea level rise, and storm exposure. Part of the reasons they are not more similar is that we wanted to focus on watersheds with diverging land use trajectories and these were two of the closest (42 km apart) small watersheds which met that criterion. The other watersheds closer to the impacted deforested watershed were subject to the same land conversion and thus we expected would be similarly sediment loaded.

Despite the smaller tidal range and scarp heights at the deforested site, the levee prominence (defined as levee elevation minus interior elevation) is greater there (0.10 m) than at the conserved site (0.05 m). It is our age-depth model which suggests this levee emerged rapidly (up to >10 mm per year) following deforestation. This is evident in our added inundation frequency figure (Fig S5), which shows the separation of inundation durations in the 2000s as the levee began accreting more rapidly.

As to why we see larger scarps in the conserved watershed; perhaps reflecting the capacity of larger tidal ranges to enhance pore-pressure fluctuations and drive hydraulic gradients between high and low tide, which can then promote seepage-driven undercutting and episodic marsh-edge failure.

- Lines 336-338: perhaps good to refer to studies where similar methods were used in marshes.

Added references to studies which use vibracoring methods and geochronologies to look at marsh response to sediment loading (e.g., Mattheus et al., 2009; Kirwan et al., 2011; Bost et al., 2024)

- Lines 413-415: what representative samples were used? From different years and/or from both watersheds included in the study, or other watersheds?

Many classifiers were trained as the spectral bands available in the imagery changed from year to year. Thus, we trained classifiers for each year and each watershed for imagery collected from 1970 and onwards. We updated the methods here to state **"**a binary land–water classification using Support Vector Machine (SVM) classifiers trained independently on ~500 representative pixels per class for each year and watershed"

Supplementary materials

- Figure S2a: from the picture it is not clearly ruled out that water will not reach the marsh interior from the creek at the back of the study site. Where the levee adjacent to the smaller channel might be lower than the marsh scarp that is studied

That is true – in fact we expect some material is reaching the marsh from the interior feeder channel. In our original manuscript we cite this specific channels as a reason why our interior sediment core does not show the same decline in sedimentation rates as the middle core location does at the deforested site (Results - Line 202-204: “At the interior location, rates remained lower but still gradually increased towards the present and did not show pronounced decrease potentially due to the presence of an interior feeder channel (Fig. S2))” but added “sourcing sediment to the marsh interior” to specify the impact of that feeder channel. Our cross-shore premise of levee emergence does not resolve the complex flow pathways which source sediment to the marsh platform, but we do think this levee emergence mechanism is applicable to the smaller levees which form along feeder tidal channels like the one mentioned.

References

1. Bouma, T.J., et al., Short-term mudflat dynamics drive long-term cyclic salt marsh dynamics. Limnology and Oceanography, 2016. 61(6): p. 2261–2275. DOI: 10.1002/lno.10374

2. Singh Chauhan, P.P., Autocyclic erosion in tidal marshes. Geomorphology, 2009. 110(3): p. 45–57. DOI: https://doi.org/10.1016/j.geomorph.2009.03.016

3. Pannozzo, N., N. Leonardi, I. Carnacina, and R.K. Smedley, Storm sediment contribution to salt marsh accretion and expansion. Geomorphology, 2023. 430: p. 108670. DOI: https://doi.org/10.1016/j.geomorph.2023.108670

Response to Referees for manuscript entitled "Accretionary levees emerging barriers to platform accretion in marshes"

Reviewer #1 (Remarks to the Author):

I thank the authors for having addressed my comments.

We thank the reviewer for their prior comments.

Reviewer #2 (Remarks to the Author):

Same responses as last round.

Interesting paper dealing with marsh accretion and edge processes. They make a compelling case that defenses to lessen or prevent marsh edge erosion may lead to enhanced levee formation that will reduce sedimentation rates and vertical accretion of interior marsh. Because of the importance of this finding with respect to proactive attempts to erect living shorelines and hardened structures, it would be beneficial to include a discussion of how their results might apply to marshes that experience substantially greater tidal ranges. While the marshlands of Louisiana have similar tidal ranges to NC, much of East Coast of the US and elsewhere in the world experience much larger tidal ranges. Would these trends still apply?

Thank you for comment. We added a paragraph right in the beginning of the discussion regarding tidal ranges and the systems to which these findings are most applicable (tracked-changes lines 321-328). We also frontloaded more details about tidal ranges (in first paragraph of results), and the microtidal nature of the systems we propose this hypothesis in (introduction).

Much of the paper discusses different land-use classes and Deforested versus Conserved sites, but to the uninitiated, these classifications are somewhat abstract. Somewhere in the Introduction or elsewhere, it would be beneficial to the reader to have these classes well defined along with a description of their important attributes that are pertinent to this paper.

We now expand our description of how deforestation leads to sediment loading in the introduction. Then, towards the end of the introduction when we define the distinct land use classes and why they are relevant to the story: "One watershed experienced rapid and extensive land clearing and deforestation, which is expected to elevate runoff and suspended sediment delivery. In this setting, our data show…." and then for the conserved watershed: "In a contrasting watershed, conservation efforts led to long-term reforestation, a trajectory associated with less fluctuation in sediment loading. There, we find…". This is meant to briefly distinguish the two watershed classifications, while in the discussion we begin with how deforestation elevates sediment supply by "by reducing root-mediated soil stability and increasing runoff" (tracked-changes line 371). We also draw attention to the National Land Cover Database definitions (tracked changes line 169-170) which our classifications are based around.

Labored writing, too many words to say simple statements. Examples given below:
Beyond watershed-derived inputs, sediment exchange at the marsh boundary itself can dominate local budgets. In marshes with migrating boundaries, lateral marsh edge erosion provides an additional source of sediment.
As efforts intensify to slow wetland degradation and coastal erosion through wave-

dampening interventions such as living shorelines, the formation of prominent marsh levees due to ephemeral sediment loading may produce suboptimal edge micro-topographies that such interventions would inadvertently preserve.

Reworded these sentences and others where wording felt a bit heavy.

Instead of: “emergent microtopography,” how about some simple that many readers will immediately recognize: vertically-building levee, or accretionary levee

We have updated the title to “Accretionary levee” which is admittedly easier to understand. There is emerging discussion of microtopography as both an indicator and structurer of marsh health, thus the initial phrasing. We still use the term microtopography elsewhere in the paper, but accretionary levee where we are explicitly referring to this edge phenomenon.

Some processes, rates, etc. are presented in the paper with little information provided as to how representative these measurements are. Also, we are not given the error bars associated with some of these measurements.

In the revision we now explicitly mention the likelihood of spatial heterogeneity across marsh edges, and note “our observations are not intended to represent marsh-wide accretion rates, but rather to resolve cross-shore gradients in sediment accumulation that arise from channel-adjacent processes. The goal of these measurements is to test a local mechanism whereby increased sediment loads can temporarily restructure marsh microtopography, adding nuance to the assumption that increased sediment supply uniformly benefits marsh systems”

For any measurement derived from multiple values, we include error bars or +/- ranges and define how those were calculated either in the manuscript body, figure captions, or supplemental document.

For land use change, we report the percent of the watershed classified as each landcover type to the nearest integer percent. While these classifications are subject to uncertainty associated with image quality and classification accuracy, we didn’t propagate formal error bounds as the imagery is relatively high spatial resolution (<2 m) and we aggregated it into just 5 classes. Errors from imagery would be exceedingly small relative to the observed change trajectories from <10% to ~50% for cleared land in both watersheds), thus would not affect our discussed interpretation of land-cover trajectories or their role in driving sediment supply.

Perhaps there is another area/method that we have overlooked uncertainty with, and are ready to address that as identified. We respond to specific comments about representativeness below.

For example:

1. How representative are your two transects? One transect for each of the Deforested and Conservative areas may be sufficient, but how do we know this?

While we can't be sure that our transects fully capture the range of variability across each marsh complex as we lack widespread sedimentation or levee morphology data, several lines of evidence suggest that the selected transects are not anomalous. Sites were selected for their average marsh width, were roughly halfway between the incised channel and the confluence of the watershed with the broader sound, and edge morphology at the transect locations (as evidenced by the digital elevation map used for shoreline mass flux calculations) is comparable to other reaches within each watershed. Additionally, the general marsh and watershed morphology has remained similar over the study period per our imagery.

We now briefly mention this motivation of sites and their potential representativeness in the first paragraph of results on accretionary gradients (across the transects) so that the reader has this information in the main text body (tracked-changes lines 214-215), then go into further details in the methods site description section (tracked-changes lines 527-529 and 533-537).

2. Marsh edge erosion may provide sediment to the levee in the rollover process, but might the sediment also have come from the channel and flat during storm events?

We agree – the channel bottom / subtidal flat are certainly reservoirs which source sediment during high energy events, especially when filled in with excess material from upstream loading or edge erosion. We allude to that slightly in explaining the lagged response to land use change (channel storage dynamics), but now re-emphasize the importance of these sources in the discussion. We include this as a new paragraph on autogenic recycling of sediment from the channel and flat in the discussion (tracked-changes lines 383-392).

3. How representative are the 210Pb age profiles. Are the computed rates affected by bioturbation?

We attempted to minimize bioturbation by moving away from the marsh scarp by +2 m (informed by our observations of burrowing at our sites). We collected companion cores split lengthwise for visual down-core analyses and didn't note any massive burrow tracks. We now include mention of bioturbation motivating our sampling in our methods as we had that in an early draft but had removed it.

As Appleby and Oldfield (1992) note, when using the constant rate of supply (CRS) / Constant Flux (CF) age-depth model, a mixing zone spanning 10 years would give a maximum error of just 2 years. To determine how long edge sediments may have spent in the surface active zone, we used (sedimentation rate (CF:CS) below * layer thickness) following the methods of citation. Our maximum mixing zone spans 6 years (2021-2015; top 5 cm in deforested-edge-

Pb210.csv). We in part selected an age-depth model that is resilient to mixing (the CF/CRS model) largely because the total Pb-210 inventory is unchanged by bioturbation, and it is inventory which drives our timing and the absolute magnitude of sediment and mass accumulation rates.

Appleby, P. G., & Oldfield, F. (1992). Applications of lead-210 to sedimentation studies. In Uranium-series disequilibrium: applications to earth, marine, and environmental sciences (pp. 731–783). Retrieved from https://inis.iaea.org/records/6r50m-60095

Arias-Ortiz, A., Masqué, P., Garcia-Orellana, J., Serrano, O., Mazarrasa, I., Marbà, N., et al. (2018). Reviews and syntheses: 210Pb-derived sediment and carbon accumulation rates in vegetated coastal ecosystems – setting the record straight. Biogeosciences, 15(22), 6791–6818. https://doi.org/10.5194/bg-15-6791-2018

Other Comments:

• At the Conserved site, why is there no signal of increased sedimentation caused by deforestation prior to 1959?

There is actually a slight increase in sedimentation and mass accumulation in the 1940s into early 1950s (when rates plateau for a period) at the marsh coring locations (Supplemental Tables). As the conserved site was selectively logged (Ballard, 2018), it never received the same rapid and non-fragmented deforestation that occurred at the deforested site, hence why we expect the sedimentation response isn't as pronounced. It was cleared more as a patchwork, which can be seen by the patchiness of forested/cleared in the land cover classifications (Figure 2). We now explicitly mention that in the results section on diverging land use change (tracked-changes line 157-161), as well as in the results text on sedimentation rates (tracked-changes lines 255-256).

• Could you add a couple of sentences that explain how deforestation of a mostly flat landscape translates to increased sedimentation on the marsh.

Beginning in the introduction, we added that "low-relief coastal plain watersheds, deforestation increases sediment delivery primarily by enhancing runoff and reducing soil cohesion. Exposed soils and compacted surfaces promote greater overland flow during rainfall, elevating suspended sediment loads which can be temporarily stored within channels and floodplains before being remobilized and delivered downstream on multi-year timescales. This lagged sediment signal has been shown to propagate efficiently through coastal plain river networks and to influence marsh sedimentation and extent for years to decades after land-use change (e.g. Slattery et al., 2001; Mattheus et al., 2009; Kirwan et al., 2011; Gunnel et al., 2013)"

Then in the discussion, we reiterate the point that deforestation can "elevate riverine total suspended solids (TSS) by *reducing root-mediated soil stability and increasing runoff*" (tracked changes line (371) which we follow with the discussion of weak stream power and channel storage leading to lagged propagations of the deforestation signal despite small watershed size (tracked changes line 383-392).

• It would be useful to know more about the processes of sedimentation on the marsh surface and why the levees develop preferentially when suspended is more abundant. Are there different plant types, stem densities, difference in inundation durations, or is this strictly a product of decreased flow velocities related to grass baffling?

Good point. We have now expanded our description of levee response to loading in the introduction which refers to the first text we know that refers to the phenomenon of disproportionate levee growth under higher edge sediment supply (Temmerman et al., 2004) and a more recent one which shows that under low sediment supply, levees do not form (Albernaz et al., 2020). We explicitly name vegetation / larger cross-sectional area which lead to reduced flow velocities and deposition. (tracked-changes lines 77-88)

• How often and to what depth is the marsh platform inundated during neap and spring tides? This knowledge would help the reader evaluate levee development.

Using tidal inundation fraction (as in Morris et al., 2021) we now include an analysis of inundation frequency through time (Fig S5) and refer to that in various areas of the results and discussion, and respond in more detail to this line of inquiry several comments below.

• Why are the scarps higher at the Conserved site and why are they eroding at a faster rate compared to the Deforested site?

There are several reasons we propose this may be due to the higher tidal ranges at the conserved site. Greater tidal oscillations will enhance pore-pressure fluctuations and drive hydraulic gradients between high and low tide, which can then promote seepage-driven undercutting and episodic marsh-edge failure. Also, the larger tidal range and smaller open water leads to a larger tidal prism per unit width, thus likely higher tidal velocities which more effectively flush fine-grained sediments and could enhance edge erosion or at least remove sediment which would otherwise shield the scarp toe. Also, with the sediment loading at the deforested site, we could (and do; Bost et al., 2023) see sediment additions to channel/bay bottoms, thus the higher scarps might reflect less sediment.

• Figure 5 shows sediment accumulation rates for two time periods, along three transect sites for two study areas: 12 scenarios. Of these, 8 of the scenarios are pretty equal with only the remaining four exhibiting much greater rates and these are all for the period of rapid SLR. Could the incrementally slight increase in inundation time during the period of acceleration SLR really explain this, or is it due to other factor(s)?

The available sediment at the deforested site likely helps potentiate the impacts of increased inundation (see the doubling in inundation fraction from 0.2 [~2000-2008] to 0.4 [2020] of interior location at deforested site) which explains the greater rates at the deforested site (three of the four scenarios which exhibit much greater rates), while the fourth scenario is the last 20 years measured at the edge core in the conserved site. There, we expect that as lateral erosion progressively allows the fixed core location draw closer to channel sourced sediments, the edge-benefit is realized. We think that the order of controls is (1) availability of sediment in channel (supply limited) (2) proximity to channel sourced sediments – controlled by lateral erosion (transport limited) and (3) inundation frequency (transport limited). So SLR and inundation is a factor, but not THE factor. We now include in the results where we mention acceleration of SLR and Sedimentation over the whole study period: "sedimentation acceleration at both sites was lower, indicating that over multi-decadal timescales inundation is increasing more rapidly than vertical sediment accretion." (tracked-changes lines 276-282).

• You talk about levee formation inhibiting sediment contribution to interior regions, but your accretion curves show that after the "accretion surge" accretion rates for the three sites become very similar.

It is perhaps the disproportionate growth of the levee at the same time as the interior regions show slowed accretion that is most interesting. Rates do indeed increase towards present. Our results for the inundation fraction seem to explain this. Under fair-weather tidal condition (MHW as high-water level), the inundation fraction towards present at the deforested site is ~0.1 (edge), ~0.2 (middle), ~0.4 (interior) (new Figure S5). Due to its height in the modern tidal frame, the edge location is least flooded, but most proximal to channel sourced sediment, thus some balance in sedimentation is achieved where lower inundation durations is compensated by higher suspended sediment concentrations at the channel-proximal locations. Because the effect of sediment limitation depends on continued levee prominence and thus may be ephemeral, I think it's fair to de-emphasize the influence of the emergent levee in reducing present day sedimentation rates. We now draw attention to this in the discussion, (tracked-changes lines 427-433).

• One of the most important findings in paper receives very little attention. Every one of your six coring sites exhibits accretion rates that do not equate with the present rate of sea level rise. Forget about the influence of levees, your work demonstrates that the marsh will be drowning. This point should more prominent.

We have increased the prominence of this point, including it as a direct comparison in the last paragraph of our results as well as a discussion of the relevance of levees as second-order control if marshes area already broadly drowning/failing to pace SLR. Results (tracked-changes lines 276-282) and Discussion (tracked-changes lines 214-215).

- **Missing the Hein et al 2024 reference.**

Added, now in references.

Reviewer #3 (Remarks to the Author):

Comments in the review report I provided are considered and thoroughly processed by the authors. Additional information is provided as suggested and more importantly the boundaries of the applicability of the studies are now clearly shaped and nuances are brought in.

We thank the reviewer for their consideration of our edits which were largely informed by their prior round of comments.

In my opinion nuances can still be improved in the last section of the main body of text (line 504 in tracked changes document). As in this section shoreline hardening and living shorelines are being discussed in relation to the formation of levees. The latter is studied in the MS, but the relation with shoreline hardening is not being discussed and quantified. So, as this is part of potential wider implications, the right nuances need to be made. There are many possibilities for shoreline hardening, including methods that stimulate sediment accretion just seaward from the marsh scarp, building the full profile. So again, this needs to be nuanced and tied to the right environment and conditions.

We now clarify that shoreline hardening (as an intervention) can indeed counteract the effects of hydraulic disconnectivity that an accretionary levee would create by producing a low energy area where sediment might accumulate, create a pioneer-zone, and enable marsh expansion. This is an important balance to draw attention to, which we feel we have adequately done in this revised version (final paragraph of discussion and below):
"As efforts intensify to slow wetland degradation and coastal erosion through wave-dampening interventions such as living shorelines, consideration should be given to scarped edge topography. If the shoreline hardening creates a depositional region between it and the marsh, raises subtidal or pioneer-zone elevations, and enables seaward marsh expansion, the effects of a hydraulically limiting prominent levee may be sufficiently counteracted. If it does not, shoreline hardening could paradoxically compromise marsh interior health in the pursuit of stabilizing the edge, preventing the edge rollover processes that would otherwise redistribute excess levee material and restore a more typical transgressive marsh profile."

Apart from this last comment, improvements are made as suggested and/or there is a reasonable explanation why choices are being made.