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

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.

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.

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.

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.

**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.

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

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.

123: See main critique number two above regarding novelty.

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.

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

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

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.

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

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

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

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."

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

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.

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?).

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).

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.

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.

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

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.

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.

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

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.

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

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.

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.

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

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