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 reviewer for their thoughtful comments and feedback. We revised the manuscript thoroughly and the changes are provided here: comments by the editor and reviewer in normal front, our reply (author comments) in *italic* front, and the revised/ added text in green.

Reviewer comment 1

This manuscript presents a well-designed and timely study investigating controls on organic carbon (OC) decomposition and CO_2 release in minerogenic salt marsh sediments of the Wadden Sea. The work addresses an important gap in the blue carbon literature, which has historically focused on peat-dominated North American marshes. The combination of detailed geochemical characterization with an in situ organic carbon manipulation experiment is innovative and provides new insight into the relative roles of electron acceptors and OC quality in regulating greenhouse gas dynamics.

The manuscript is generally well written, logically structured, and well-grounded in relevant biogeochemical literature. The conclusions are consequential and should interest the coastal biogeochemistry, blue carbon, and wetland modelling communities.

Author comments: *Thank you for the positive feedback!*

Reviewer comment 2

However, several aspects of the experimental design, interpretation, statistics, and framing require clarification or strengthening before publication. Some assumptions remain insufficiently justified, and a few conclusions appear stronger than the presented evidence supports.

The conclusion that electron acceptor availability, particularly sulphate, does not limit organic carbon (OC) decomposition is not fully supported by the data presented. First, the study relies solely on concentration profiles of sulphate, Fe(II), and O_2, but concentrations alone cannot indicate whether terminal electron acceptors (TEAs) were saturating or limiting for microbial respiration; resolving TEA limitation requires information on rates of sulphate and iron reduction or O_2 fluxes, which were not measured. Additionally, the OC addition experiment demonstrates stimulation of CO_2 production by acetate but does not distinguish whether this reflects true OC limitation or an unquantified shift in TEA-dependent respiration pathways, since no sulphate or iron-reduction rate measurements were performed to assess potential changes in electron acceptor turnover. To strengthen the conclusion, the authors should either moderate their claim or provide additional justification such as discussion of expected TEA turnover dynamics in minerogenic marshes, reaction–transport considerations, or clear reasoning as to why concentration data alone can rule out TEA limitation. As written, the conclusion overstates what can be confidently inferred from the available evidence and should acknowledge these uncertainties.

Author comments: *The reviewer brings up an important and valid point. It is true that our conclusion in only based on concentrations and no* O_2*, Fe(III),* SO_4^- *turnover rates were determined. Based on different lines of evidence, we concluded that TEAs likely did not limit OC decomposition: First,* SO_4^{2-} *remained high throughout the sampled depth and the sulfate:chloride ratio stayed stable (only a slight decrease was observed in the intertidal flat). This indicates no strong* SO_4^{2-} *depletion with depth. We considered whether the lack of observed decrease was due to continuous resupply of* SO_4^{2-} *with infiltrating tidal water. However, the fine particle size of the sediment suggests that tidal water does not percolate completely through the sediment, even over multiple tidal cycles. Thus, we should have been able to observe* SO_4^{2-} *consumption due to microbial reduction if occurring.*

Second, the OC addition experiment showed a clear response due to the addition of labile OC (acetate). We agree that these results cannot exclude potential shifts in TEA respiration pathways; however, the rapid increase of CO_2 *release after addition of acetate (in some injection cycles already 1.5h after addition) combined with no depletion of* SO_4^{2-} *or observed changes in TEA availability suggest that microorganisms capable of utilizing acetate were already present in the sediment, and is consistent with electron donor limitation rather than electron acceptor limitation.*

Third, porewater and solid-phase respiration end products e.g., Fe(II) and/or sulfide levels give evidence that Fe(III) and SO_4^{2-} *reduction was occurring. This suggests that these electron acceptors were available and utilized. Furthermore, the high* SO_4^{2-} *concentrations may suppress methane (*CH_4*) formation. The absence of* CH_4 *therefore suggests that electron acceptors were not depleted, as a thermodynamically more favorable electron acceptor was still available.*

In summary, we suggest that there are indications of an electron donor limitation. We agree that the absence of rate measurement introduces uncertainty and we will add a paragraph in the conclusion (line 659 and below) describing that our data do not fully exclude TEA depletion but are consistent for OC limitation, in addition to softening the conclusions.

Revised/ added text:

Line 22 and below: Overall, we found that the microbially mediated CO_2 release was likely limited by OC availability and composition, and electron acceptor availability was unlikely to be the primary limiting factor, as evidenced by the presence of aqueous sulfate (SO_4^{2-}) at all tested depths and the lack of CH_4.

Line 536-538: Collectively, due to the occurrence of Fe(III) reduction (especially in the upper sediment layers) and the availability of SO_4^{2-} throughout the sediment, we suggest that electron acceptor availability likely did not limit microbial OC decomposition in our study.

Line 545-547: Based on the availability of electron acceptors (e.g., SO_4^{2-}) at all depths and the lack of CH_4, we hypothesize that at our field site and other comparable coastal sites, OC is likely the constraint on microbially mediated CO_2 release and that electron acceptors are likely not a limiting factor.

Line 659 and below: … in terrestrial wetlands. We caution here that we did not directly measure TEA reduction rates. Future studies should investigate turnover rates, potentially utilizing isotopes to confirm this finding. Overall, our results indicate that the OC composition, rather than the concentration alone, controlled 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. The higher CO_2 release observed in the …

Reviewer comment 3

The interpretation that methanogenesis is absent, and therefore that electron acceptors are non-limiting, relies heavily on the lack of detected CH_4 in porewater and flux measurements. However, several alternative explanations for CH_4 absence are not considered. Methane produced at depth can be rapidly consumed by anaerobic oxidation of methane (AOM), or physically flushed from the sediment via tidal exchange before detection. In addition, the manuscript does not report detection limits for dissolved CH_4 or flux measurements, making it unclear whether low but non-zero methanogenesis could have gone undetected. To strengthen the argument, the authors should discuss these potential CH_4 sink processes and provide analytical detection limits or moderate the conclusion to reflect these uncertainties, as the current data cannot rule out methanogenesis or CH_4 cycling within the system.

Author comments: *The reviewer raises an excellent point about the lack of methane. We agree that we did not discuss potential reasons for this lack of* CH_4 *in the porewater. The reviewer is right - the absence of detectable* CH_4 *alone cannot definitively exclude methanogenesis within the sediment. We examined different lines of reasoning to determine if* CH_4 *was being produced and then consumed at the depths we investigated.*

First, we observed no CH_4 *in the porewater over depth (until 50 cm) over multiple field campaigns (in this study as well a previous study at this site (Kubeneck et al., 2025)). We also conducted measurements of emitted* CH_4 *using headspace chambers from the sediment in a separate experiment in 2022, with resulting* CH_4 *concentrations similar to ambient air (Table S3). Second, anaerobic oxidation of methane (AOM) with* SO_4^{2-} *requires a 1:1 stoichiometry; this implies that* SO_4^{2-} *should show a decrease with depth if* CH_4 *was being oxidized. In the case of the pioneer marsh, we see no change in the* SO_4^{2-} *concentration over depth, while in the case of the tidal flat, a slight decrease (< 5%) could be observed (Figure 2). Some studies conducted* *at the Wadden Sea have detected CH_4; however, CH_4 was present only at depths were SO_4^{2-} was largely depleted (Røy et al., 2008; Wu et al., 2015). Hence, we suggest that methanogenesis, and consequently AOM, if any, may could occur at depths below 50 cm in the intertidal flat, provided that labile OM is available at that depth.*

Although we did not measure CH_4 in any of our measurement campaigns, we acknowledge that it is possible that some CH_4 was physically flushed out by tides. Furthermore, we thank the reviewer for the suggestion about reporting the detection limits for CH_4.

We would like to note here that our ongoing work focuses on characterizing the microbial community in the sediments, which will provide further insight into the question of methanogenesis.

We will revise the manuscript by adding the detection limit and discussing possible reasons for the absence of detected CH_4.

Revised/ added text:

Line 24: … at al depth and the lack of detectable CH_4.

Line 285-286: In both the pioneer marsh and intertidal flat, no CH_4 release, neither as fluxes or in the porewater up to a depth of 50 cm, was detected (detection limit: 0.28 and 0.53 ppm respectively; Table S3).

Line 345: In all treatments and the control, no CH_4 as a flux was detected (lower than detection limit (0.28 ppm); Table S3).

Line 451-452: Methane was not detected in the fluxes of any treatment in the intertidal flat plots (lower than detection limit (0.28 ppm); Table S3).

Line 538 and below: We examined different possible explanations for the lack of detected CH_4 as we could not entirely exclude that CH_4 was produced further down in the sediment and oxidized via anaerobic methane oxidation (AOM), as observed in coastal wetlands (Capooci et al., 2024; La et al., 2022; Wang et al., 2019), or lateral transport to surrounding tidal channels (Trifunovic et al., 2020). 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). 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. 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. Thus, our results indicate that CH_4 production and consumption is unlikely until 50 cm, and could occur, if at all, below these depths. Further analysis using microbial analysis and/or CH_4 injection experiments is needed to fully exclude methanogenesis and AOM at lower depths.

Line 545: Based on the availability of electron acceptors (i.e., SO_4^{2-}) at all depths and the lack of detectable CH_4, we hypothesize …

SI: Detection limit of CH_4 (Table S3):

Table S3. Detection limit of CH_4 for the porewater samples (2022) and for the fluxes (2023). *Headspace gas of chambers was not exchanged before measurement. Thus, ambient air was present in the samples as well (concentration ambient air 4.27 ± 0.28 ppm). No linear response of CH_4 was observed and changes within the incubation time in some cases were below detection limit.

	Detection limit	Range of measured samples
CH_4 flux (2023)	0.28 ppm	3.78 – 7.72 ppm *
CH_4 porewater (2022)	0.53 ppm	n.a. (all below detection limit)

Reviewer comment 4

The OC manipulation experiment provides valuable insight into short-term microbial responses to labile and complex carbon additions. However, each injection cycle spans only 48 hours and represents an instantaneous, high-concentration OC pulse. Natural OC inputs, such as root exudation, plant litter deposition, or episodic eutrophication, occur over much longer and more variable temporal scales, and microbial communities may respond differently to varying OC inputs. To avoid over-extrapolating short-term dynamics to ecosystem scale processes, the manuscript should more explicitly discuss how these pulse-style experiments relate to natural OC supply regimes and the extent to which the observed responses can/cannot be generalized to longer-term carbon cycling or environmental change scenarios.

Author comments: *Thank you for this valuable point. We agree that OC inputs of natural events (e.g., eutrophication or root exudation) have a lower OC input compared to concentrations used in this experiment and last longer than 48 h (e.g., root exudation over a growing period). The high concentrations over a short incubation time (48 h) were intentionally chosen to test in a mechanistic, process-oriented framework, if OC addition can stimulate microbially mediated* CO_2 *release from minerogenic salt marshes. Our goal was not to simulate OC supply at natural levels but rather to analyze the system response to increased labile and complex OC availability. High OC concentrations were chosen to minimize the effect of dilution and physical flushing due to tidal exchange which potentially obscure the biogeochemical response. We acknowledge that there might be differences in the carbon cycle response between short-term, high concentration OC inputs and sustained, low OC inputs. To address this limitation and avoid over-extrapolation, we added a paragraph in the discussion addressing short-term, high concentration OC inputs to natural OC supply (line 601 and below). Moreover, we will discuss the extent to which our findings can be transferred to long-term C cycling in minerogenic salt marshes.*

Revised/ added text:

Line 601 and below: Furthermore, it is important to note that OC concentration used in this experiment are higher than those expected for naturally occurring OC inputs, such as root exudates, which are typically released at lower concentrations with a continuous input. Thus, upscaling the enhanced CO_2 fluxes measured in our study might result in overestimation of CO_2 release from minerogenic salt marshes. Our findings rather reveal, on a process level, that the addition of labile OC stimulates microbially mediated CO_2 release. Enhanced CO_2 release from the acetate amended plots was measured at nearly all sampling time points (1.5, 24, and 48 h) without a clear trend, while the concentration of the inert tracer showed a slight decrease over the same period (Fig. S3) – indicating dilution and flushing of the injected OC. This suggest that the elevated CO_2 release was driven by enhanced availability of labile OC independently of its concentration. These findings allow us to generalize that the system is likely limited by labile OC availability, regardless of the concentration; however, further work should quantify how the magnitude of CO_2 promotion corresponds to OC concentration, particularly under low, naturally sustained OC input rates. In conclusion, we can reliably predict the direction of increased OC inputs to minerogenic salt marshes, but further studies are needed to predict the long-term magnitude of changes in the carbon cycle in these ecosystems.

Reviewer comment 5

Lines 40-42: This statement is very general. Authors should mention *which* climate-driven processes, such as temperature, sea-level rise, vegetation shifts, storm frequency, are most relevant to OC turnover in minerogenic marshes. This would help frame the specific hypothesis tested later in the manuscript.

Author comments: *We see the reviewer's point and added a few scenarios as examples.*

Revised/ added text:

Line 40-42: Therefore, understanding carbon turnover in coastal wetlands is crucial for predicting how these ecosystems will respond to climate change, such as temperature increase, sea-level rise, and eutrophication events.

Reviewer comment 6

Lines 56-59: I suggest clarifying why minerogenic sediments might differ from organogenic ones here. For example, in TOC content, mineral surface area, or porewater exchange, would better justify the hypothesis. This distinction is central to the paper, but it's currently presented it in broad terms.

Reviewer comment 7

Lines 64-68: This is a key knowledge gap the study is addressing. I would suggest explaining in more detail examples of the work in European Saltmarshes with respect to TEA turnover and not GHG emissions.

Reviewer comment 8

Lines 59-61: I suggest rewording this text to focus on how previous work has focused on locations that are biogeochemically different to the current study and mention the geographic locations as an aside. Currently this text could be misinterpreted as implying that previous work is geographically narrow or uninformative. Explaining how the U.S. sites differ biogeochemically (peat-dominated, high TOC, strongly reducing conditions) would prevent oversimplification and more clearly position the present study as filling a genuine gap.

Author comments: *Thank you for the comments related to differences between organogenic and minerogenic salt marshes. Since these comments are related, we combined the responses to all three. We acknowledge your feedback and will refine the descriptions of the differences. Also, adding detailed examples with respect to TEAs is a valid point regarding the key question of if the system is limited by electron donor or acceptors.*

Revised/ added text:

Line 59 and below: Past studies on OC dynamics (including GHG release) in salt marshes have largely concentrated on the eastern coast of the US (Capooci et al., 2024; Kostka et al., 2002; Lowe et al., 2000; Seyfferth et al., 2020), which is dominated by organogenic peat marshes. Organogenic marshes are low energy, microtidal wetlands, characterized by a high organic matter deposition via autochthonous pathways that results in high TOC contents (Logemann et al., 2025).

Line 64 and below: European salt marshes, in contrast, are primarily minerogenic, i.e., contain high fractions of mineral sediment due to high sedimentation rates, resulting in comparably lower TOC content (Nolte et al., 2013). Studies conducted in European salt marshes have focused on the TEA turnover (e.g., SO_4^{2-}respiration rates) and not GHG emissions (Bosselmann et al., 2003; de Beer et al., 2005; van Erk et al., 2023). These studies showed that O_2 penetrate into the sediment, Fe(III) is available, and SO_4^{2-} reduction occurs. Hence, these studies have provided indirect links between belowground biogeochemistry, especially in the context of available TEAs, and the release of GHGs in minerogenic salt marshes; however, a direct investigation of the determining factor(s) of OC degradation from these ecosystems is missing.

Complete paragraph (line 55-70):

The primary controls on GHG release in salt marshes, as interfaces between land and open ocean, are not fully understood. Specifically, it is unclear whether OC turnover is primarily controlled by the availability of the electron acceptors – as observed in organogenic marshes and consistent with studies in terrestrial wetlands (Schlesinger and Bernhardt, 2013) – or by the organic matter itself, as suggested for marine sediment in general (Arndt et al., 2013). Past studies on OC dynamics (including GHG release) in salt marshes have largely concentrated on the eastern coast of the US (Capooci et al., 2024; Kostka et al., 2002; Lowe et al., 2000; Seyfferth et al., 2020), which is dominated by organogenic peat marshes. Organogenic marshes are low energy, microtidal wetlands, characterized by a high organic matter deposition via autochthonous pathways that results in high TOC contents (Logemann et al., 2025). For example, Lowe et al. (2000) and Kostka et al. (2002a) observed that OC oxidation was controlled by SO_4^{2-} and Fe(III) reduction in organogenic salt marshes in Georgia, USA. Further, CH_4 fluxes were detected in salt marshes in Delaware, suggesting a co-occurrence of methanogenesis and SO_4^{2-} reduction (Capooci et al., 2024; Seyfferth et al., 2020). European salt marshes, in contrast, are primarily minerogenic, i.e., contain high fractions of mineral sediment due to high sedimentation rates, resulting in comparably lower TOC content (Nolte et al., 2013). Studies conducted in European salt marshes have focused on the TEA turnover (e.g., SO_4^{2-}respiration rates) and not GHG emissions (Bosselmann et al., 2003; de Beer et al., 2005; van Erk et al., 2023). These studies showed that O_2 penetrate down the sediment, Fe(III) is available and SO_4^{2-} reduction occurs. Hence, these studies have provided indirect links between belowground biogeochemistry, especially in the context of available TEA, and the release of GHGs in minerogenic salt marshes; however, a direct investigation of the determining factor(s) of OC degradation from these ecosystems is missing.

Reviewer comment 9

Lines 91-95: I suggest including quantitative information here. The current phrasing provides qualitative differences but lacks quantitative information (inundation duration, frequency, elevation relative to MHW). Hydrology strongly influences redox conditions and solute transport and providing explicit values or ranges would help readers assess how representative and comparable the two zones are.

Author comments: *Unfortunately, we were not able to provide exact information about the elevation relative to MHW at the sampling site. We could find general information that the water depth of the intertidal flat during inundation is 1 m below the MHW and for the pioneer marsh 0.5 m below MHW, however, we could find no data for the specific site. Thus, we decided to only include inundation duration.*

Revised/ added text:

Line 91-93: Both the pioneer marsh and intertidal flat are inundated twice a day during high tide, in general, with the pioneer marsh experiencing less and shorter inundation (< 3h fully inundated) compared to the intertidal flat (> 3h fully inundated) (de Vlas et al., 2013).

Reviewer comment 10

Lines 111-115: Were actions taken to reduce compaction? Push-core sampling can cause compression or smearing, especially in fine-grained sediments. This text would benefit from a short note on steps taken to minimize disturbance.

Author comments: *We see the reviewer's concern and thus added a more detailed description of the push core sampling approach. As we are familiar with the concern of compaction, we have attempted to decrease this effect by using open push cores and only capping them after the cores were pushed into the sediment. Further, we sampled sediment/porewater not from the edges where there was the potential for smearing but rather from the middle which was likely undisturbed.*

Revised/ added text:

Line 113 and below: To minimize compression, we used open push cores and only capped them when the core liner was fully in the sediment. Furthermore, the inside wall of the cores was plain and clean to smoothly insert the core liner in the sediment. To further minimize disturbance, the sampled cores were immediately closed, vertically transported, and stored in the dark.

Line 115: We made sure to not take sediment or porewater samples at the edges but rather from the middle of the cores, where the sediment is likely undisturbed.

Reviewer comment 11

Lines 175-177: It would be useful for the readers to know the chamber volume here.

Author comments: *We added the chamber volume (3000 cm^3).*

Revised/ added text:

Line 175: Gas sampling was conducted using an opaque, static, non-flow gas chamber made of polypropylene (chamber volume 3000 cm^2).

Reviewer comment 12

Line 195: Should be 'inner diameter of 2.5 cm, and a length of 10 cm'

Author comments: *Thank you for this comment.*

Revised/ added text:

Line 195-197: For this, push cores (inner diameter of 2.5 cm, and a length of 10 cm) were taken from the middle of each plot at the same positions as the porewater samples and immediately frozen until further analysis.

Reviewer comment 13

Lines 246-248: This section lacks detail for the reader to appreciate what numerical analysis was carried out. I appreciate all the details of statistical analysis is provided in the supplement, but some of the important information should be provided within the main body of the text. As a minimum, a summary of the statistical workflow should be included in the main text.

Author comments: *We expanded a short version of the applied tests and kept the detailed explanation in the SI.*

Revised/ added text:

Line 246 and below: For statistical analysis RStudio (R version R-4.4.3) was used. The significance level for all tests was set at $p < 0.05$. Normal distribution of the data and homogeneity of variances were tested by Shapiro-Wilk test and Levene test, respectively. Correlations between parameters was tested with the relevant tests (Pearson's correlation test or Spearman's rank correlation test depending on the normality of the data). Statistical differences between two groups were tested with a t-test and for more than two groups with a one-way Analysis of Variance (ANOVA) or Kruskal-Wallis rank sum test. For differences in the CO_2 release, a linear mixed model was applied. More details on the chosen tests and model are given in Supplement, S1.7. We reported the p-value in the text; further relevant statistical test results and parameters are shown in the corresponding sections in the SI. The variability of the geochemistry analysis is represented by the standard deviation of triplicates/duplicates. For the in situ experiment, the variability is reflected in the standard error of triplicates. For duplicate analyses, variability reflects the range of the two samples.

Reviewer comment 14

Line 315: 'for the' is repeated

Author comments: *Thank you for the comment. We removed the duplication.*

Revised/ added text:

Line 314-315: Here, the mean residual fraction was 38.2 ± 4.8 % for the acetate treatment and 37.3 ± 3.6 % for the humic acid treatment.

Reviewer comment 15

Line 341: 'Complimentarily' is not an appropriate word here. Something like 'Similarly' or 'In comparison' should be used

Author comments: *Thanks, we replaced it with "Similarly".*

Revised/ added text:

Line 341-343: Similarly, the cumulative CO_2 emissions from the acetate treated plots were the highest while the emissions from the humic acid and control plots were in a similar range for all four injection cycles (Fig. 4b).

Reviewer comment 16

Line 496: Typo ‘decreasing to 0mM below.’

Author comments: *Thank you for the comment. The original sentence was confusing, so we have rephrased it and added the* O_2 *concentrations to provide context and clarify what the 0 mM in the original sentence refers to.*

Revised/ added text:

Line 496: Based on microsensor measurements during low tide, we observed O_2 concentrations in the top 2 mm decreasing with depth, from 131.02 ± 26.49 to 0.18 ± 0.12 $\mu mol\ L^{-1}$ in the pioneer marsh, and in the intertidal flat from 155.17 ± 12.71 to 0.62 ± 1.10 $\mu mol\ L^{-1}$, reaching 0 µM below that depth.

Reviewer comment 17

Line 503: I suggest rewording ‘We speculate’. Speculation is not something that’s encouraged in scientific work.

Author comments: *Thanks, we exchanged it with “suggest”*

Revised/ added text:

Line 503: We suggest that the lack of O_2 penetration beyond 2 mm at our study site results from the presence of fine particles.

Reviewer comment 18

Line 623: should be ‘led’ instead of ‘lead’

Author comments: *We have corrected this to “led”.*

Revised/ added text:

Line 622-624: The functional gene analysis provided further evidence for this, as SRB were present (absolute gene copy numbers in Supplement, Fig. S7); however, none of the treatments led to an increase in their metabolic activity compared to the control (Fig. 6).

Reviewer comment 19

Line 627: ‘was much higher’ should be ‘were much higher’

Author comments: *We have now corrected this to “were”.*

Revised/ added text:

Line 327: … concentrations and availability of SO_4^{2-} were much higher.

Reviewer comment 20

Figures 4-8: Statistically significant differences should be denoted on the figures. It is common practice to place italics letters above bars in such comparison to show statistical groupings. This makes it easier for the reader to very quickly determine the statistical differences, if any are present.

Author comments: *Thank you for this point – we mostly agree. We have now tried to strike a balance between readability and statistical information on the plots by only denoting the significant differences in the bar plots (Figure 5b and 8). We did not indicate statistical significance in the line graph of the* CO_2 *plot (Figure 4a), as this would have decreased figure clarity and significance was not consistently observed across all cycles. Nevertheless, the key observation was that the acetate treatment showed consistently higher* CO_2 *release compared to humic acid and control, although differences were not significant at all time points and injection cycles, likely due to high variability among replicates reflecting natural in situ heterogeneity. No statistically significant between treatments and control was measured for the bar plots of the cumulative* CO_2 *release (Figure 4b), likely due to the variability of the fluxes that resulted in variable cumulative* CO_2 *emissions. We added a line (line 345) explaining the potential absence of statistical differences in the cumulative* CO_2 *release. For Figure 7a/b (*CO_2 *release from the intertidal flat), no statistical analyses were applied due to missing values caused by nonlinear* CO_2 *release during the incubation time of gas sampling. Therefore, we decided to not apply statistical comparisons and thus rephrased it accordingly (line 446-451) and will remove Table S12. Figure 6 already had significance denoted.*

Revised/ added text:

Line 345: No statistical differences were measured between the cumulative CO_2 emission of the acetate treated plots and the control or humic acid treatment. Overall, these differences were smaller than those seen at individual CO_2 fluxes at specific time points (Fig. 4a), likely due to high variability in fluxes that resulted in variable cumulative CO_2 emissions.

Line 446-451: Figure 7a presents the CO_2 release from the intertidal flat over three injection cycles 1.5, 24, and 48 h post injection. Acetate treated plots released the highest CO_2 in all three injection cycles compared to the humic acid and the control plots. Similar to the pioneer marsh, no strong differences were observed between humic acid treated plots and the control plots. Consistently, the maximum cumulative CO_2 emissions were observed in the acetate treated plots (Fig. 7b). Due to nonlinearity of CO_2 release over the incubation time of gas sampling, some data points are missing; therefore, statistical comparison of CO_2 release between treatments and the control were not possible. Nevertheless, plots amended with acetate consistently showed higher CO_2 releases across all injection cycles.

Revised figures:

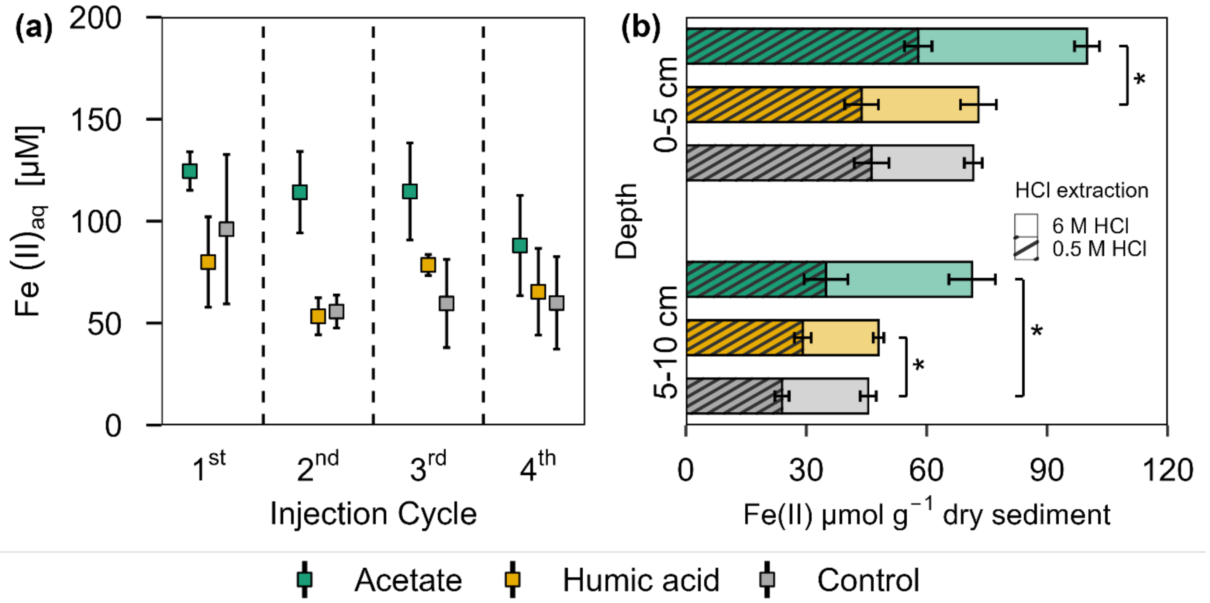

*Figure 5b: Line 371: … Significance is denoted for the 0.5 M HCl extraction. Statistical details are given in the SI (Table S10), significance level p < 0.05 *….*

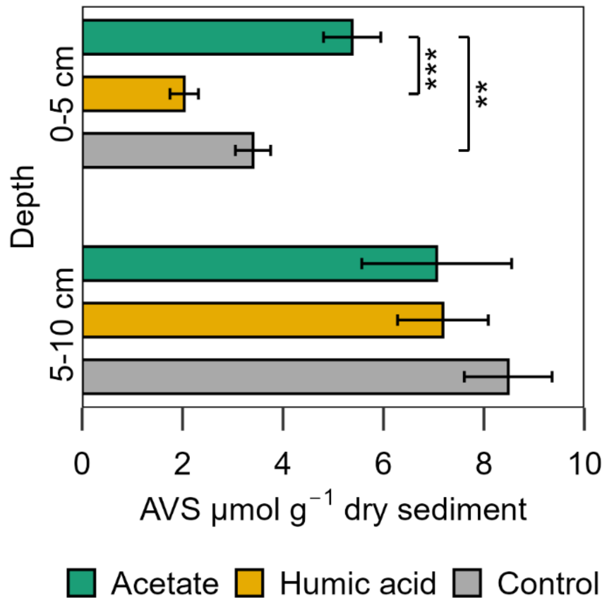

*Figure 8: Line 468: …Significance is denoted for the upper sediment layer (0-5 cm), deeper layer no statistically significant difference occurred. Statistical details are given in the SI (Table S13), significance level p < 0.05 *, p < 0.01 **, and p < 0.001 ***.*

References

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