egusphere-2026-1559

Reviewer comments are in bold text and responses are in regular text. Line number refer to locations within the clean version of the revised manuscript. Where sections of text from the manuscript and quoted, additions are shown by red text and deletions are shown by red text with a strikethrough.

To summarise, the main changes that we implemented to address the feedback in both reviews are (1) restructuring the Discussion so that the conclusions being drawn from the results are dealt with in turn (as opposed to a site-by-site discussion); (2) streamlining the results section by moving foraminifera species specific-results to the Supplementary Information; (3) adjusting the breach depth ranges to reflect OC and N results as well as those of the foraminiferal analysis. Detailed explanations of these and other changes are included below.

Review #1

Thank you for the opportunity to review this submission. The paper "*Organic matter composition and origin differ between restored and natural UK saltmarshes up to 100 years after the breach*" aims to both identify methods for dating in extremely heterogenous environments as well as describe OC content/stocks, provenance, and thermal stability in restored saltmarshes. This manuscript begins by identifying a need for better understanding of OC dynamics in MR sites due to claims of OC additionality in C crediting schemes and policies. This paper highlights the need to take a multiple site approach due to the extreme variability across MR sites.

This manuscript exhibits several notable strengths, reflecting a high level of care in study design and analysis. While situated within an established body of work–comparing MR and natural saltmarshes–this manuscript provides considerable advances within the current literature by synthesizing data across multiple MR sites. This is a timely contribution considering the increasing pressures to include costal saltmarshes and restoration areas in GHG reporting inventories and C crediting schemes.

To provide the most useful feedback for this manuscript, I have listed my four most overarching critiques followed by line-by- line comments. I have not spent considerable time regarding grammar, spelling, or errors within the references.

1. **The breadth of information and lack of consistently framed aims make it challenging to clearly identify the study's main objectives. Generally, the overarching aims are only briefly introduced and are not consistently reinforced throughout the manuscript. This is most apparent in the Discussion, which largely reiterates site-specific results rather than synthesising broader patterns, further obscuring the study's main messages.**

This is also exemplified within the title which emphasises OM composition and origin but does not reflect several other key goals addressed in the paper, such as breach identification methodology. Greater clarity in the study's focus would substantially strengthen the manuscript.

First, thank you for the thorough and well-structured review. Our main objective was to compare the biogeochemical properties of the restored and natural study sites, and the foraminiferal method was a means to an end to ensure the restored marsh cores were properly characterised. In using the foraminiferal method, we identified various ways in which the technique could be improved, and these were reported for the benefit of future studies.

The final section of the Results has been changed: first, the foraminifera data is described; second, the biogeochemical variables; finally, Table 1, which replaces Figure 8, is presented. In this table, several different carbon stock values calculated for each site are described. Mentions of labile and recalcitrant organic matter stocks and of recalcitrant organic matter accumulation rates have been removed to streamline the manuscript. The three parts of the Results also follow the logic described in the previous paragraph: the foraminifera data was analysed to identify the breach depth ranges, which in turn allow the biogeochemical variables to be interpreted and carbon stocks to be calculated.

To improve the focus of the Discussion, we have removed section "5.2 Stocks and Accumulation Rates in Restored Marshes" and the section formally called "5.3 Indications of allochthonous versus autochthonous sources" has been renamed to "5.2 Difference in OM composition between restored and natural saltmarshes". We have restructured and rewritten the new section 5.2 to create a more integrated discussion of the biogeochemical variables by making cross-site comparisons using the key drivers of these differences. As picked out in Review #2 these drivers include age, elevation and vegetation but also cover OM decomposition and stabilisation. We work through each of these drivers in turn and bring in site specific examples for each to illustrate the conclusions being drawn.

Section "5.1 Breach identification" has been retained but we have also restructured this section along similar lines to those discussed above (for the details see our response to the comment referring to lines 594-665). Together, we believe that amending the structure of these two sections of the Discussion bring the conclusions being drawn by the study front and centre, as opposed to being hidden among the site-specific details.

The final paragraph of the introduction has also been amended (see our response to the comment in reference to lines 124-135) to make the goals of the study clearer from the outset.

Regarding the title of the paper, we believe it best that the key message of the paper be summarised, even if this leaves out mention of the foraminifera based-breach identification method.

2. **The novelty of the paper could be more clearly framed and better emphasised. While the application of foraminiferal dating in a wetland restoration context is relatively uncommon, the method itself has been applied and discussed previously in restoration contexts (e.g. García-Artola et al., 2016) and is widely used for sediment core dating in closely related fields. In addition, comparisons of OC dynamics between natural and MR sites have been explored in earlier studies. I therefore suggest reframing the novelty away from the method itself and instead emphasising aspects such as the success of its application in this context, any methodological refinements, or new insights gained through its use. Further clarification of how the comparison of OC dynamics between natural and MR sites advances or extends the existing body of literature would also strengthen the manuscript.**

The foraminiferal method has been reframed in the Introduction so that it is presented as a means of allowing for accurate comparisons between natural and restored sites. As far as we know, the method has not been applied to UK sites before, so this does present an opportunity for methodological refinement, but we accept that novel method development is too exaggerated a claim. The 5th paragraph of the Introduction has been amended to reflect this. We have added the reference mentioned in this comment and another that demonstrate how foraminifera were formerly used to study anthropogenic impacts within saltmarshes cores. Additionally, we now make mention of the long-established practice of using saltmarsh foraminifera to reconstruct past sea-level changes.

Furthermore, we place additional emphasis in the 6th and final paragraph of the Introduction upon how the comparison of biogeochemical variables between restored and natural marshes advances our understanding of the field. This includes the fact that we make comparisons across four different restored sites of different ages by applying a wide array of different analyses that are not often brought together to produce a comprehensive discussion of the role saltmarsh restoration can play as a natural climate solution.

3. **While the authors acknowledge some limitations of foraminiferal dating in the discussion, the degree of variability observed in the foraminiferal record raises concerns about the robustness of its application. For example, the variability evident in Fig. 3a makes it difficult to confidently identify transitions or events based solely on the foraminiferal data, yet these interpretations underpin much of the subsequent analysis. Although the**

method can be valuable–particularly when paired with other methods–greater emphasis is needed on how this variability affects uncertainty in the derived chronology when additional methods are not applied. In dynamic restored marsh environments, processes such as sediment mixing, reworking, and bioturbation of forminifera may further complicate interpretation, yet these processes are only briefly touched upon. Clarifying how these uncertainties are accounted for, and how specific events are distinguished from background variability, would strengthen confidence in the interpretation and conclusions. Further clarification is also required regarding the specific metrics and thresholds used to identify breach events from the foraminiferal record. It is currently unclear whether these interpretations are based on presence–absence patterns, changes in abundance, shifts in assemblage composition, diversity indices, or a combination of these measures.

We have restructured and rewritten the first part of the Discussion (i.e., section 5.1 that deals with the foraminiferal analysis) so that we no longer focus on each site individually. Instead, go through each of the different methodological refinements we have identified in turn (see our response to the comment referring to lines 594-665). This now includes an acknowledgment of the potential impacts that bioturbation, sediment mixing and reworking could have (L677-688).

Our responses to the other specific comments (comment relating to lines 255-274) below show how we will make it clearer that the threshold for identifying a breach was the presence/absence of foraminifera (i.e., breach depth in an individual core was the depth of the deepest sample in which foraminifera were first found). This rule was applied to Fingringhoe Wick, Brancaster West and Freiston Shore while the age-depth model results were relied upon for Aldboro Point.

Finally, in keeping with the recommendations in Review #2, we propose adjusting the breach depth range for the site Brancaster West after the introduction of the OC and N data.

4. **Overall, as the manuscript currently stands, it functions both as a methods-focused study and as an investigation of differences in OC dynamics between natural and MR sites. Consequently, the scope is quite broad, and the manuscript feels too large for a single publication. I therefore recommend considering separation into two complementary submissions, each with a clearer focus and more targeted discussion. This approach would allow the authors to more effectively (a) highlight advances in the developed methods, (b) provide sufficient space for detailed interpretation, and (c) improve clarity and coherence, ultimately benefiting the reader's understanding of the work. Implementing this suggestion would largely resolve the comments raised**

above. If the authors prefer not to split the manuscript, I would strongly recommend reducing the breadth of information presented.

We acknowledge that the study had two distinct goals and that the breadth of information presented is therefore vast. However, we prefer not to split the manuscript and, by the approach presented in our responses to main critique number one and to the more specific comments below, believe that the manuscript has been made less unwieldy and more focused. Furthermore, we believe it best that the foraminiferal results be analysed alongside the biogeochemical variables as these provide context that aids the interpretation of the results and, therefore, the recommendations we make on how best this method can be refined. For example, in our responses to Review #2, we propose expanding the breach depth range for Brancaster West so that the results of the OC and N analysis, as well as the foraminifera results, are reflected in the range. Finally, if the study were split in two, there would be a great deal of overlap between the two manuscripts, for the reasons already mentioned, and therefore we strongly believe that a combined version is the best way to present our findings.

Abstract:

33-35: I think a more cautious approach would be to state that the system is just storing more allochthonous C. Stating "…allochthonous organic matter that had undergone the processes of decomposition in an external setting…" assumes that the incoming allochthonous OC was originally labile and the refractory OC observed within the marsh is the product of prior degradation of that labile pool, rather than being refractory at the point of input. Observing a higher proportion of refractory OC does not, on its own, demonstrate a degradation pathway, only a storage outcome.

This sentence in the Abstract has now been amended as advised (L33-34):

"Together, these results indicated that a higher proportion of allochthonous organic matter ~~that had undergone the processes of decomposition in an external setting,~~ was entering these restored marshes"

Introduction:

102-103: Perhaps this should read "relative proportions" or "fractions", as TGA alone does not provide absolute quantities.

This sentence has been amended as follows (L102-105):

"Another important method for characterising the OM present in saltmarshes is via thermogravimetric analysis (TGA), whereby the ~~amounts~~ fractions of labile or recalcitrant OM in a sediment sample can be differentiated and quantified"

105-106: The definitions of labile and recalcitrant organic matter could be framed more carefully. As written, the statement implies that molecular complexity alone governs lability, whereas molecular structure does not necessarily control long-term decomposition or persistence of OM (Schmidt et al. 2011). In the context of TGA, lower-temperature mass loss reflects thermally sensitive compounds that are operationally classified as "labile." Clarifying that these fractions represent thermal stability and prefacing that compounds are "potentially more easily degradable" would differentiate this from biogeochemical lability and would improve conceptual clarity.

The description of thermogravimetric analysis (TGA) has been changed from:

"Labile OM refers to simple molecules that are easily broken down while recalcitrant OM is more complex and stable."

To the following, in accordance with the recommendations provided (L105-108):

"In the context of TGA, labile OM refers to thermally sensitive molecules that burn at lower temperatures and are thus potentially more easily degradable in the environment (Schmidt *et al.*, 2011). On the other hand, recalcitrant OM is only lost at higher temperatures and so represents potentially more complex and stable molecules."

123: See main critique number two above regarding novelty.

The 5th paragraph of the Introduction has been edited extensively. References are made to previous applications of foraminifera-related methods and the goal for this study is framed as methodological refinement, as opposed to novel method development (L132-140).

124-135: The overarching goals for the foraminiferal dating where quite clearly stated. I think a clear statement regarding the goals for biogeochemical analyses is also required due to the vast number of indices and measurements taken within this category. I think this will help guide the direction of the discussion better as well as help clarify the aims to the reader. The addition of the expected outcomes or hypotheses would also add clarity.

The final paragraph of the introduction has now been amended significantly.

Study Sites:

141-143: This sentence could be re-written for clarity; I am not entirely clear on the main message here.

This sentence has been re-written. Formerly, it read:

“Often, studies aiming to investigate the development of restored saltmarshes over time turn for examples of older marshes to areas with storm created breaches in their seawalls, given the relatively recent development of deliberate restoration”

Now, it reads as follows (L159-161):

“As examples of older restored sites, previous studies have looked at marshes restored “accidentally” when storms created breaches in the surrounding sea walls, often as long as a century ago.”

Figure 1: The grey pinpoints are difficult to see; another color, shade, or size could make this easier to see.

Agreed, the grey pinpoints in panel (b) of Figure 1 are hard to make out. To maintain differentiation between the restored and natural sites, the text labels for the restored sites have been changed to black with a red background and the natural reference marsh pinpoints have been changed to white and the text labels to red with a white background. A key has also been added to panel (b) of Figure 1.

In responses to a comment in Review #2, we have also amended Figure 1 so that the seawall breaches within each site are highlighted with turquoise lines.

203-204: Because this site receives tidal flow via piped culverts are there additional factors here not captured by the study that would make this site less comparable to the other sites? I think this needs justification.

We do not believe that tidal flow via piped culverts strongly impeded comparisons because, although they modify the tidal inundation, they do not prevent it and thus the site still experiences the impacts of regular inundation by sea water. The following sentence has now been added as a justification (L223-225):

“This difference does not impede comparisons, however, as the site still experiences regular tidal inundation that drives sediment and OM delivery, sediment redox conditions and plant species zonation”

Furthermore, breach geometry and number also differ between sites, meaning that even the three sites without piped culverts cannot be experiencing exactly the same conditions.

Methodology:

247-248: It says three wide diameter cores were taken but the map shows only two. Is this a typo?

There were two wide diameter core sampling points within each marsh but at each of these locations three replicate wide cores were taken within the same ~30 cm^2 zone to

ensure we had enough material for all the analyses we wished to perform. The relevant sentence (L265-267) has been amended as follows to improve the clarity of this point:

"Within each of the two wide core sampling locations at every site, three replicate wide-diameter cores were always taken from within the same ~30 cm^2 area to ensure enough material was available for all required analyses"

265-268: Is there justification for selecting to a minimum of 50 individuals as a benchmark?

This follows the work presented in Kemp *et al*. (2020). This study is cited in a later sentence within the paragraph but has now been moved into the sentence referred to (L292-294):

"Samples were split into eight equal aliquots and when foraminifera were present, the aim was to count a minimum of 50 individuals for each sample, following Kemp *et al*. (2020)"

255-274: Was there a systematic way for identifying breach depth using foraminifera (i.e. count cut off, point break changes? etc…).

The lowest depth in a core at which any number of foraminifera were encountered for the first time was the criterion used (i.e., the depth at which a sample containing ≥ 1 foraminifera test was encountered; that sample must be preceded at lowered depths exclusively by other samples that were entirely absent of foraminifera). A sentence has been added to clarify this point (L297-300):

"The breach was deemed to have occurred at the lowest depth at which any number of foraminiferal tests (i.e., ≥ 1) were first encountered. This criterion was applied to Fingringhoe Wick, Brancaster West and Freiston Shore while the age-depth model results (see section 3.3.) were relied upon for Aldboro Point."

333-340: This section could benefit from a few points of clarity. While the Introduction and Discussion emphasise OC additionality, this section appears to indicate that OC stocks are calculated across the entire core. It is not clear under which circumstances the full core is used for stock calculations versus only post-restoration sediments. In addition, further clarification would be helpful regarding what is meant by assessments being conducted on a "case-by-case basis."

Section 3.6 (L366) has been rewritten extensively to make the stock calculation methodology clearer.

Results:

The Results section does not report DBD values. Please include these data as it provides transparency regarding stock calculations and chronology estimates.

The DBD values for the narrow and wide cores have been plotted (similarly to the way in which other variables are presented in Figures 4, 5 and 7) and added to the Supplementary Information (Fig. S2 and S3). The wide core dry bulk densities are included because carbon stocks are also calculated for these cores in the new Table 1. Wide core OC and N data are now shown in Figure S6. The DBD values are now referred to on line 369.

384-468: The foraminiferal results section could be streamlined, particularly if the primary purpose of the paper is to delineate OM/OC additionality in MR sites. Furthermore, I think highlighting why using diversity indices are important could clarify what ecological patterns you are examining.

We believe that retaining the detailed descriptions of foraminiferal species assemblage changes across depths is important because it represents a useful resource for assessing the ecology and successions of saltmarsh foraminifera. However, we acknowledge the need to streamline the manuscript to ensure focus is placed upon the key goal of examining OC additionality within restored saltmarshes. Therefore, we have shifted the details into the Supplementary Information while retaining brief summaries (i.e., 1-2 sentences per site) of the key species assemblage trends in the main text. The results moved to the Supplementary Information include the assemblage panels within Figure 3 (i.e., panels c, d, g, h, k, i, o and p) as well as the detailed descriptions of the patterns seen. Retaining descriptions of only the key species assemblage trends within the main text also help to make the ecological patterns being examined clearer.

Figure 3 caption: It is not particularly mentioned how the margins for breach depth were chosen using foraminifera profiles. Also, it does not mention if the shaded regions and light blue line for Aldboro point represent error (standard deviation?).

In the sections of the Results that corresponds to the foraminiferal results of a particular site, the breach depth range that was determined using those results is summaries, usually in the final sentence. The caption of Figure 3 has been changed so that further clarity is given:

"This is based upon the age depth model (Fig. 2) for Aldboro Point and upon the foraminifera results for the other sites (details are discussed in sects. 4.2.1, 4.4.2, 4.2.3 and 4.2.4). In the Aldboro Point panels, the thick line shows the depth estimated from the mean ages while the two thin lines show the depths calculated using the minimum and maximum ages (i.e., 2 σ uncertainties)."

Figure 4: Where possible, using consistent x-axis scales for the same measurements would improve comparability across sites and make the figure easier to interpret. It may also be helpful to briefly remind the reader of the meaning and significance of the blue shaded regions (see comment on Fig. 3 caption).

Figures 4 and 7 have been amended to ensure consistent x-axis scales are being used throughout.

A brief reminder regarding the blue shaded region has been added to the Figure 4 legend but also to the legends of Figures 5 and 7:

"The shaded blue areas denote the depth range at which the breaches may have occurred and is based upon radiometric dating for Aldboro Point and foraminiferal analysis for the other three sites."

Figure 6: This figure contains a large amount of information and is somewhat difficult to interpret in its current form. As there are no data points in the upper right region of the panels, removing the C4 plant reference may help reduce visual clutter. In addition, the very wide uncertainty associated with the C3 plant reference limits its usefulness; including more specific, species-level reference values may provide more meaningful context. Reducing the y-axis range could also help make underlying patterns more apparent.

We agree that the changes suggested would make Figure 6 more readable and comprehendible and are happy to implement each of these points. We have:

1) Removed the C4 plant reference
2) Reduced the y-axis range
3) Included species level references instead of the wide ranges previously being used

Figure 8 (a): Please add what the percentage values mean within the caption. Further, without knowledge of the depth (i.e. if the stock was taken from breach or across the whole length of core) you cannot make meaningful comparisons. Perhaps changing the units to density would resolve this issue.

Figure 8 has been removed and replaced by Table 1. We believe this new table is a good way of providing all the necessary details that explain how each different stock was calculated, as well as allowing us to demonstrate the wide range of different values that can be calculated depending upon the interpretation of the results.

Furthermore, information has now been added to the Methodology (section 3.6) to make the stock calculation method clearer.

Discussion:

586-593: This paragraph would read better if the main points regarding breach depth identification were outline first, followed by key biogeochemical patterns.

The paragraph referred to has been removed and a very brief introduction to the Discussion is given in its place (LXX-XX):

“Using the foraminiferal method to determine breach depth meant that it was possible to make comparisons between restored and natural marsh in terms of the biogeochemical variables that characterise the OM present in their sediment. The consistent differences identified can be used as indicators in restoration (Table S4).”

594-665: This section revisits detailed, site-specific results that have already been touched upon in the Results section. While these examples highlight important caveats associated with using foraminifera for dating, the discussion would benefit from shifting toward a higher-level synthesis that evaluates the method more holistically. In particular, how to distinguish large background variability from meaningful signal would strengthen this section. Additionally, briefly reiterating the novel aspects of the approach and clearly articulating where this study advances methodological understanding would help to better emphasize its overall contribution.

Here, we have integrated the different subheading and now discuss the site-specific differences already covered within a structure that works through each methodological refinement in turn. We have also added more specific recommendations about how to overcome the limitations we encountered during our application of the method. In this way, the contribution our findings make to the field are made clearer. Therefore, section 5.1 has been extensively restructured.

648-650: If the information about the duration of reclamation at the other MR sites exists this would be an important aspect to add to the Study Site section. Because the history of each site is unique and seems to be a important driver of many of the patterns seen within the data, perhaps a visual timeline of reclamation duration, restoration, and any other notable work on the site would be helpful either as apart of Figure 1 or in the appendix.

Thank you for the idea of including a visual timeline of site histories. This would work well for Freiston Shore and Aldboro Point but, to our knowledge, detailed information is not available for Fingringhoe Wick and Brancaster West. For this reason and because the information is already available within the main text, we do not believe that adding this figure would enhance the manuscript.

675: A less definitive term than “rule” might be more appropriate.

The term “rule” has been changed to “tendency for” (L710).

783-803: Please use caution when referring to the term “stability” throughout this paragraph as the TGA only measures thermal sensitivity. The permanence of a compound within a system is largely determined by its environmental context and the ability of microbes to access these compounds.

The sentence on lines 745-747 has been amended accordingly.

Greater caution is also taken in the 4th paragraph of the Introduction when explaining the TGA method.

795: Lehmann et al. 2007 is not found within your references.

In the process of restructuring the Discussion, reference to this publication was removed from the main text.

792: I am unclear what is meant by the comparison to upland soils here. If this is intended to refer to terrestrial soils, this is the first point in the manuscript where they are mentioned, which makes the comparison somewhat unexpected.

The reference to terrestrial/upland soils has been removed from the Discussion. Furthermore, have moved the general explanation of the mechanisms by which organic matter is preserved within minerogenic marshes to the Introduction (following advice given in Review #2; L111-115).

802-803: The interpretation that the allochthonous OM was decomposed externally prior to deposition may be valid, but it implicitly assumes that much of the incoming OM had initially labile components and was subsequently transformed into a more refractory form. However, a proportion of allochthonous OM may be inherently refractory upon input rather than the product of prior decomposition. It is also plausible that both restored and natural marshes receive broadly similar allochthonous inputs, particularly given their geographic proximity, with observed differences instead driven by site-specific factors such as elevation, hydrology, or sediment dynamics that influence OC retention. Considering additional processes alongside external decomposition may provide a more complete explanation for the observed patterns.

After presenting the suggestion that allochthonous OM entering the marsh was initially decomposed in an external setting as one possibility, we have continued with a discussion of the other possibilities that are outlined above (L746-756).

1056-1061: McMahon et al. 2023 has been cited twice.

Thank you for catching this; the duplicate reference has been removed.

Overall, this paper provides an impressive comparison of OC/OM quantity and quality across various natural and MR sites and will prove as a useful contribution to the current literature. The issues outlined above indicate a need for considerable restricting or consolidation, however, the underlying dataset and analyses provide a strong foundation for an impactful paper. I hope these comments are helpful and constructive, and I look forward to seeing the final version of the manuscript.

References:

García-Artola A, Cearreta A, Irabien MJ, et al (2016) Agricultural fingerprints in salt-marsh sediments and adaptation to sea-level rise in the eastern Cantabrian coast (N. Spain). Estuarine, Coastal and Shelf Science 171:66–76. https://doi.org/10.1016/j.ecss.2016.01.031

Schmidt MWI, Torn MS, Abiven S, et al (2011) Persistence of soil organic matter as an ecosystem property. Nature 478:49–56. https://doi.org/10.1038/nature10386

Review #2

Organic matter composition and origin differ between restored and natural UK saltmarshes up to 100 years after the breach

I found this manuscript very interesting and enjoyable to read. The authors have clearly put a great deal of effort into data collection and interpretation, and the study addresses an important topic for understanding organic matter composition differences between natural and restored saltmarshes. It also has clear relevance for evaluating the use of restored sites in carbon credit systems. I have three main overarching comments:

1. **I am not fully convinced that foraminifera alone provide a robust proxy for determining breach depth at Brancaster West. The inferred depth appears inconsistent with other lines of evidence presented in the manuscript and is notably different from a similar-aged site (Fingringhoe Wick). This raises questions about whether additional proxies or corroborating evidence are needed to strengthen the interpretation of breach depth.**

First, thank you for your comprehensive and careful review. Upon reflection, we agree that ignoring the contradictory evidence when selecting the breach depth for Brancaster West does not produce an accurate picture of the site and is likely to leave any readers with more questions than answers. Therefore, in our responses to the comments below (comments referring to line 382 and lines 490-491) you will see that we have increased the breach depth range for Brancaster West after the OC and N data is introduced. This will mean that we are not relaying on the foraminifera data alone for this site. We also include a discussion of the fact that the peaks in the Brancaster West data are likely representative of the non-halophyte pre-breach plant community (L714-718 & 779-782). OC stock data is presented differently in the revised manuscript (i.e., in the new Table 1) and this provided an opportunity for calculating OC stocks that included and excluded the OC peaks, as well as a discussion of whether these peaks can legitimately be included (L718-727).

2. **I agree with the previous reviewer that the discussion would benefit from restructuring. At present, it reads somewhat sequentially, following the order of analyses, which makes it feel closer to a thesis-style presentation. A clearer narrative focus, potentially structured by site and then compared across key gradients such as age since restoration, elevation, or vegetation, would help emphasise the broader story rather than individual analyses.**

We have restructured the “5.1 Breach identification” section so that, instead of discussing each study site in turn, we discuss the different methodological refinements/limitations that we have identified during the study, with site specific examples integrated throughout. For more details, see our response to the comment

referring to lines 823 – 830. Furthermore, we have also restructured section 5.3 (now called "5.2 Difference in OM composition between restored and natural saltmarshes") so that, instead of discussing each variable in turn, we go through the possible drivers (i.e., age, elevation, vegetation and OM decomposition and stabilisation) of the differences in OM composition. Again, the explanations relevant to specific sites have been integrated throughout the section.

3. **Coring locations should be described in greater detail in the study area section, particularly with respect to marsh zone and spatial context within each site, as these are important for interpreting differences between cores used throughout the analyses.**

We have amended Figure 1 so that breach locations are highlighted with turquoise lines and the specific distance of each coring location from the nearest breach has been added as a column in Table S1. These distances are also referenced in the main text of the Study Sites section. Distance from breach is now referred to with more detail within the Discussion when interpreting the results (e.g., L613-614). We did not measure elevation but can used the vegetation data as a proxy (Table S2). This information is also brought into the Discussion in the revised manuscript (e.g., L754-756).

Detailed comments:

50-53: Consider mentioning high productivity here in combination with regular tidal inundation producing waterlogged conditions.

The sentence referred to has been amended as follows (L50-53):

It is the capacity of saltmarshes to continuously accrete sediment and build elevation, ~~in~~ combined~~ation~~ with waterlogged conditions caused by tidal inundation and a high organic matter (OM) input from productive saltmarsh plants ~~regular tidal inundation and high productivity creating producing waterlogged conditions~~, that create~~s~~ an effective organic carbon (OC) sink~~the sink into which organic matter (OM) is deposited.~~

131-132: This sentence ("When measuring the biogeochemical variables…") seems out of place in the introduction.

This phrase has been removed from the last paragraph of the Introduction in the process of rewriting this section.

137: More justification is needed for the selection of sampling locations within each site. It would also be helpful to describe the marsh zone of each coring location, particularly for the wide cores that are used for comparison throughout the study.

See our responses below to the comments in reference to lines 589-591 and 610-612 for detailed information about how we added more insightful descriptions to the Study Sites and Discussion sections.

246: Why was the <50 cm threshold chosen? Please provide supporting references if available.

The following supporting statement has been added to section "3.1 Sample collection" (L270-276):

"Although sedimentation rates of 7.5 cm yr^{-1} have been reported for restored sites in the Severn estuary, far lower values (<2 cm yr^{-1}) have been recorded within east of England restored marshes (Mossman *et al.*, 2022). Furthermore, very high sedimentation rates usually only occur within the first few years following a breach, until the elevation of a site within the tidal frame has increased (Mossman *et al.*, 2022). Therefore, the assumption that no more than 50 cm of sediment had accreted in the younger restored sites was deemed legitimate."

259: Provide manufacturer details for isopropanol for reproducibility.

The isopropyl manufacturer (HEXEAL) has been added to the relevant sentence (L287).

266: Kemp et al. (2020) could be cited earlier when first mentioning the 50-test threshold.

The sentence referred to now has the Kemp *et al*. (2020) added at the end (L292-294):

Samples were split into eight equal aliquots and when foraminifera were present, the aim was to count a minimum of 50 individuals for each sample, following Kemp *et al*. (2020).

285, 287: Provide acid concentrations and manufacturer details for reproducibility.

HF is from NORMATOM (47%) and HNO_3 is from ARISTAR (69%) – this information has been added (L313-314).

294: Please specify the version of R used.

The R version used was 4.4.1 and this information has been added to the radiometric dating section of the Methodology (L323).

311: Provide manufacturer details for sulphurous acid. Also, for consistency, consider using either chemical names or compositions throughout.

The sulphurous acid manufacturer is Fisher Scientific, and this information has been added (L339).

Also, the reference to HNO_3/HF has been changed to "nitric acid and hydrogen fluoride" (L313-314).

310-317: Sulphurous acid treatment may also remove total nitrogen, potentially affecting CN ratios. This should be acknowledged as a methodological limitation.

In the Discussion, we deal with why we do not believe that this methodological limitation would substantially change the conclusions being drawn in the manuscript; see our response to the comment referring to lines 713-717 for details. We have added the following sentence to the Discussion (L705-709) to introduce the possible limitation:

"Treating sediment samples with sulphurous acid can lead to losses of total N (Fernandes and Krull, 2008), if losses did occur and the N content of our samples were higher than recorded, this would only result in lower OC/N ratio values and thus further exacerbate the difference between natural and restored marshes."

316, 323: Please specify whether certified reference materials or standards were used for QA/QC.

Capsules containing acetanilide were run as standards during the EA analysis while capsules containing an internal laboratory standard (collagen) were used during the stable isotope analysis. This information has been added to the parts of the Methodology that describe each of these techniques (L346; L355-357).

358: Add (Fig. 2b) at the end of the sentence. Also ensure consistent notation (e.g., Lead-210 vs ^{210}Pb).

Reference to the figure has been added to the sentence indicated (L395).

The following sentence has been slightly restructured to ensure consistent notation:

"~~Lead-210~~ Activity concentrations of ^{210}Pb decreased…" (L395-396)

366: In Fig. 2, consider adding y-axis values for panels c and d for consistency.

Thank you for pointing this out. We have added y-axis labels to these panels in Figure 2.

382: I am not fully convinced that foraminifera alone are sufficient to determine breach depth. This interpretation may benefit from stronger corroboration with additional evidence, as suggested later in the manuscript.

Based upon the OC and N, the upper limit of the Brancaster West breach depth range has been moved to a higher depth from Figure 4 onwards (given that this when the OC and N data is first introduced). Furthermore, in section 5.1 of the Discussion, greater emphasis is now placed upon the need to supplement the foraminifera analysis with other sources of information (e.g., L652-654).

490-491: Could the lower values observed here be related to a shallower breach depth than suggested by foraminifera?

We now believe that the OC and N peaks in Brancaster West are representative of the pre-breach non-halophyte vegetation formerly present in the site (L715-719). If this is the case, these peaks would be expected after the breach depth because sediment would first settle around the base of the vegetation and only gradually come to cover it

completely. However, we have still adjusted the upper limit of the breach depth to above the peaks as this allows us to calculate OC stocks that both include and exclude these values. This in turn leads us to a discussion of whether of not this vegetation can be legitimately included, an especially important point given the large influence upon the stock calculation result (Table 1).

507: Table 1 may be better placed in the supplementary material, with p-values reported in the text.

We agree that the former Table 1 (now Table S3) can be moved to the Supplementary Materials as this would help to streamline the manuscript and limit the amount of information presented, given that there is already a lot of material. We have reported relevant *p*-values in the main text as we go through the descriptions of each difference.

534: In Figure 6, the overlap in error bars between potential sources should be acknowledged when interpreting results.

As was suggested in Review #1, we have removed the large ranges of potential sources and replaced them with species specific values. This allows us more accurately constrain the likely sources of OM to the cores.

576-578: Could these differences be related to elevation and/or vegetation?

Labile and recalcitrant OM stocks are no longer reported, only OC stocks in the new Table 1. We believe that the higher OC stocks in Brancaster West compared with Freiston Shore were owing to the peaks in OC present in the cores from the former and our interpretation of this fact is outline above in our response to the comments on lines 490-491.

585: The discussion would benefit from reorganisation. A site-based structure, followed by cross-site comparison using key drivers (e.g., age, elevation, vegetation), may help improve narrative clarity. The current structure follows analyses too closely and the order of sites differs from the Results section, which affects readability.

As discussed in our response to Review #1, we have kept (but amend in accordance with responses to other comments) the sections “5.1 Breach Identification” and “5.3 Indications of allochthonous versus autochthonous sources” (renamed “5.2 Difference in OM composition between restored and natural saltmarshes”). Subheadings have been entirely removed from the former section while in the later possible drivers of OM composition differences are used as subheadings. The drivers discussed are age, elevation, vegetation and OM decomposition and stabilisation.

589-591: Were cores within each site collected across different marsh zones or environmental gradients (e.g., distance from creek or breach point)? Were vegetation differences considered?

We aimed to take cores that were evenly distributed across our sites. Plant community compositions were recorded in each 1 m^2 sampling area and these results are summarised in the Study Sites section and reported in full in Supplementary Information (Table S2). Distances from creeks or breaches were not recorded during field work but distances from breach have been calculated using Google Earth and are reported in Table S1 and summarised in the Study Site section. Furthermore, breach locations are now shown in Figure 1.

610-612: Coring locations should be described in more detail in the study area.

Distances of each coring location from the nearest breach have been calculated and reported in the Study Sites section. Individual values for each core have been added to Table S1, and the main text includes references to this information when it is relevant for interpreting the results (e.g., L614-615 & L638-640). Additionally, we have highlighted the locations of breaches within panels c-f of Figure 1.

Furthermore, we have added explicit references to whether the plant communities we identified in the different sampling stations were more indicative of low or high marsh settings (e.g., L755-757, L766-768 & L795-796). As we lack actual measurements of elevation, the plant communities can be used as a rough proxy.

Finally, a figure showing photographs of each site has been added to the Supplementary Information as Figure S1.

612-616: These sentences appear contradictory regarding sediment accretion rates between the two cores.

The first sentence referred to describes the results of another study that measured accretion rates in the site in the 15 months immediately following the breach. Indeed, these results do contradict our findings if the locations of the cores in terms of the former land use are considered in isolation, but we go on to specify that looking at the distances of these sampling locations from the breach explains the contradictory results. We believe this has been made clearer in the newly restructured Discussion (L629-640).

655-657: Consider whether this applies to more than just older sites.

When restructuring and rewriting the section “5.1 Breach identification”, we have included paragraphs that deal with the desirability to using multiple methods – not just the foraminiferal analysis – to constrain the breach depth range as closely as possible (L652-664 & L690-699).

658-659: This method may be more effective when used in combination with other proxies.

Agreed, see our response to the above comment for a summary of how we intend to change this part of the Discussion to make clear the limitations of looking at foraminifera data in isolation.

663-665: Again, more detailed description of coring sites would strengthen interpretation.

See our responses to the comments in reference to lines 589-591 and 610-612.

682-685: Carbon crediting is mentioned but not extensively discussed. A dedicated section or more explicit discussion of implications and limitations may improve clarity.

We believe that the new Table 1 and the restructured Discussion make clearer the implications of our findings for carbon crediting. The culmination of the manuscript is the OC stock calculations that are based upon the foraminifera-derived breach depth ranges. The wide variety of different values produced depending upon the core type and depth used in the calculation are dealt with (L690-699). Furthermore, we include a discussion of the types of organic matter that should or should not be included in OC stock calculations in the Conclusions section of the manuscript.

700-701: This is an interesting result and could be discussed in more depth.

In the interest of streamlining the manuscript, the recalcitrant OM accumulation rate Figure has been removed, and focus is placed upon the OC stock estimates.

711-712: It would be helpful to compare these values directly with thresholds cited for recently deposited marine OM.

Comparison with the threshold reported for recently deposited marine OM has now been added to the Discussion (L764-768).

713-717: Could differences in methodology (e.g., TN vs acidified N measurements) explain discrepancies with previous studies? Differences in vegetation between restored and natural sites may also be relevant.

We were unaware that acidification might result in the removal of N from our samples so thank you for bringing attention to this. Given that the consistent difference between restored and natural saltmarshes was lower OC/N ratios (i.e., OM that is richer in N relative to OC) in the former, we do not believe that this methodological difference is the driver. Even if some N was removed by the acidification process, the inclusion of the true TN value in the OC/N ratio calculations would result in even lower values for the restored marsh sites. See also the related response to the comment for lines 310-317. This point has now been addressed in the Discussion (L705-709).

734-736: This may suggest a shallower breach depth at Brancaster West.

As discussed above we will amend the breach depth range for Brancaster West so that the OC and N results are reflected, as well as the foraminifera results.

757-759: This provides further indication that breach depth may be shallower than inferred from foraminifera alone.

The breach depth range for Brancaster West has been amended, and the OC stock estimates reflect this.

764: Figure S1 appears important and could potentially be moved to the main text or combined with Fig. 7.

After simplifying the individual panels of Figure 6 in accordance with the suggestions made in Review #1, we added the panels currently presented in Figure S1 as smaller insets within each Figure 6 panel. This makes changes in the $\delta^{13}C$ signature across depth more readily apparent to the reader.

775-776: A comparison of site age here would be useful.

References to site ages have been added to the Discussion (L794).

782: These findings are interesting and could be further expanded.

This section has been edited in-keeping with the advice in Review #1 and are now discussed under the section "5.2.2 Elevation".

793: If other sites are organogenic, this should be described in the study site section for clarity.

All of the sites included in this study are minerogenic marshes. The discussion of the mechanisms by which OM is preserved in minerogenic marshes specifically (to avoid confusion) has been moved to the Introduction (L111-115).

796-800: This justification would fit better in the introduction.

See the previous comment.

813-815: Carbon crediting is mentioned but not fully developed in the discussion. Given its importance, a dedicated section may be beneficial.

See our response to the comment in reference to lines 682-685.

823-830: A limitations section would strengthen the manuscript, particularly regarding the use of foraminifera as a standalone proxy.

We believe that by restructuring the “Breach Identification” section and expanding the breach depth range of Brancaster West after the introduction of OC and N data will make the limitations of this method clearer throughout the text without the need to add an

additional section. Furthermore, we propose acknowledging the potential impacts of bioturbation, sediment mixing and reworking within the “Breach identification” section.

Grammar:

444: Change “expect” to “except”
542: Change “expect” to “except”

Thank you for pointing out these errors; both have been fixed.

I hope these comments help improve the manuscript. It is already a very interesting and important piece of work, and with some restructuring and additional clarification, I believe it will make an excellent final publication.