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 #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 intend to increase the breach depth range for the site 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.

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.**

Yes, we agree that the manuscript could be made more focused and that doing so would lead to significant improvements. We propose restructuring the "5.1 Breach Identification" section so that, instead of discussing each study site in turn, we will discuss the different methodological refinements/limitations that we have identified during the study, with site specific examples integrated throughout. For specific examples of methodological limitations that we intend to deal with, see our response to the comment referring to lines 823 – 830. Furthermore, we propose restructuring the "5.3 Indications of allochthonous versus autochthonous sources" section so that, instead of discussing each variable in turn, we will go through the possible drivers (i.e., age, elevation, vegetation and OM decomposition and stabilisation) of the differences in OM composition. Again, the explanations that derive from specific variables will be integrated throughout. We also propose renaming this section to "5.3 Difference in OM composition between restored and natural saltmarshes".

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 will add details about the marsh zones from which cores were taken (based upon using the vegetation data as a proxy) and we will specify the distances of each coring location from the relevant breach and highlight breach locations within Figure 1. For details, see our responses to the comments in reference to lines 589-591 and 610-612.

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:

It is the capacity of saltmarshes to continuously accrete sediment and build elevation, ~~in~~ combinedation 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. The sentence now reads as follows:

"Following the foraminifera analysis, it was possible to know which core sections were representative of pre- and post-breach settings."

We believe it is legitimate to refer to a particular analysis here as this paragraph gives a brief summary of what the study undertook to do before going into the details of the Methodology 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 intend to add more insightful descriptions to the Study Sites section.

The selection of sampling locations was based upon achieving a wide spatial distribution of cores from across the site, as is reflected in Figure 1.

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

The following supporting statement has been added to the end of the section "3.1 Sample Collection" within the Methods:

"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.

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:

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.

294: Please specify the version of R used.

The R version used was 4.4.1 and this information has now been reported within the manuscript.

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.

Also, the reference to HNO_3/HF has been changed to "nitric acid and hydrogen fluoride".

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 propose adding the following sentence to that part of the Discussion (as opposed to including it in the Methodology) to introduce the possible limitation:

"Treating sediment samples with sulphurous acid can lead to losses of total N (Fernandes and Krull, 2008)."

Additional reference: Fernandes, M. and Krull, E. (2008) 'How does acid treatment to remove carbonates affect the isotopic and elemental composition of soils and sediments?', *Environmental Chemistry*, 5(1), pp. 33–39. Available at: https://doi.org/10.1071/EN07070.

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 collagen were used during the stable isotope analysis. This information, along with the manufacturer details, has been added to the parts of the Methodology that describe each of these techniques.

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.

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

"~~Lead-210~~ Activity concentrations of ^{210}Pb decreased…"

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

Thank you for pointing this out. We will add y-axis labels to these panels.

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.

We believe that the only case in which the breach depth ranged used could be altered by the consideration of additional information is that of Brancaster West. Based upon this information, upper limit of the breach depth range would be moved to a higher depth, and we suggest showing this from Figure 4 onwards (given that this when the OC and N data is first introduced).

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

Yes, these lower values in combination with the interpretation of the preceding peaks in OC and N values as representing the agricultural root mat layer would suggest a shallower breach depth than what is suggested by the foraminifera data alone. We suggest expanding the breach depth range so that the upper limit is moved to a higher depth.

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

We agree that Table 1 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 also propose reporting relevant, significant *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 propose removing the large ranges of potential sources and replacing them with species specific values (i.e., so that all the potential source datapoints resemble the way in which the zooplankton and phytoplankton sources are currently represented). This would allow us more accurately constrain the likely sources of OM to the cores.

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

Thank you for this suggestion. We lack quantitative information about the elevation of the sites but, based upon the plant communities present, it seems likely that Brancaster West, in which *Spartina anglica* dominates, is situated at a lower elevation than Freiston Shore and thus receives more regular tidal inundation. However, upon examination, we believe that the higher OC stock in Brancaster West compared with Freiston Shore, while the opposite was true of recalcitrant OM stock, is owing to the peaks in OC that we attribute to the agricultural root mat layer, evidence of which was absent from the OM profile. We acknowledge that this agricultural root mat layer should not be included in the carbon stock estimates and have already proposed expanding the potential breach depth range for Brancaster West (see responses to comments relating to lines 382 and 490-491). In additional we propose altering Figure 8 (panels a and b) so that the stock calculations show the upper and lower limits of the breach depth ranges for each site.

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 will keep (but amend in accordance with responses to other comments) the sections “5.1 Breach Identification” and “5.2 Stocks and Accumulation Rates in Restored Saltmarshes” but remove the subheadings from “5.3 Indications of allochthonous versus autochthonous sources” and develop a more integrated discussion of the biogeochemical variables by making a cross-site comparison using the key drivers discussed. These drivers would include age, elevation and vegetation but could also cover 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 primarily aimed to take cores that were relatively 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 distance from breach can be calculated using, for example, Google Earth (see our response to the next comment).

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

Distances of each coring location from the relevant breach will be calculated and reported in the Study Sites section. Individual values for each core will be added to Table

S1, and the main text will describe the distances of the wide core sampling locations, given that these cores were most important for interpreting the OM source signals. Additionally, we propose highlighting the locations of breaches within panels c-f of Figure 1.

Furthermore, we will add explicit references to whether the plant communities we identified in the different sampling stations are more indicative of low or high marsh settings. As we lack actual measurements of elevation, the plant communities can be used as a rough proxy.

Finally, we suggest that a figure showing photographs of each site could be added to the Supplementary Information.

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 specified in the preceding sentence that looking at the distance of these sampling locations from the breach explains the contradictory results. This will be made clearer when the Discussion is restructured along the lines already detailed.

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

When restructuring and rewriting the "Breach Identification" section of the Discussion, we will include a paragraph that deals with the desirability to using multiple methods – not just the foraminiferal analysis – to constrain the breach depth range as closely as possible. In doing so, we will acknowledge that this was relevant to the oldest site, Aldboro Point, because of the lack of samples entirely absent in foraminifera but also to Brancaster West. In the case of Brancaster West, the OC and N offered a breach depth outside the range covered by the foraminifera analysis alone.

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.

Agreed, after the new, integrated section dealing with autochthonous and allochthonous sources, we propose to add a section to the Discussion entitled "Implications for carbon crediting". In this section, we will deal in greater depth with the factors mentioned in the Conclusions section (e.g., additionality of the OC in restored saltmarshes).

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

A possible reason for this increasing rate of recalcitrant OM accumulation could be that OM classed as recalcitrant according to the thermal sensitivity test performed by thermogravimetric analysis was in fact vulnerable to microbial decomposition and was thus being broken down and lost as the OM was buried. We propose adding this suggestion to the Discussion as this would also ensure a more nuance discussion of the TGA results (as has been emphasised in Review #1).

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

We will add a sentence that specifies which samples from which depths and cores fall below the threshold C/N ratio value of ten.

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 C/N ratios (i.e., OM that is richer in N relative to C) 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 C/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.

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

Agreed, 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.

Yes, this again supports the interpretation that the OC and N peaks are indicative of an agricultural soil layer and that the breach occurred at a shallower depth. We will amend the breach depth range for Brancaster West.

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

Given that we propose simplifying the individual panels of Figure 6 in accordance with the suggestions made in Review #1, we believe that the panels currently presented in Figure S1 could be added as smaller insets within each Figure 6 panel. This would make changes in the d13C signal across depth more readily apparent to the reader.

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

The following has been added to the sentence referenced in this comment:

"Therefore, Freiston Shore, a 21-year-old site at the time of sampling, must also receive an input of allochthonous OM."

We will endeavour to discover the ages of the natural marshes referenced in the subsequent sentences and add this information to allow for comparisons.

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

This section has been edited in-keeping with the advice in Review #1. Furthermore, we propose to add a more nuanced discussion of the possible reasons for the higher proportion OM classed as recalcitrant in the restored marshes. Specifically, we will discuss the possibilities that OM of an inherently more recalcitrant nature entered 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.

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. This information can be added to the Study Sites section and the discussion of the mechanisms by which OM is preserved in different marsh types can be moved to the Introduction, making references to these mechanisms within the Discussion less unexpected.

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. As stated in our responses to Review #1, 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 reference: 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.

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.