Response to Reviewer Comments for Manuscript

Carbon dioxide release driven by organic carbon in minerogenic salt marshes

egusphere-2025-4621

We would like to thank the reviewers for their appraisal of our revised manuscript. To address the remaining comments of reviewer 1, we have revised the text in the manuscript and the changes are provided here. The comments by the reviewer are in normal front, our reply (author comments) are in *italic* front, and the Revised/added text are in green. All line numbers in the revised text refer to the revised manuscript.

Reviewer 1:

The manuscript has been substantially improved in response to reviewer comments, with clearer framing of the study objectives, strengthened discussion of experimental limitations, and improved transparency in the statistical and methodological descriptions. In particular, the authors have moderated previously strong conclusions regarding the role of electron acceptor availability in regulating organic carbon (OC) decomposition, clarified that turnover rates of terminal electron acceptors were not directly measured, and reframed several interpretations to emphasise consistency with OC limitation rather than definitive exclusion of alternative controls. The discussion of methane dynamics has also been strengthened through the inclusion of analytical detection limits and expanded consideration of potential methane sink processes, which improves confidence in the interpretation of the absence of detectable CH_4. Furthermore, the ecological relevance of the short-term OC manipulation experiment is now more clearly articulated, with appropriate acknowledgement that the pulse-style additions represent mechanistic tests rather than direct analogues of natural OC supply regimes. Statistical reporting has also been improved through the addition of a summary workflow in the main text, clearer reporting of sample sizes and variability, and improved figure annotations.

Author comments: *We appreciate the positive feedback from the reviewer.*

A small number of minor revisions are still recommended to further strengthen the manuscript. Specifically, the discussion of electron acceptor limitation would benefit from slightly more cautious wording acknowledging that concentration-based evidence cannot fully exclude terminal electron acceptor constraints in the absence of rate measurements.

Author comments: *We note the reviewer's concern. We have now modified the text to soften the wording further about electron acceptor limitation.*

Abstract: In the abstract, we now present the observations first and soften the wording:

Line 22-25: Overall, we found that electron acceptors, primarily sulfate (SO_4^{2-}), were present at all tested depths and no CH_4 was detected, suggesting that electron acceptor availability was unlikely to be the primary limiting factor on microbially mediated CO_2 release; the availability of OC (concentration and composition) may rather act as a limiting factor.

Section 4.1: In the discussion of the geochemical measurements of the porewater and solid phase sediment, we have softened the wording and added a caveat. We have also decreased redundant text:

Line 596-600: Based on the high concentrations of SO_4^{2-} with relatively low changes with depth as well as the lack of detectable CH_4, we suggest that electron acceptors may not be limiting the microbial turnover of OC and release of CO_2 in our study. To our knowledge, this is rarely reported for coastal wetlands and not commonly expected for terrestrial ecosystems. Here, we caution that further measurements of rates of electron acceptor turnover and/or incubation experiments are needed to unambiguously exclude an electron acceptor limitation.

Section 5: In the conclusion, we have replaced the word 'determine' that indicates unambiguity with the word 'drive' and the word 'controlled' is replaced with 'influenced'. The line 'Overall,…OC composition, rather than concentration alone, limits the CO_2 release from minerogenic salt marshes' is cut.

Line 726-739: Our study demonstrated that the composition in combination with the concentration of OC can drive the CO_2 release from minerogenic salt marshes typical of the Wadden Sea. Initial porewater and sediment geochemical characterization suggested that microbially mediated CO_2 release is likely not limited by the availability of electron acceptors in both the pioneer marsh and intertidal flat, contrary to what is generally observed in terrestrial wetlands. Overall, our results indicate that the OC composition, rather than the concentration alone, influenced CO_2 release in both succession zones. This suggests that OC composition likely plays a limiting role in microbially mediated CO_2 release from minerogenic salt marshes. We caution here that we did not directly measure TEA reduction rates. Future studies should investigate turnover rates, potentially utilizing isotopes to confirm our findings. The higher CO_2 release observed in the acetate treated plots within the pioneer marsh was accompanied by higher levels of reduced iron. This pattern also corresponded with greater activity of Fe(III)-reducing bacteria in these plots, indicating that microbially mediated CO_2 release resulted from Fe(III) reduction driven by increased labile OC input. The addition of the complex OC (humic acid) did not exceed the CO_2 release of the control, showing that complex OC was not decomposed. Similar trends in CO_2 release were measured for the intertidal flat, further indicating that OC (both in terms of composition and concentration) is a key driver of microbial decomposition of OC to CO_2 for salt marsh systems.

Similarly, interpretations linking the absence of methane directly to non-limiting electron acceptor availability should continue to emphasise remaining uncertainties associated with methane oxidation and analytical detection sensitivity.

Author comments: *We are not entirely sure which uncertainties exactly the reviewer is referring to other than the ones we have specified in the revised version, but we note the reviewer's concern. We have now added a few lines to note the analytical detection sensitivity and contextualized it with respect to other reports of porewater methane concentration. We also made small edits to specify that we expect little AOM within our tested depths but it could be occurring below.*

Line 584-596: We did not measure any CH_4 as a efflux or in the porewater over multiple field campaigns, similar to a study conducted at the same study site by Kubeneck et al. (2025). We considered the sensitivity of detection: the detection limit for porewater CH_4 was 0.53 ppm (Table S3) which corresponds to 1.3 μmol/L based on our sampling method. We would expect that CH_4, if produced within the depths examined here, would be above this concentration as porewater concentrations of CH_4 in wetlands further inland within the Elbe estuary were much higher (0.16 to 2.46 mmol/L, Kubeneck et al 2025). It is possible that trace amounts of CH_4 (ppb) were present below the detection limit. The few other studies that have detected CH_4 in the Wadden Sea were at depths where SO_4^{2-} was largely depleted (Røy et al., 2008; Wu et al., 2015), which is not the case in our study. Furthermore, the absence of an observed decrease in SO_4^{2-} concentration, particularly in the pioneer marsh, suggest a lack of AOM until 50 cm, as CH_4 and SO_4^{2-} are consumed in a 1:1 stochiometric ratio during sulfate AOM. Thus, our results indicate that CH_4 production and consumption are unlikely until 50 cm. Below these depths, these processes may be occurring. Further analysis of the microbial community and/or CH_4 injection experiments would help to determine if methanogenesis and AOM occur at lower depths.