**egusphere-2026-1559**

*Reviewer comments are in bold text and responses are in regular text. We have addressed minor comments directly by changing the text within the manuscript. Minor deletions in passages from the manuscript are shown in red with a strikethrough (e.g., "~~amounts~~") and minor additions are shown in red (e.g., "fractions"). Where passages have been rewritten substantially, the before and after are shown in full. With large changes that would require the restructuring and/or rewriting of large sections of text, we instead specify how we intend to go about implementing the suggestions of the reviewers.*

*In summary, the main changes that we propose 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.

To improve the focus of the Discussion, we suggest keeping sections "5.1 Breach Identification" and "5.2 Stocks and Accumulation Rates in Restored Saltmarshes" but removing the subheadings from "5.3 Indications of allochthonous versus autochthonous sources". We suggest renaming this section to "5.3 Difference in OM composition between restored and natural saltmarshes". Furthermore, we propose restructuring and rewriting elements of this section to develop a more integrated discussion of the biogeochemical variables by making a cross-site comparison using the key drivers discussed. As picked out in Review #2 these drivers would include age, elevation and vegetation but could also cover OM decomposition and stabilisation. We would work through each of these variables in turn and bring in site specific examples for each to illustrate the conclusions being drawn.

While we would keep the section "5.1 Breach Identification", we suggest restructuring 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 would 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 will be 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. We have added the reference mentioned in this comment and another that demonstrates how foraminifera have formerly been used to study anthropogenic impacts within saltmarshes cores. The following sentences were added to the 5th paragraph of the Introduction:

"Foraminifera have previously been used as proxies to identify human disturbance in saltmarsh sediment (e.g., García-Artola *et al*., 2016; Mueller-Navarra *et al*., 2019) but the application of the technique in this study presented an opportunity for its refinement and an assessment of its performance in saltmarshes restored via seawall breaching."

Additional reference: Mueller-Navarra, K. *et al.* (2019) 'Evolution of a salt marsh in the southeastern North Sea region – Anthropogenic and natural forcing', *Estuarine, Coastal and Shelf Science*, 218, pp. 268–277. Available at: https://doi.org/10.1016/j.ecss.2018.12.022.

Furthermore, we will place additional emphasis in the Introduction upon how the comparison of biogeochemical variables between restored and natural marshes advances our understanding of the field. This will include 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 in play as a natural climate solutions.

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 propose restructuring and rewriting the first part of the Discussion (i.e., the section that deals with the foraminiferal analysis) so that we move away from a structure that focuses on each site individually. Instead, we will structure this section as a discussion of each of the different methodological refinements, most of which we have already mentioned in the Conclusions section (see our response to the comment referring to lines 594-665).

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.

Furthermore, we propose acknowledging the potential impacts of bioturbation, sediment mixing and reworking. These factors could mean that the foraminiferal assemblages found within a centimetre of sediment represent a mixture of populations formerly present upon the marsh surface during very different periods of its development, as opposed to representing just one period. Furthermore, bioturbation can enable the passage of electron acceptors (e.g. sulphate) into deeper sediment layers and thus allow for organic matter mineralisation, acid production and calcium carbonate dissolution (i.e., the destruction of foraminiferal tests; Berkeley *et al*., 2007). Another possible limitation that could be addressed is that foraminifera can live down to 50 cm deep (or deeper) in the sediment/soil (e.g., Patterson *et al*., 2004; Tobin *et al*., 2005; Duchemin *et al*., 2005; Milker *et al*., 2015). This could influence the fossil assemblages if recent infaunal species lived deeper in the sediment. Most species are found at shallower depths and hence the impact of infaunal species should not large, but we believe it is another element of the methodology that should be acknowledged.

Additional references:

Berkeley, A. *et al.* (2007) 'A review of the ecological and taphonomic controls on foraminiferal assemblage development in intertidal environments', *Earth-Science Reviews*, 83(3), pp. 205–230. Available at: https://doi.org/10.1016/j.earscirev.2007.04.003.

Tobin, R. *et al.* (2005) 'Infaunal benthic foraminifera in some North American marshes and their influence on fossil assemblages', *Journal of Foraminiferal Research*, 35(2), pp. 130–147. Available at: https://doi.org/10.2113/35.2.130.

Patterson, R.T. *et al.* (2004) 'The distribution of salt marsh foraminifera at Little Dipper Harbour New Brunswick, Canada: implications for development of widely applicable transfer functions in sea-level research', *Quaternary International*, 120(1), pp. 185–194. Available at: https://doi.org/10.1016/j.quaint.2004.01.017.

Duchemin, G. *et al.* (2005) 'Foraminiferal microhabitats in a high marsh: Consequences for reconstructing past sea levels', *Palaeogeography, Palaeoclimatology, Palaeoecology*, 226(1), pp. 167–185. Available at: https://doi.org/10.1016/j.palaeo.2005.05.009.

Milker, Y. *et al.* (2015) 'Variability of intertidal foraminiferal assemblages in a salt marsh, Oregon, USA', *Marine Micropaleontology*, 118, pp. 1–16. Available at: https://doi.org/10.1016/j.marmicro.2015.04.004.

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, thus making our interpretation of our results more nuance, accurate and justifiable.

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 can be 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:

"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:

"Another important method for characterising the OM present in saltmarshes is via thermogravimetric analysis, 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:

"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.**

Previously, in reference to the foraminifera breach identification method, the sentence on the line read:

"However, the usefulness of this method is yet to be investigated."

Instead, the following, more nuanced statement has been put in its place and the additional reference provided has been added:

"Foraminifera have previously been used as proxies to identify human disturbance in saltmarsh sediment (e.g., García-Artola *et al.*, 2016; Müller-Navarra *et al.*, 2019) but the application of the technique to this study presented an opportunity for its refinement and an assessment of its performance in marsh restored via seawall breaching."

**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:

"The aim of this study was ~~is~~ to compare restored and natural saltmarshes with a view to identifying and explaining consistent differences in biogeochemical variables and thus determine the potential impact of any differences upon the efficacy of using restoration as a climate change mitigation strategy. We took sediment cores from four restored saltmarshes in the UK and combined the new data we collected with an existing database of natural marsh reference sites. The variables quantified were OC, N and $\delta^{13}C$ content – the former two allowing C/N ratios to be calculated – and the fractions of labile and recalcitrant OM, based upon TGA. To achieve our overarching goal, the first objective was to determine the depth of the breach in the sediment cores using foraminifera. In doing so, conclusions were drawn about possible refinements to this method ~~and, in doing so, establishing the reliability of this method~~. Following the foraminifera analysis, ~~When measuring the biogeochemical variables of interest~~ was possible to know which core sections were representative of pre- and post-breach settings ~~saltmarsh~~. Therefore, the sediment variables examined could be compared across sites and the possible reasons for any differences or similarities attributed to the stages of marsh development. We also used the breach depth estimates to calculate carbon stocks for the post restoration sections of the cores. ~~to demonstrate the potential usefulness of this technique for carbon crediting schemes. Finally, we make recommendations about how these differences can be understood and utilised."~~

**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:

"As examples of older restored sites, previous studies have used marshes to which tidal inundation was restored by storm created breaches in sea walls, often as long as a century ago, given the relatively recent development of deliberate restoration."

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

Agree, 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 red and the natural reference marsh text labels and pinpoints have been changed to white.

In responses to a comment in Review #2, we will also amend Figure 1 so that the seawall breaches within each site are highlighted.

**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 impede comparisons because, although they modify the tidal inundation, they do not impede it and thus the site still experiences the same conditions. The following sentence has now been added as a justification:

"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 has been amended as follows to improve the clarity of this point:

"Within the two wide core sampling locations at each 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:

"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 (i.e., a sample preceded at lowered depths by other that were entirely absent of foraminifera. A sentence has been added to clarify this point:

"Generally, the breach was deemed to have occurred at the lowest depth at which any number of foraminiferal tests were first encountered. This rule 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."

In Aldboro Point, the older site, a series of samples consistently lacking any foraminifera were not identified but there was a clear fall in the number of tests within the breach depth range in core AP23/1 (i.e., the core used to develop the age-depth model) and this is highlighted within the manuscript and taken as support for the model based breach depth range.

**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."**

These points have now been clarified with the following amendments:

"To calculate OC and OM stocks we used the narrow cores data and followed Howard *et al*. (2014) by multiplying the carbon density of a segment by its length, summing these values for the ~~entire~~ length of a core that was deemed representative of a saltmarsh ecosystem. ~~and, f~~Finally, taking an average of the totals of each core within a site. The sections of the cores taken to represent the post-breach marsh was determined upon a case-by-case basis: for Aldboro Point the radiometric dating results were used and for the other sites the foraminiferal analysis was the basis of this determination. Accumulation rates for Aldboro Point were calculated by multiplying the radiometric dating derived sediment accretion rates for each cm of sediment with the OC density of each individual cm."

**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 cores will be plotted (similarly to the way in which other variables are presented in Figures 4, 5 and 7) and added to the Supplementary Information. These values can then be referred to and described in the "Estimating Organic Carbon Stocks and Accumulation Rates" section.

**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 propose shifting the details into the Supplementary Information while retaining brief summaries (i.e., 1-2 sentences per site) of the key species assemblage trends. The parts moved to the Supplementary Information would 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 described within the Results section. Retaining descriptions of only the key species assemblage trends within the main text will 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 in summaries in the final sentence. The caption 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 (discussed in sections 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).**

For readability, we believe it best to keep all the figures except for Aldboro Point on a 50 cm y-axis. We will ensure that y-axis labels are included for the first panel within each site set to improve clarity.

A brief reminder regarding the blue shaded region has been added to:

"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 will:

1) Remove the C4 plant reference
2) Reduce y-axis range
3) Include species level references instead of the wide ranges presently being used so that other reference datapoints are more similar to those currently shown for zooplankton and phytoplankton

**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.**

The following sentence has been added to the caption to explain the percentage values:

"The percentage values in panel (a) represent the size of the restored marsh OC stock relative to that of its natural counterpart."

The stock was calculated from the depth of the breach in each case, and this information has now been added to the Methodology section.

**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 rearrange accordingly:

There was often variability between the cores taken from different locations within the same site and this meant that the breach depth ranges for two undated sites, Brancaster West and Fingringhoe Wick, were broad. Furthermore, considering evidence beyond the foraminiferal populations would mean extending the potential breach depth further yet. Persistent differences were apparent in the composition of the OM in the sediment of restored marshes compared to natural counterparts, suggesting differences in OM source that continued long after the creation of the breach. Consistency in these differences mean they can be used as indicators in restoration (Table S3). ~~There was often variability between the cores taken from different locations within the same site and this meant that the breach depth ranges for two undated sites, Brancaster West and Fingringhoe Wick, were broad. Furthermore, considering evidence beyond the foraminiferal populations would mean extending the potential breach depth further yet.~~

**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 suggest integrating the different subheading and discussing the site-specific differences already covered within a structure that works through each of the recommendations that are currently covered in the Conclusions section, as opposed to one that deals with each site separately. In addition, we would add more specific recommendations about how to overcome the limitations we encountered during our application of the method. For example, a discussion of the large background variability highlighted within this comment would focus upon the results of Fingringhoe Wick and Brancaster West, in particular. We already state that taking a higher number of cores would help to constrain the breach depth range and show how different parts of a site vary from one another. To develop this recommendation further, we would include a discussion of how species level data could be used to improve the interpretation of the results (e.g., the predominance of calcareous vs agglutinated species).

Additional points that would be included in this reworked Discussion section are outlined in the third paragraph of our response to major comment 3.

**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".

**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 following sentence has been changed accordingly:

Taken together, these factors lend support to the idea that the OM present in these surface sediments ~~had already aged and stabilised~~ was thermally insensitive and so potentially more stable and complex.

Furthermore, the final sentence has been amended from:

"The higher recalcitrant OM content of the surface sediments was an additional piece of evidence that suggested a higher allochthonous contribution to the restored marshes, given that the labile component of this material was likely lost during decomposition in an external location before being washed into the restored sites."

To:

"The higher recalcitrant OM content of the surface sediments could also suggest that a higher allochthonous proportion of allochthonous OM entered the restored marshes, given that the labile component of this material can be lost during decomposition in an external location before being washed into the restored sites. Alternatively…"

See the discussion of the comment for lines 802-803 for an explanation of how we propose to continue this section.

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

The following reference will be added to the list:

Lehmann, J., Kinyangi, J. and Solomon, D. (2007) 'Organic matter stabilization in soil microaggregates: implications from spatial heterogeneity of organic carbon contents and carbon forms', *Biogeochemistry*, 85(1), pp. 45–57. Available at: https://doi.org/10.1007/s10533-007-9105-3.

**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. We proposed moving the general explanation of the mechanisms by which organic matter is preserved within minerogenic and organogenic marshes to the Introduction (following advice given in Review #2). This will allow references to made to these mechanisms in the Discussion that appear less unexpected to readers.

**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 (see our response to the comment for lines 783-802), we suggested continuing this paragraph with a discussion of the other possibilities that are outlined above. As observed, including a discussion of OM of an inherently more recalcitrant nature entering the restored sites and the possibility that the same amounts of recalcitrant OM were entering both sites but that more was retained within the sediments of the restored sites, will produce a more complete and interesting explanation.

**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**