

Response to Associate Editor and Reviewers comments on manuscript egusphere-2026-582 titled “Different paths, same destination: similar functional outcomes in nitrogen cycling within artificialized coastal habitat”.

Associate Editor

Dear Authors,

Thank you for your thoughtful responses to the reviewers' comments and suggestions.

Two reviewers acknowledged the novelty and significance of your comparison of biogeochemical processes in artificialized and reference coastal ecosystems. While the overall evaluation is positive, the reviewers have identified several areas for improvement, including your conclusion regarding the insignificant difference in denitrification rates between the two systems. I agree with both reviewers that you need to provide a more in-depth discussion of the implications of the observed N transformation processes (inorganic N fluxes, denitrification rates, and DNRA), rather than simply stating that insignificant differences are signs of “ecosystem plasticity”.

I wonder if low-level anthropogenic perturbations (in terms of structure size and deployment duration) might leave no discernible impact. A more comprehensive comparison of your sites with a range of impacted coastal systems would help readers see your findings from a broader perspective of increasingly artificialized coastal systems worldwide. From a technical perspective, it would be helpful for understanding the presented N measurements if you provided a depth profile of sediment DO (Fig. 2c) as well as more methodological details (e.g., how to minimize sediment exposure to oxygen, how to ensure analytical accuracy, specifically QA/QC measures) regarding your key measurements (DO, N species and their isotopes).

Based on these reviews, I recommend "Reconsider after major revisions." Upon receiving the revised manuscript, I may ask the original reviewers or new ones to assess whether the revisions have adequately addressed the concerns.

When submitting the revised manuscript, please ensure that all changes are clearly shown in a track-changed version. Additionally, please provide a point-by-point response to the reviewers' comments, specifying the relevant line numbers in the revised text.

We look forward to receiving your revised manuscript.

Sincerely,

Ji-Hyung Park

Associate Editor, Biogeosciences

AC-AE1-1: Dear Associate Editor,

We thank you and the reviewers for the careful evaluation of our manuscript and for the constructive comments provided.

We have revised the manuscript in depth following these suggestions. In particular, we substantially improved the analysis and discussion of nitrogen transformation processes by integrating additional biogeochemical evidence, including new solid-phase reactive iron data, and by clarifying the mechanisms underlying the observed patterns. The Figure 2 now includes the oxygen microprofiles. These modifications allowed us to move beyond a generic interpretation in terms of “functional plasticity” and explicitly describe the distinct pathways sustaining denitrification across sites.

We also revised the title to better reflect this mechanistic framework and the main message of the study, which emphasizes that similar benthic nitrogen cycling can be sustained by distinct biogeochemical mechanisms in artificialized coastal habitats.

In addition, we expanded the discussion to better position our results within the broader context of artificialized coastal systems and refined the interpretation of our findings accordingly. Given that the influence of coastal artificialization on nitrogen cycling remains poorly documented, this broader contextualization helps place our findings within a wider perspective. Methodological details and clarifications were also added throughout the manuscript to improve transparency and readability.

The following paragraph addresses this comment (L524-534):

*“Placed in a broader context, although the impacts of coastal protection structures on benthic nitrogen cycling remain poorly documented, our results suggest that these effects are structure specific. Indeed, vertical, hard-engineered structures like seawalls induce coastal squeeze and habitat loss. This reduces the seascape-scale nitrogen-removal capacity, even if unit-area denitrification rates remain unaffected (O’Meara et al., 2015). Conversely, in the present study, our offshore breakwater accumulates organic-rich sediment. This accumulation, associated with dense populations of bioturbators (e.g., *H. diversicolor*), sustains a bioirrigation pathway that functionally substitutes for the lost sediment oxygenation by roots. While this functional equivalence aligns with observations from nature-based protections like living shorelines (Onorevole et al., 2018; Schoell et al., 2023), our study shows that it can also occur in a completely different, fully artificial environment. Taken together, these results show that even in artificialized settings, nitrogen-removal processes can remain functionally equivalent, as long as biogeochemical mechanisms capable of coupling nitrification and denitrification are maintained. Whether such compensatory*

mechanisms occur in other artificialized systems will depend on local environmental conditions and biological assemblages.”

The following paragraphs address this comment (L194-195; 224-225; 250-262, respectively):

“Microelectrodes (OX100, Unisense© with a repeatability better than 2% as specified by the manufacturer) were inserted into the sediment at 100 µm intervals using a motor-driven micromanipulator (MU1, Pyroscience©).”

“The dissolved O₂ concentration was measured in real time using an optode (O₂ sensor SP; PreSens© with a repeatability better than 0.5% as specified by the manufacturer)”

“For porewater sample, NO₃⁻ (quantification limit 0.6 µM), NO₂⁻ (quantification limit 0.05 µM), NH₄⁺ (quantification limit 0.1 µM) and PO₄³⁻ (quantification limit 0.1 µM) concentrations were analyzed using an autoanalyzer AA500 (Seal analytical©) following Aminot et al. (2009). For water incubation samples, where sample volume was limited to avoid dilution due to replacement water, NH₄⁺ was determined manually according to Hansen and Koroleff (1999), while NO_x were analyzed by continuous flow analysis following Anderson (1979) both with measurements reproducibility better than 5%. Analytical accuracy was assessed using certified reference materials (MOOS-4, NRC Canada).

The isotope ²⁸N₂, ²⁹N₂ and ³⁰N₂ measurements were completed within one week of collection using a membrane inlet mass spectrometer (MIMS, Bay Instruments©) (Kana et al., 1994). The protocol followed Kana et al. (2006). Calibration factors for standard sample were computed based on solubility equations (Hamme and Emerson, 2004). Analytical precision for N₂/Ar ratios (coefficient of variation of five replicates standards) was consistently < 0.5%.”

As well as in the new paragraph presenting methods for the solid-phase iron (L211-220):

“2.6 Reactive Fe oxyhydroxide concentrations

The reactive fraction of iron-oxyhydroxides was extracted from homogenized, freeze-dried sediment samples using a buffered ascorbate solution. The term “reactive” refers to the easily reducible fraction of Fe oxides, typically amorphous Fe(III) oxides

(Hyacinthe et al., 2001; Kostka and Luther, 1994)(Anschutz et al., 2005). The ascorbate reagent consisted of 10 g of sodium citrate and 10 g of sodium bicarbonate dissolved in 200 ml of Nanopure® water, with 4 g of ascorbic acid added to reach pH 8 (Kostka and Luther, 1994). Approximately 250 mg of ground, freeze-dried sediment was digested in 15 ml of the ascorbate reagent. After 24 hours of shaking at room temperature, the sediment slurries were centrifuged, and a fraction of the supernatant was diluted with a 1 M HNO₃ solution before Fe was analyzed using Microwave Plasma Atomic Emission Spectroscopy (4210 MP-AES, Agilent®). Fease concentrations were corrected for sediment porosity and adjusted for salinity, with a measurement reproducibility better than 2%.”

We are grateful for these comments, which significantly improved the quality, clarity and scope of the manuscript.

We provide below a point-by-point response to all comments.

Sincerely,

Ludovic PASCAL

Please note that line numbers refer to the track-change version of the manuscript.

Minor technical and typographical corrections have been implemented throughout the manuscript without systematic indication of line numbers.

Reviewer 1

RC1-1: The manuscript “Different paths, same destination: similar functional outcomes in nitrogen cycling within artificialized coastal habitat” by Pascal et al. investigates how man-made structures aimed at mitigating coastal erosion, such as breakwaters, shape the natural coastal ecosystem, particularly the benthic habitat and its nitrogen-cycle. Method wise, they look at porewater geochemistry, flux measurements, denitrification process rates and macrofauna at both impacted and non-impacted coastal sites. Interestingly, while there are clear differences in benthic biogeochemistry and fauna between the impacted and non-impacted sites, denitrification rates do not follow this pattern but show high and low rates at both impacted and non-impacted sites. This leads the authors to the conclusion that there is no impact of the breakwater structure on the benthic nitrogen-cycle. This is an interesting and important study, and I had pleasure reading it. Because of climate change, man-made structures to protect the coastline are of increasing societal and infrastructural importance, and it is nice to read that this does not necessarily come

at the cost of ecosystem functioning. Particularly the nitrogen-cycle is important in a coastal setting, given that coastal eutrophication is pre-dominantly driven by nitrogen.

AC-R1-1: Dear Reviewer 1,

We thank you for your careful evaluation of our manuscript and for the supportive and constructive comments. We have carefully addressed all the concerns raised and responded to each comment below.

Please note that, as we have added new data to address both reviewers' comments, we have included Carole-Anne Guay in the author list. She collected the additional sediment samples and performed the solid-phase ascorbate-extractable iron analyses presented in the revised manuscript.

Specific comments:

RC1-2: However, I do have a couple of concerns, the major one being the lack of explanation for the variation in denitrification rates, from which then the conclusion was derived that there is no general/concise effect of the breakwater on the nitrogen-cycle (as the variation was in both impacted and non-impacted sites). The impacted sites had much higher organic matter and organic carbon content, different macrofauna, and a lower OPD / higher DOU than the non-impacted sites, while nitrate concentration in surface sediments were about similar between all sites. Nevertheless, given the higher organic carbon content, which was seemingly of labile composition given the higher DOU, it could have been expected that denitrification rates would have been higher than at the non-impacted sites. Instead, the highest rates were found at an impacted and a non-impacted site, and the main explanation offered is “functional plasticity”- which, I feel, is too vague and a bit unsatisfying (l. 436). I would like to encourage the authors to look more thoroughly into their biogeochemical data and use them for their conclusions. Referring to the title, this means to describe in more detail what the “different path” is. Overall, for a manuscript that focusses on the nitrogen-cycle, the nitrogen data are too little discussed, esp. the results of the denitrification rate measurements.

AC-R1-2: We thank the reviewer for the suggestion. We agree that “functional plasticity” was too vague. We have thoroughly revised the Discussion (Section 4.1 and 4.3) to integrate more biogeochemical results (porewater and new solid-phase reactive iron profiles to explicitly demonstrate how distinct mechanisms (“different paths”) lead to similar denitrification rates at LBWS and VS. Also, we now compare our denitrification rates with the literature and place our finding in a larger context of

coastal artificialization. The title, abstract and conclusion have been modified as well to better emphasize on the mechanisms rather than vague term such as “function” and “functional plasticity”.

Here is how the distinct pathways are explained:

- Vegetated sediment (root-driven pathway):

The porewater data at our vegetated reference site (VS) shows a distinct subsurface NO₃⁻ peak (Fig. 2c), which we interpret as a signature of coupled nitrification-denitrification fueled by cordgrass root oxygenation deep in the sediment. To strengthen this, we have added solid-phase ascorbate-extractable Iron profiles (reactive iron oxyhydroxides) to figure 2. At VS, this reactive Fe(III) fraction exhibits a peak at 3-4 cm depth suggesting rhizosphere oxidation. This deep O₂ release drives the high denitrification rates and coupled nitrification-denitrification at VS despite lower surface OM content and (quasi) absence of bioturbation.

- Landward southern side of the breakwater (bioturbation-driven pathway):

The porewater data at LBWS (Fig. 2d) shows a rapid accumulation of NH₄⁺ and PO₄³⁻ from the surface to the deep layers of the sediment, indicating a strong reducing environment driven by the accumulation of fine, organic-rich particles (Table 1). In such condition, sedimentary nitrification should be limited. However, the high density of small bioturbators such as *H. diversicolor* and oligochaete maintains bioturbation of the surface sediment layer. The bioirrigation supplies O₂ to sustain nitrification and its coupling with denitrification as well as supplying NO₃ to sustain high denitrification rates

LBWN shares the high organic matter characteristics of LBWS but lacks the high density of bioturbators. Consequently, the sediment remains reduced which can inhibit nitrification and result in low denitrification.

US represent a sandy mudflat with low OM and low macrofauna density, resulting in low denitrification rates due to organic carbon limitation.

The section 4.1 now reads as follows (L443-477):

“4.1 Impact of breakwater construction on benthic habitats

Our study highlights marked alterations in surficial sediment characteristics, with impacted sites (i.e., LBWS and LBWN) accumulating finer and more organic-rich sediment compared to reference and intermediate sites. These observations are consistent with expectations for low-energy depositional environments landward of

high-crested breakwaters (Da Silva et al., 2022; van Rijn, 2011; Wang et al., 2021). In contrast, sediments of the intermediate site (SBW) resembled reference sites (i.e., US and VS), suggesting minimal breakwater influence in this area. The consistent sedimentary $\delta^{13}\text{C}_{\text{org}}$ signature among sites suggested a shared origin of organic carbon, likely a mix of terrestrial and marine inputs typical of intertidal zones. This homogeneity suggests that low-energy hydrodynamic conditions landward of the breakwater enhance deposition of fine, organic-rich particles without selective retention of organic matter from specific sources. Therefore, differences among sampled areas are driven by organic carbon quantity, not origin.

Geochemical measurements further support this pattern. Sediment cores from impacted sites showed up to 2.3 times higher diffusive O_2 uptake (DOU) and shallower O_2 penetration depth (OPD) than reference sites (Fig. 2a and b), consistent with enhanced organic carbon mineralization driven by higher organic carbon content (Glud, 2008). These results align well with our porewater nutrient profiles extracted from another sediment core. The decrease in porewater NO_3^- concentration with depth in all sites indicates a consumption by the sediment (Fig. 2c), consistent with our measured NO_3^- benthic fluxes (Fig. 6). This consumption results from the denitrification process, which has been detected among all sites (Fig. 6). VS exhibited a subsurface NO_3^- peak, suggesting active nitrification consistent with rhizosphere oxygenation (Koop-Jakobsen et al., 2018), which may explain the relatively high proportion of coupled nitrification-denitrification detected in incubation experiment. This interpretation is further corroborated by the solid-phase ascorbate extractable iron (Feasc) profiles (Fig. 2d). At VS, Feasc increase progressively from the surface to a peak at 3-4 cm depth, well below the NO_3^- maximum (Fig. 2c), suggesting subsurface oxidation consistent with rhizosphere oxygenation. The absence of NO_3^- peak co-located with the Feasc one suggests a tight coupling between nitrification in the rhizosphere area and denitrification in adjacent anoxic sediment (Koop-Jakobsen and Giblin 2010, Zheng et al. 2016). The quicker NO_3^- consumption in the porewater of impacted sites in the upper four centimeters compared to six centimeters in reference sites suggests a more reduced condition in impacted sites. Similarly, deeper sediment layers in impacted sites exhibit greater increases in NH_4^+ and PO_4^{3-} compared to reference sites, indicating larger ammonification driven by their higher organic matter content, along with PO_4^{3-} release, typically associated with the reductive dissolution of iron-oxyhydroxides in anoxic sediment layers (Slomp, 2011). Consistently, Feasc concentrations at impacted sites are highest at the sediment surface and decrease rapidly with depth (Fig. 2d). This indicates that aerobic processes, including nitrification, are restricted to the very surface of the sediment at

these sites. Overall, these O_2 and nutrient profiles suggest more reduced conditions and greater nutrient accumulations in the sediment column in impacted sites compared to reference sites. Despite these changes in sedimentary conditions, denitrification rates were not substantially altered, suggesting that the capacity of these sediments to remove excess nitrogen remains resilient to these perturbations.”

The section 4.3 now reads as follows (L493-542):

“4.3 Potential convergence in ecosystem functioning

*Despite marked differences in sediment characteristics, macrobenthic community structure and trait expression between impacted and reference sites (i.e., LBWN vs. US and particularly LBWS vs. VS), total benthic fluxes and denitrification rates remained similar. A similar convergence in process rates despite contrasting biological drivers has been reported in other benthic systems (Woodin et al., 2016). Here, however, it results from distinct mechanisms, namely root-driven oxygenation and bioturbation. These findings, derived from dark incubations (see section 2.10), suggest that distinct benthic environments can yield similar functional outcomes regarding oxygen uptake, nutrient cycling and notably denitrification rates. Our measured denitrification rates ranging from 6 to 75 $\mu\text{mol m}^{-2} \text{h}^{-1}$ fall within the range of reported denitrification rates for temperate intertidal mudflats and salt marshes (Hinshaw et al., 2017; Koch et al., 1992; Poulin et al., 2007; Sousa et al., 2012). Coupled nitrification-denitrification (Dn) was the dominant N-removal pathway at impacted sites and VS, indicating that denitrification relies on locally produced NO_3^- . Conversely, at US, denitrification was primarily driven by water-column nitrate. The high density of subsurface bioturbators at LBWS (Fig. 5c) likely enhances oxygen and nutrient exchange at the sediment-water interface. The sediment irrigation enhances aerobic mineralisation of the OM, stimulates nitrification fuelling coupled nitrification-denitrification, a pattern supported by our isotope pairing results (Fig. 6). In VS, although macrofauna is present, bioturbation is absent or constrained to the sediment surface (Fig. 5c). However, as evidenced by the deep subsurface Feasc peak (Fig. 2e; Section 4.1), radial O_2 loss from *S. alterniflora* roots actively creates deep oxidized zones (Koop-Jakobsen et al., 2018). This rhizosphere oxygenation sustains subsurface nitrification, which is tightly coupled to denitrification, thereby achieving high denitrification rates similar to those driven by bioturbation at LBWS. By contrast, LBWN showed a trait expression tendency toward US that could yield similar benthic fluxes. The low denitrification rates at US likely reflect the relatively low*

organic carbon availability compared to impacted sites, which limits local nitrification fuelling denitrification. Moreover, macrofauna at US is dominated by a few large burrowers (e.g., *Mya* sp.) whose local influence may not be captured at the core scale, leading to low benthic fluxes. Interestingly, despite being both impacted sites and having similar sediment characteristics, LBWN and LBWS exhibited different ecosystem functions based on total benthic fluxes and denitrification rates. This most likely results from the higher density of macrobenthic organisms at LBWS than at LBWN, highlighting the critical role of macrobenthic community activity in shaping biogeochemical processes (Snelgrove et al., 2018). In the absence of dense bioturbators, the reduced sediment at LBWN (Fig. 2d and e) prevents nitrification despite the high availability of organic carbon, effectively limiting denitrification. However, the factors driving the variation in macrobenthic community structure between these two areas remain elusive. The absence of a detectable breakwater effect on denitrification rates therefore does not reflect an absence of biogeochemical change, but rather a functional compensation mediated by distinct biological drivers at each site.

Placed in a broader context, although the impacts of coastal protection structures on benthic nitrogen cycling remain poorly documented, our results suggest that these effects are structure specific. Indeed, vertical, hard-engineered structures like seawalls induce coastal squeeze and habitat loss. This reduces the seascape-scale nitrogen-removal capacity, even if unit-area denitrification rates remain unaffected (O'Meara et al., 2015). Conversely, in the present study, our offshore breakwater accumulates organic-rich sediment. This accumulation, associated with dense populations of bioturbators (e.g., *H. diversicolor*), sustains a bioirrigation pathway that functionally substitutes for the lost sediment oxygenation by roots. While this functional equivalence aligns with observations from nature-based protections like living shorelines (Onorevole et al., 2018; Schoell et al., 2023), our study shows that it can also occur in a completely different, fully artificial environment. Taken together, these results show that even in artificialized settings, nitrogen-removal processes can remain functionally equivalent, as long as mechanisms capable of coupling nitrification and denitrification are maintained. Whether such compensatory mechanisms occur in other artificialized systems will depend on local environmental conditions and biological assemblages.

From a coastal-management perspective, one of the main issues is whether areal flux rates measured in LBWS can offset, at the seascape level, the functional loss caused by salt marsh vegetation removal. LBWS area (~18,800 m²) is about three times larger than the vegetated area directly lost due to the breakwater implementation (~6,300

m²). Assuming that the measured fluxes capture the whole area heterogeneity, LBWS could compensate for these specific functional capacity losses of that part of the salt marsh. However, it is worth noting that this area is still more than three times smaller than the total vegetated area lost due to combined erosion and breakwater implementation (~62,700 m²) so any functional compensation remains only partial at the scale of the whole seascape.”

The conclusion now reads as follows (L544-563):

“5 Conclusion

Our study highlights the complex relationships between sediment characteristics, macrobenthic communities, and ecosystem functioning in coastal environments impacted by shoreline hardening structures. We observed that some ecosystem functions (i.e., benthic respiration, nutrient cycling, and denitrification rates) can be maintained through several combinations of abiotic and biotic factors. This finding reflects a convergence in ecosystem functioning that can arise from distinct underlying mechanisms. However, it is important to interpret these results carefully. While we observed functional similarities in specific ecosystem functions, natural coastal habitats provide an extensive collection of ecosystem functions and services (Friess et al., 2020; Gedan et al., 2009) that are far from being fully captured by our measurements. The observed functional convergence may contribute to the resilience of some ecosystem functions at the seascape level. However, these localized similarities do not mean that these functions are unaltered, as the underlying mechanisms are completely modified and potentially more vulnerable to additional environmental pressures. Furthermore, it does not imply that artificialized environments can sustain all ecosystem functions and services provided by natural habitats. These findings highlight the challenges in predicting and managing ecosystem responses to shoreline hardening, coastal squeeze and probably other coastal disturbances. It also emphasizes that physical, geochemical, and biological factors should be considered when assessing the functional impact of coastal development projects. Given the increasing pressures on coastal ecosystems (Halpern et al., 2019), future research should assess the ability of artificialized ecosystems to maintain their functions under multiple stressors and investigate their long-term stability under various climate change scenarios. Finally, broader ecological implications of apparent functional convergence in coastal ecosystems including potential cascading effects on higher trophic levels should be explored. This understanding will be crucial for developing more accurate predictive models and

effective management strategies in the face of ongoing coastal development and climate change.”

Minor changes have been done in the abstract (L14-32):

“Human activities are increasingly affecting coastal ecosystems, with shoreline stabilization structures becoming a prevalent response to sea-level rise and extreme climatic events. While these structures aim to protect coastal communities and infrastructures, their effects on ecosystem functioning, and particularly nitrogen cycling and its removal processes such as denitrification, remain poorly understood. To assess the ecological impacts of breakwater construction, we employed a space-for-time substitution approach to examine changes in sediment biogeochemistry, macrobenthic community structure, and nitrogen cycling in an intertidal salt marsh in the St. Lawrence Estuary, Canada. We measured benthic fluxes, denitrification rates, and macrofaunal assemblages at five locations: two landward of the breakwater (impacted sites), one situated immediately seaward of the breakwater (intermediate site) and two reference sites (one vegetated and one unvegetated outside the breakwater system). Landward sites exhibited finer sediment with higher organic carbon content compared to other sites and supported distinct macrobenthic communities dominated by opportunistic Oligochaetes while reference sites were dominated by Molluscs and Crustaceans. The intermediate seaward site closely resembled the unvegetated reference site in both sediment characteristics and oxygen dynamics. Surprisingly, despite these substantial physical and biological changes, dark total benthic oxygen uptake, nitrate and ammonium benthic fluxes and benthic denitrification rates showed no significant differences between impacted and reference sites. This functional similarity reflects an apparent functional convergence, where distinct combinations of abiotic and biotic factors can sustain similar ecosystem functions. However, this convergence might not apply to all ecosystem services offered by natural salt marshes and may depend on specific biological communities, emphasizing the importance of careful consideration in coastal management decisions.”

Finally, the title now reads as follows:

“Different paths, same destination: similar benthic nitrogen cycling sustained by distinct biogeochemical mechanisms in artificialized coastal systems”

RC1-3: Furthermore, dissimilatory nitrate reduction to ammonium (DNRA) is not mentioned in the introduction but suddenly pops up in materials & methods (l. 192)

and results, whereas anaerobic ammonium oxidation (anammox) is mentioned in the introduction, but not in the materials & methods and results; neither of them is mentioned in the discussion.

AC-R1-3: We apologize for the inconsistency. We added DNRA to the introduction alongside nitrogen removal processes. Which now reads as follows (L61-72):

“In this context, understanding nitrogen removal processes in salt marshes is crucial when assessing the impacts of shoreline hardening on coastal ecosystem functioning (Sousa et al., 2008; Valiela and Cole, 2002). Shoreline hardening can alter sediment characteristics, hydrodynamic conditions, and benthic communities (Gittman et al., 2016), all of which influencing nitrogen cycling processes (Albert et al., 2021; Stief, 2013). Two key microbial processes drive nitrogen removal from soft-sediment systems. (i) Denitrification which involves an oxidation of organic matter (OM) using nitrate (NO_3^-) as an electron acceptor under anaerobic conditions, resulting mainly in the production of dinitrogen gas (N_2), but can also release as nitrous oxide (N_2O), a potent greenhouse gas, together with carbon dioxide (CO_2). (ii) Anammox (anaerobic ammonium oxidation), produces N_2 through the oxidation of ammonium (NH_4^+) coupled to the reduction of nitrite (NO_2^-) although in many organic-rich coastal sediments its contribution can be smaller than that of heterotrophic denitrification (Dalsgaard et al., 2005; Devol, 2015). In addition to denitrification and anammox, dissimilatory nitrate reduction to ammonium (DNRA) can also occur, competing for nitrate and retaining nitrogen within the system.”

We also added a clarifying sentence in the methods section (2.7) confirming that both DNRA and anammox rates were negligible, justifying our focus on denitrification in the discussion (270-274).

“The validity of the original isotope pairing technique (Nielsen, 1992) was tested by checking the independence of the estimated $^{28}\text{N}_2$ production and the amount of added $^{15}\text{N-NO}_3^-$ (Risgaard-Petersen et al., 2003). No production of $^{15}\text{N-NH}_4^+$ was detected over the incubation periods, DNRA was thus considered to be negligible. Since no deviation from the original isotope pairing technique was detected, anammox was also considered negligible. Both processes are therefore not considered further in the manuscript and denitrification rates were computed on the six sediment cores following Nielsen 1992”

RC1-4: Another issue that needs clarification is the sampling in two years: 2021 and 2022 (l. 125). However, it is not mentioned whether this means (i) that some sites were sampled in 2021 and others in 2022, or (ii) that all sites were sampled repeatedly over two years? Maybe I missed it, but I couldn't find this information. If the sites were sampled in different years, it would add potential interannual variability to the data and maybe that could be a reason for the variation in results (particularly the nitrogen results). Please clarify that point.

AC-R1-4: We apologize for the confusion. All benthic flux incubations, including isotope pairing experiments, porewater extraction, and macrofauna sampling were conducted during a single campaign in June 2021, ensuring no interannual variability in our main dataset. The June 2022 campaign was specifically dedicated to strengthening this dataset by performing O₂ microprofiles at SBW (which was not possible in 2021 due to logistical constraints) and to increasing the replication of baseline sediment characteristics. Cross-validation between the two field campaigns confirmed that spatial gradients remained consistent across years, based on sediment characteristics and O₂ microprofiles at VS and US (Fig 1 and 2 below), further supporting the temporal stability of our observations.

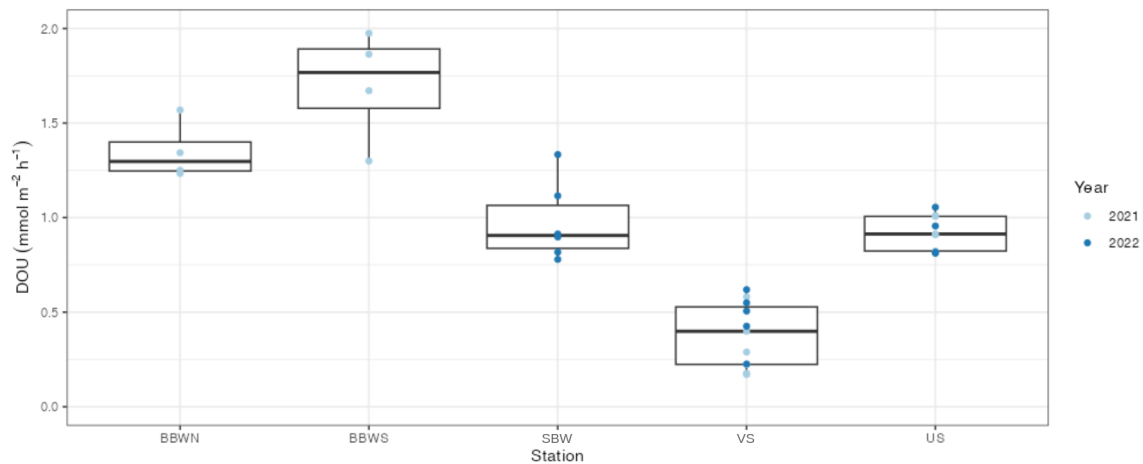


Figure 1: Computed diffusive oxygen uptake (DOU in mmol m⁻² h⁻¹) at the different stations in 2021 and 2022.

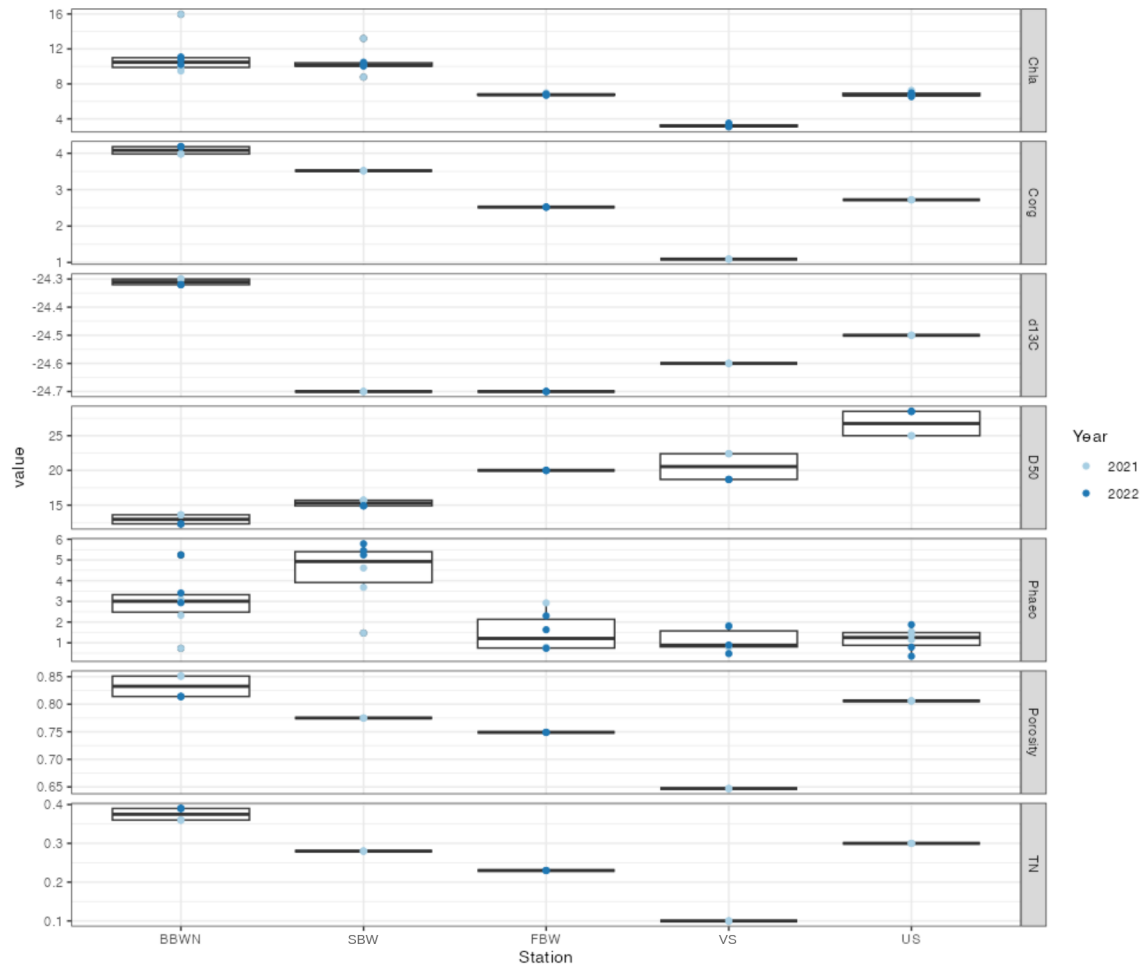


Figure 2: Sediment characteristics in 2021 and 2022. Median grain size (D_{50} in μm), porosity (volume ratio), chlorophyll *a* and phaeopigments (in $\mu\text{g g}^{-1}$), organic carbon and total nitrogen (C_{org} and TN in %) content along with $d^{13}\text{C}$ isotopic signature (in ‰)

RC1-5: L. 168: I would be cautious with the porewater data taken in 0.5 cm resolution; commonly the “catchment area” of rhizons is assumed to be 1 cm (Seeberg-Elverfeldt et al. (2005) Rhizon sampling of porewaters near the sediment water interface of aquatic systems. *Limnol Oceanogr Methods* 3: 361–371); hence, when sampled in smaller resolution you likely draw water from “wrong / overlapping layers”.

AC-R1-5: We agree with the reviewer. This was, in fact, partly acknowledged in the methods section L168-169 of the original manuscript:

“To avoid significant overlap between successive samples, porewater extraction was limited to 8 mL”.

We have modified this sentence to make this limitation clearer and should read as follow (L208-210):

“To avoid significant overlap between successive samples, porewater extraction was limited to 8 mL, we cannot exclude that nutrient profiles in the uppermost cm of the sediment column are slightly smoothed due to limited smearing from adjacent layers.”

RC1-6: L. 178: did you account for the different nutrient concentration of the replacement water in your flux calculation (given that during the flux incubation nutrients will either accumulate or deplete, different to the replacement water)?

AC-R1-6: We did not correct the flux calculations for the nutrient concentration of the replacement water because the dilution effect was negligible. Specifically, at each sampling point (1 h, 2 h, and 3 h), we collected less than 20 mL of water (12 mL for exetainers and 4.5 mL for nutrients), which represents less than 2% of the total overlying water volume in the incubator (>1.1 L). Cumulatively, the total volume replaced over the course of the entire incubation was around 5%. Because of this very low dilution ratio, any correction would have been close to the analytical uncertainty. We have added a brief sentence in the methods (Section 2.7) to clarify this (L233-235).

“Overall, cumulative volume of replacement waters corresponded to < 5% of the overlying water and its dilution effect was thus considered as negligible.”

RC1-7: L. 185: how did you decide for the pre-incubation time of 3 h, was it calculated? Given the OPD of max. 1.3 mm, the silty sediment with a porosity of 0.65-0.83 and a temperature of 9°C, you should have reached a 90% steady state denitrification within less than 15 min (see e.g. Dalsgaard et al 2000, NICE handbook).

AC-R1-7: The 3 h pre-incubation time was chosen to allow the $^{15}\text{N-NO}_3$ tracer to penetrate deeper into the sediment and reach the active coupled nitrification-denitrification zone in vegetated (shallow rhizosphere) and bioturbated sediment. Assuming an apparent effective solute transport coefficient of $2\text{--}3\text{ cm}^2\text{ d}^{-1}$, accounting for both molecular diffusion and enhanced transport due to bioturbation, the $^{15}\text{N-NO}_3$ tracer was estimated to penetrate approximately 1 cm during this period. Now the corresponding sentence in the methods (Section 2.7) reads as follows (L241-243):

“A 3 h pre-incubation period of 3 hour was implemented to ensure sufficient diffusion of $^{15}\text{N-NO}_3^-$ to the sediment nitrate reduction zone (Nielsen, 1992), including shallow rhizosphere and bioturbated layers.”

RC1-8: L. 189: did the replacement water during the ^{15}N -incubation also contain the tracer?

AC-R1-8: No, the replacement water did not contain the $^{15}\text{N-NO}_3$ tracer. Based on the response to RC1-6 cumulative water replacement over the incubation period had a negligible (<3%) effect on the enrichment ratio well below the spatial variability of replicate sediment core.

RC1-9: L. 190: did you also take a sample for denitrification after mixing the porewater with the overlying water? Commonly, a lot of the produced $^{29}\text{N}_2$ and $^{30}\text{N}_2$ sits in the porewater, rather than diffuses out into the water phase; hence, if you do not sample the porewater, you likely miss a portion of $^{29}\text{N}_2$ and $^{30}\text{N}_2$.

AC-R1-9: No, we did not collect sample for denitrification from the slurry after mixing. The denitrification samples were collected exclusively from the overlying water. Mixing the sediment at the end of the incubation would release the $^{29}\text{N}_2$ and $^{30}\text{N}_2$ that had accumulated in the porewater during the 3 h pre-incubation period. Since the initial samples were collected from the overlying water without mixing, we wanted to use the same undisturbed sampling approach at the end of the incubation. Using a final slurry sample would have introduced an inconsistency between the initial and final sampled pools. This could have led to an artificial overestimation of the denitrification rates by including the pre-incubation production not accounted for in the initial sampling. However, for DNRA, slurry samples were indeed collected at the end of the incubation to capture the total ammonium pool. Now the corresponding sentence in section 2.7 reads as follows to clarify this point (L247-249):

“At the end of the incubations, the remaining water was mixed with the top 5 cm of the sediment column. From the slurry, 12 mL of sample was transferred to an Exetainer to which 200 μL of 7 M ZnCl_2 were added for further $^{15}\text{N-NH}_4^+$ analysis to assess DNRA rates.”

RC1-10: L. 190: did you treat the slurry sample with KCl to desorb the $^{15}\text{N-NH}_4$ that might be attached to the grains? The particle-bound portion of NH_4 can be

significant, so if no KCl was applied or a correction factor used, DNRA rates can be potentially underestimated.

AC-R1-10: Yes, we treated the slurry samples collected for DNRA by adding 200 μL of 2 M KCl directly into the exetainers. This is now mentioned in the methods section 2.7 (L260-262):

“The water samples were then treated 200 μL of 2 M KCl to desorb any particle-bound NH_4^+ followed by the addition of alkaline hypobromite to oxidize NH_4^+ to N_2 prior to analysis using MIMS as described in Yin et al. (2014)”

RC1-11: L. 200-201: checking 28N2 vs 15N-concentration relates to the potential occurrence of anammox, which should be mentioned here. Further, did you also check for the linear increase of D15 with 15N-concentration, needed to assure nitrate-limitation of sediment and functioning of the method / reliability of calculated rates (see Risgaard-Petersen et al. 2003, page 69)?

AC-R1-11: Yes, we verified the linear increase of D15 with 15N-concentration (Fig. 3) to ensure the reliability of the calculated rates, which confirms the sediment was nitrate-limited and the isotope pairing assumptions were met.

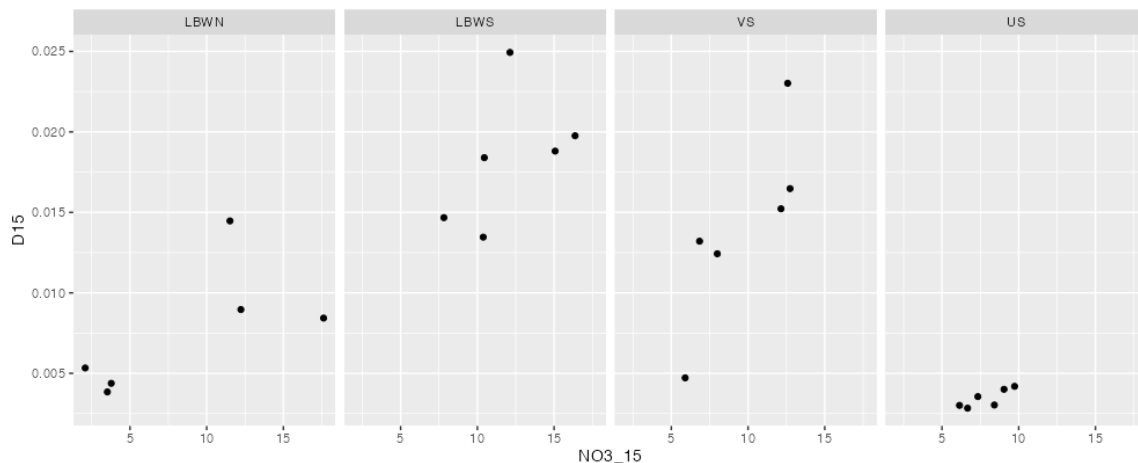


Figure 3: Relationship between denitrification based on ^{15}N -labelled nitrate (D15) and the concentration of ^{15}N - NO_3^- added in the overlying water of the incubators across stations.

RC1-12: L. 423: I am skeptical of the argument that higher denitrification rates at LBWS than at LBWN are due to higher density of macrofauna at LBWS, as the reference site VS had similarly high denitrification rates as LBWS but also similarly low macrofauna as LBWN.

AC-R1-12: We agree that without distinguishing the underlying mechanisms, this comparison appears contradictory. Particularly for the US site for which mechanisms were not detailed. In addition to the changes made to section 4.3 detailed for **RC1-2**, we added a discussion about underlying mechanisms occurring at US (L513-517).

*“By contrast, LBWN showed a trait expression tendency toward US that could yield similar benthic fluxes. The low denitrification rates at US likely reflect the relatively low organic carbon availability compared to impacted sites, which limits local nitrification fuelling denitrification. Moreover, macrofauna at It is worth noting that US is dominated by a few large burrowers (e.g., *Mya* sp.) whose local influence may not be captured at the core scale, leading to low benthic fluxes.”*

Technical comments:

RC1-13: L. 14: nitrogen removal is part of nitrogen-cycling, so maybe reformulate to something like “particularly nitrogen-cycling and its removal processes denitrification and anammox” or similar.

AC-R1-13: This has been reformulated as suggested

RC1-14: L. 19: maybe add some location to the reference sites, such as “reference sites outside the breakwater system”.

AC-R1-14: This has been corrected as suggested

RC1-15: L. 20: “higher organic carbon content” compared to what? Please add.

AC-R1-15: We meant “compared to the other sites”. This has been added

RC1-16: L. 23: “NO₃, NH₄” needs a prior abbreviation or write in full.

AC-R1-16: Abbreviations have been removed from the abstract and replaced by full word.

RC1-17: L. 29: “benefits” or maybe “services”?

AC-R1-17: This has been corrected as suggested

RC1-18: L. 36-38: could you add the difference between the two structures? They sound very similar, but I guess bulkheads might be typically a bit smaller (?).

AC-R1-18: We have added more details to better characterize both structures. Bulkheads are primarily designed to retain soil in low- to moderate-energy environments, whereas seawalls are built to reflect and/or dissipate wave energy along higher-energy shorelines. The corresponding sentences in the introduction now read as (L43-46):

“Bulkheads are vertical walls typically deployed in environments with low to moderate hydrodynamic energy, built directly on the shoreline to retain soil and prevent land loss due to wave action. Seawalls are robust, vertical or sloped structures designed to protect the shoreline in high-energy environment by reflecting and/or dissipating wave energy and preventing coastal flooding and erosion.”

RC1-19: L. 39: “they” don’t refer to the waves (as I guess you wanted to) but rather the structures; please edit.

AC-R1-19: “they” has been replaced by “the waves”

RC1-20: L. 51: coastal eutrophication is predominantly driven by nitrogen-enrichment, which I would put here into focus to also justify why you specifically look at the nitrogen-cycle (rather than the phosphorous-cycle); this also makes your next sentence “understanding nitrogen removal is crucial.” a bit smoother.

AC-R1-20: “phosphorus” has been removed

RC1-21: L. 57: “microbially-mediated” is redundant as you already said that it is a microbial process.

AC-R1-21: This has been removed

RC1-22: L. 59: this is a matter of taste, but in the context of denitrification I would argue that the release of N₂O is more important than that of CO₂; N₂O has a higher warming potential than CO₂ (due to a longer lifetime in the atmosphere) and denitrification can be a significant source of it under certain circumstances; whereas a portion of CO₂ is always emitted and kind of part of the process, as it relates to OC oxidation.

AC-R1-22: This sentence has been modified to reflect the higher warming potential of N₂O compared to CO₂ and should now read as (L65-68):

“(i) Denitrification which involves the oxidation of organic matter (OM) using nitrate (NO₃⁻) as an electron acceptor under anaerobic conditions, resulting mainly in the

production of dinitrogen gas (N_2), but can also release as nitrous oxide (N_2O), a potent greenhouse gas, together with carbon dioxide (CO_2)."

RC1-23: L. 59 ff: anammox is often not too common in coastal systems, which can be organic-rich, thus putting (autotrophic) anammox at a disadvantage compared to (heterotrophic) denitrification, see e.g. Dalsgaard et al. 2005. However, it of course can appear. Maybe reformulate, e.g. either talk about "sediment systems" (rather than "coastal systems", line 56) or add some info to anammox.

AC-R1-23: We agree with the reviewer comment and have modified the sentence accordingly (L64-70).

"Two key microbial processes drive nitrogen removal from soft-sediment systems. (i) Denitrification which involves a oxidation of organic matter (OM) using nitrate (NO_3^-) as an electron acceptor under anaerobic conditions, resulting mainly in the production of dinitrogen gas (N_2), but can also release as nitrous oxide (N_2O), a potent greenhouse gas, together with carbon dioxide (CO_2). (ii) Anammox (anaerobic ammonium oxidation), produces N_2 through the oxidation of ammonium (NH_4^+) coupled to the reduction of nitrite (NO_2^-) although in many organic-rich coastal sediments its contribution can be smaller than that of heterotrophic denitrification (Dalsgaard et al. 2005, Devol, 2015)."

RC1-24: L. 71: please add a "that" after "showed".

AC-R1-24: This has been done

RC1-25: L. 71 ff: do you mean here that the areal nitrogen removal rates would decrease (due to a shrinkage of the area) while the process rates itself would stay constant? Please reformulate for clarity.

AC-R1-25: Yes, O'Meara et al. (2015) showed that, at sites with hardened shorelines, nitrogen removal process rates remained comparable to those at sites without hardened shorelines. However, the primary effect was a reduction in salt marsh area due to coastal squeeze, which ultimately decreases areal nitrogen removal capacity. The sentence has been modified to clarify this point and should read as follows (L81-84):

“Previous research on bulkhead implementation found no direct effects on nitrogen removal rates per unit area, but showed that these structures can reduce the overall nitrogen removal capacity of the ecosystem by decreasing the total area of salt marshes through coastal squeezing, rather than impacting nitrogen removal process rates (O’Meara et al., 2015).”

RC1-26: L. 73: could this be simplified to “when built with sufficient vertical distance between...”

AC-R1-26: We agree this sentence was too complex and we thank the reviewer for their suggestion. We have modified the sentence as suggested.

RC1-27: L. 93: “adaptation” or “structure”?

AC-R1-72: The original sentence “... provides a representative example of such adaptation” has been replaced by (L105): *“... provides a representative example of coastal hardening.”*

RC1-28: L. 93 ff: this sentence seems a bit out of place; is this not exactly what you first want to find out?

AC-R1-28: This sentence has been removed

RC1-29: Fig. 1: maybe a slightly stupid question: is all the green in Figure 1C the saltmarsh?

AC-R1-29: We agree the Fig. 1 did not allow to clearly identify the saltmarsh. This figure has been modified as well as its caption to make it clearer.

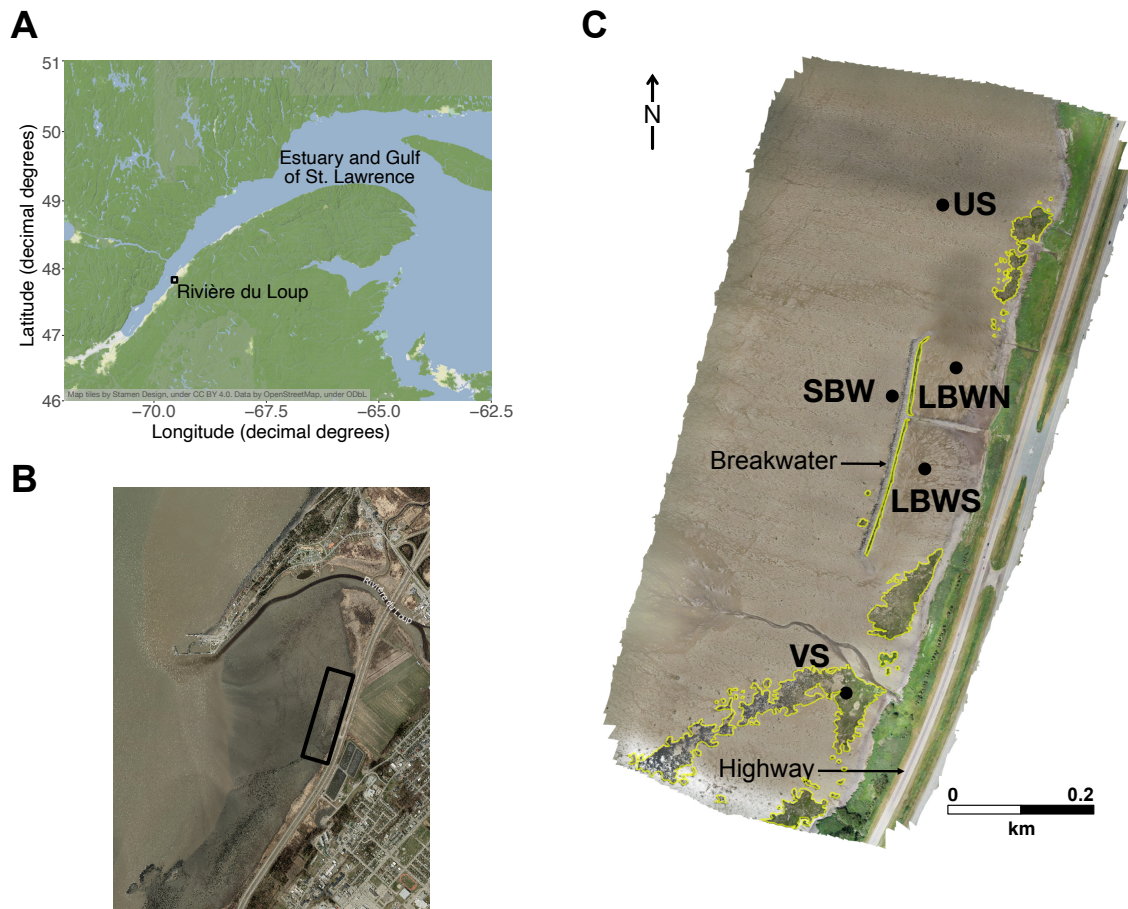


Figure 1: Study site. (a) Location of the study area (open square) in the St. Lawrence system, (b) location of the study site (black rectangle) in the bay of Rivière-du-Loup and (c) aerial view of the sampling location around the breakwater (from Zouaghi et al., 2024) with saltmarsh areas highlighted by yellow contours. Reference sites: Vegetated Sediment (VS), and Unvegetated Sediment (US); Impacted sites: Southern area landward of the breakwater (LBWS), Northern area landward of the breakwater (LBWN); and Intermediate site: Seaward of the breakwater (SBW). (a) © Stamen Design (CC BY 4.0) / © Stadia Maps; © OpenStreetMap contributors (ODbL), (b) © Gouvernement du Québec (CC BY 4.0; MNRF 2025).

RC1-30: L. 102: what is the trophic status of the bay, resp. what is the nutrient load of the river / does it carry a significant nutrient load into the bay? This might not be essential for your study, but it gives some background understanding, especially as you mention coastal eutrophication in your introduction.

AC-R1-30: The river delivers a low to moderate nutrient load under mean conditions and is unlikely to drive eutrophication in the bay. Nevertheless, episodic high-

discharge events, typically during spring freshet, may represent significant short-term nutrient inputs. Mean annual concentration have been added to the text to provide background context (L115-117):

“The Rivière-du-Loup embayment receives freshwater inputs from a river located in its northern part, with a mean annual discharge of 14.7 m³ s⁻¹ (from 3.4 to 141.1 m³ s⁻¹) and mean annual concentrations of 2.7, 13.7 and 0.5 µmol L⁻¹ for NH₄⁺, NO₃⁻ and PO₄³⁻, respectively.”

RC1-31: L. 118-119: maybe one time “intertidal” is enough.

AC-R1-31: We agree and have removed the multiple “intertidal” words.

RC1-32: L. 129: the sediment cores were 15 cm long but the acrylic liners were 30 cm long- was the leftover 15 cm then overlaying water or air? Please clarify.

AC-R1-32: Sediment collection was performed at low tide, therefore, the liners were filled with 15 cm of sediment and the remaining headspace was air. The sediment surface was then capped with Styrofoam® before adding overlying water collected separately from the bay. The sentence has been modified and should read as follow (L147-150):

“At each of the five sites, eight large sediment cores (10 cm diameter, ~ 15 cm of sediment and ~ 15 cm of air) were manually collected at low tide using transparent acrylic liners (30 cm long), with compaction < 0.5 cm. The sediment surface was immediately capped with Styrofoam® to prevent disturbance during transport.”

RC1-33: L. 141 ff: you say that sediment characteristics were analyzed from the small cores taken at each location, but in the following sentences you specify to “analyzed in one sediment core”- this is a bit confusing (also, which core was chosen?), but as you have a separate chapter dealing with your replicates, maybe you could reformulate the sentence here a bit and refer to the other chapter?

AC-R1-33: This paragraph has been rewritten to make it clearer and now reads as follows (L164-186):

“2.4 Characterization of the sediment

Sediment characteristics were analyzed using samples from the top layer (0 - 0.5 cm) of the five small sediment cores collected at each location. One core was dedicated to measuring sediment grain size (D_{50}), porosity (ϕ), and organic carbon (C_{org}) and total nitrogen (TN) contents, as well as C_{org} natural isotope composition ($\delta^{13}C_{org}$). From this core, D_{50} was assessed by laser diffraction (Malvern Instruments®, 2 μm detection limit) after OM digestion with hydrogen peroxide and porosity was determined by measuring water loss after freeze-drying, corrected for sea salt content, using a dry sediment density of 2.65 g cm⁻³. Prior the C_{org} and $\delta^{13}C_{org}$ analyses, inorganic carbon was removed by fuming of sediments in a closed container in the presence of an open beaker of concentrated HCl. Elemental characteristics of the OM, (i.e., C_{org} , TN) of the sediment were determined using an elemental analyzer (Carlo Erba® NC2500). The $\delta^{13}C_{org}$ was then measured using a Micromass Isoprime 100 isotope ratio mass spectrometer coupled to an Elementar Vario MicroCube elemental analyzer. Carbon isotope ratios are reported using the delta notation, relative to the Vienna Pee Dee Belemnite standard. The remaining sediment from this first core, along with the other four cores, was used to determine the OM content via loss on ignition (LOI, $n=5$), as the weight loss after combustion at 550 °C during 4 h. Due to storage failure, LOI was not assessed at US.”

RC1-34: L. 144: could you add a small description after LOI like you did for porosity, i.e. “weight loss after combustion”?

AC-R1-34: This has been added (see text in **AC-R1-33**)

RC1-35: L. 148: do you mean “natural” rather than “stable”? While it’s both, it’s commonly called natural $\delta^{13}C$ abundance.

AC-R1-35: “stable” has been replaced by “natural”

RC1-36: L. 162: please add the meaning of the variables.

AC-R1-36: The meaning of the variable in Eq. 1 has been added (L202):

“where ϕ is the sediment porosity, D_0 the diffusion coefficient ($m^2 h^{-1}$) and $\delta C/\delta z$ is the concentration gradient ($mmol m^{-3} m^{-1}$)”

RC1-37: L. 233: I feel this chapter is not needed but the given information could be integrated in the previous chapters, such as line 237 ff would fit better after l. 190. Also, the information from l. 239 should come earlier, as the result (no DNRA) was already given in l. 202.

AC-R1-37: This paragraph was originally isolated to consolidate all analytical details within a single section. However, we agree that this may have appeared redundant with information provided earlier in the text. Following the reviewer's suggestion, we have moved the relevant analytical details to their respective sections where the corresponding experiments are described and removed the "Sample analyses" section accordingly.

RC1-38: L. 241: hypobromite *iodine*?

AC-R1-38: Yes, this has been added

RC1-39: L. 286 ff: was the difference statistically significant?

AC-R1-39: This point required clarification. We did not report formal statistical results for sediment characteristics in the manuscript because replication levels were uneven among variables and stations, as explained in the methods (Section 2.10). However, we did perform statistical analyses where replication allowed. For LOI, replication was sufficient ($n = 5$) to support a significant difference between the impacted sites (LBWN and LBWS) and the reference stations (SBW and VS) with $p < 0.01$. For D50, the low replication level ($n = 2$) limited the statistical power to reliably assess pairwise differences although the main test showed the presence of significant differences between stations ($p < 0.05$). Therefore, for consistency across sediment characteristics, we chose not to report these statistical results in the manuscript and instead used descriptive wording such as "showed" or "tended to".

RC1-40: L. 288: "TS" probably means "TN"

AC-R1-40: Yes, this has been corrected

RC1-41: L. 301 ff: three times "notable / notably" in three consecutive sentences is a bit too much.

AC-R1-41: We agree and this has been corrected

RC1-42: L. 365: what “remains limited”: the effect of artificial structures on ecosystems or the amount of studies dealing with that? Please reformulate.

AC-R1-42: We agree the original phrasing was ambiguous. The sentence has been reformulated to clarify that it is the scientific knowledge on this topic that is currently limited, not the effect itself. "Remains limited" has been replaced by "remains poorly documented" to remove this ambiguity.

RC1-43: L. 383: Please reformulate this sentence, as coupled nitrification-denitrification cannot have been enhanced by rhizone oxidation, as you used different cores for rate measurements and porewater sampling.

AC-R1-43: The reviewer is correct that porewater profiles and denitrification rate measurements were obtained from different set of cores, preventing a direct mechanistic link. We have reformulated the sentence to present the geochemical profiles (NO₃ and Feasc) as independent corroborating evidence consistent with rhizosphere-driven coupled nitrification-denitrification, rather than as a direct explanation for the incubation results. The causal language has been replaced with an interpretive one. This part now reads as follows (L457-466):

“This consumption results from the denitrification process, which has been detected at all sites (Fig. 6). VS exhibited a subsurface NO₃⁻ peak, suggesting active nitrification consistent with rhizosphere oxygenation (Koop-Jakobsen et al., 2018), this pattern is consistent with the relatively high proportion of coupled nitrification-denitrification detected in incubation experiment. This interpretation is further corroborated by the solid-phase ascorbate extractable iron (Feasc) profiles (Fig. 2d). At VS, Feasc increase progressively from the surface to a peak at 3-4 cm depth, well below the NO₃⁻ maximum (Fig. 2c), suggesting subsurface oxidation consistent with rhizosphere oxygenation. The absence of NO₃⁻ peak co-located with the Feasc peak suggests a tight coupling between nitrification in the rhizosphere area and denitrification in adjacent anoxic sediment (Koop-Jakobsen and Giblin 2010, Zheng et al. 2016).”

RC1-44: L. 387: the impacted sites had also higher organic matter, so it makes sense that they have higher NH₄ accumulations in porewaters.

AC-R1-44: We agree with the reviewer. The text has been amended to explicitly acknowledge that the higher NH₄ accumulation observed in porewater at impacted

sites is consistent with their higher organic matter content, which fuels greater ammonification. The sentence now reads (L467-472):

“Similarly, deeper sediment layers in impacted sites exhibit greater increases in NH_4^+ and PO_4^{3-} compared to reference sites, indicating larger ammonification driven by their higher organic matter content, along with PO_4^{3-} release, typically associated with the reductive dissolution of iron-oxyhydroxides in anoxic sediment layers (Slomp, 2011). Consistently, Fease concentrations at impacted sites are highest at the sediment surface and decrease rapidly with depth (Fig. 2d). This indicates that aerobic processes, including nitrification, are restricted to the very surface of the sediment at these sites.”

RC1-45: L. 390 ff: I slightly disagree with the argument / conclusion here, as earlier you suggested that nitrogen-cycling was not largely affected by the change in sediment biogeochemistry.

AC-R1-45: We agree with the reviewer. The original conclusion was inconsistent with our results, as denitrification rates were not substantially altered at impacted sites despite changes in sediment biogeochemistry. We have amended the text to reflect this, replacing the speculative statement about potential implications for nitrogen removal capacity with a conclusion grounded in our findings. The text now reads as follows (L472-477):

“Overall, these O_2 and nutrient profiles suggest more reduced conditions and greater nutrient accumulations in the sediment column in impacted sites compared to reference sites. Despite these changes in sedimentary conditions, denitrification rates were not substantially altered, suggesting that the capacity of these sediments to remove excess nitrogen remains resilient to these perturbations.”

Reviewer 2

General Comments:

RC2-1: Pascal et al. have provided a well-written and concise manuscript about the biogeochemical and macrofaunal composition of sediments, both impacted and unimpacted by a 300 m-wide breakwater on the south shore of the St. Lawrence

Estuary. In the introduction, the authors outlined the global relevance of the study by detailing the increasing prevalence of shoreline stabilization structures in response to sea-level rise and extreme climatic events. The authors indicated that there is a research gap in how these structures affect nitrogen cycling and potential coastal eutrophication. The methods and results sections are clearly presented and relevant to the objectives of the study. The authors found significant differences in sediment characteristics and macrofaunal composition, yet nitrogen fluxes were not significantly different between impacted and reference sites. The authors conclude by suggesting that functional plasticity within their study area allows for minimal differences in nitrogen cycling. Overall, I believe that this study provides an interesting and relevant contribution to Biogeosciences and is suitable for publication, with minor revisions.

AC-R2-1 : Dear Reviewer 2,

We thank you for your careful evaluation of our manuscript and supportive and constructive comments. We have carefully addressed all the concerns raised and responded to each comment below.

Please note that, as we have added new data to address both reviewers' comments, we have included Carole-Anne Guay in the author list. She collected the additional sediment samples and performed the solid-phase ascorbate-extractable iron analyses presented in the revised manuscript.

Specific Comments:

RC2-2: Section 2.1 Study area: Please provide some brief information on water temperature and salinity in your study area.

AC-R2-2 : Temperature and salinity during field campaigns are now given in section 2.3 (L143-144).

“Sea water temperature and salinity during sampling were similar between years: 9 °C and 19.6 in 2021 and 10 °C and 18.7 and 2022.”

RC2-3 : Line 125: My question is related to sample collection taking place over two years (June 2021 and 2022). This needs to be addressed and explained clearly. Was each site sampled twice (once each year) or were some sites sampled in 2021 and others sampled in 2022? Depending on how the sampling was conducted, please address the differences in ice cover between the two years (e.g., Galbraith et al.

Physical Oceanographic Conditions in the Gulf of St. Lawrence during 2021 and 2022) and how that could affect the data.

AC-R2-3: We apologize for the confusion. See our response to RC1-4 below:

AC-R1-4: “All benthic flux incubations, including isotope pairing experiments, porewater extraction, and macrofauna sampling were conducted during a single campaign in June 2021, ensuring no interannual variability in our main dataset. The June 2022 campaign was specifically dedicated to strengthening this dataset by performing O₂ microprofiles at SBW (which was not possible in 2021 due to logistical constraints) and to increasing the replication of baseline sediment characteristics. Cross-validation between the two field campaigns confirmed that spatial gradients remained consistent across years, based on sediment characteristics and O₂ microprofiles at VS and US (Fig 1 and 2 below), further supporting the temporal stability of our observations.”

Regarding interannual differences in ice cover, Galbraith et al. report that 2021 was characterized by anomalously low ice cover and volume in the Gulf of St. Lawrence, while 2022 was close to the long-term normal. However, these reports document regional-scale ice conditions and do not resolve local ice dynamics in coastal embayment such as Rivière-du-Loup Bay. Field observations before both campaigns confirmed the presence of sea ice in the bay at the study site in both years. Since all primary biogeochemical and ecological measurements were conducted in 2021, any interannual difference in sea ice conditions between 21 and 22 does not affect our main dataset. In addition, cross-validation was performed to support the representativeness of the micropifiles acquired at SBW in 2022 (See AC-R1-4 for further details).

RC2-4: Additionally, the authors specify that sampling was targeted for the beginning of the salt marsh growing season following winter ice cover. Please clarify why that is the case (e.g., due to short leaf length, which is mentioned on lines 171-172, or another reason?).

AC-R2-4: We have clarified the rationale for our sampling timing in the revised text. The beginning of the growing season was targeted for two reasons: (1) root oxygen release and rhizosphere processes are expected to be active during early plant growth, influencing nitrogen cycling; and (2) the short leaf canopy at this stage allows intact core incubation without cutting shoots. These sentences now read as follows (L139-144):

“Sampling was performed in June 2021 and 2022, targeting the beginning of the salt marsh growing season following winter ice cover break-up. This period was chosen because salt marsh plants are entering their active growth phase, during which root oxygen release and associated rhizosphere processes are expected to influence sediment nitrogen cycling (Koop-Jakobsen & Giblin, 2010; Zheng et al., 2016). Additionally, the relatively short leaf canopy at this stage allows intact sediment cores with vegetation to be incubated without cutting shoots and associated artefacts. Sea water temperature and salinity during sampling were similar between years: 9 °C and 19.6 in 2021 and 10 °C and 18.7 and 2022.”

RC2-5: Line 130: “with few if any core compaction”, please be more specific. This reads as if you do not know if there was or not.

AC-R2-5: We apologize for the lack of precision. After verifications of our field records, we updated the sentence, which now reads as follows (L147-149):

“At each of the five sites, eight large sediment cores (10 cm diameter, ~ 15 cm of sediment and ~ 15 cm of air headspace) were manually collected at low tide using transparent acrylic liners (30 cm long), with compaction < 0.5 cm.”

RC2-6: Line 142: “hydrogen peroxide on one sediment core”, are you referring to the small sediment core collected at each location? Please specify.

AC-R2-6: This paragraph has been corrected. See our answer to RC1-33. The new paragraph reads as follows (L164-186):

“2.4 Characterization of the sediment

Sediment characteristics were analyzed using samples from the top layer (0 - 0.5 cm) of the five small sediment cores collected at each location. One core was dedicated to measuring sediment grain size (D_{50}), porosity (ϕ), and organic carbon (C_{org}) and total nitrogen (TN) contents, as well as Corg natural isotope composition ($\delta^{13}C_{org}$). From this core, D_{50} was assessed by laser diffraction (Malvern Instruments®, 2 μm detection limit) after OM digestion with hydrogen peroxide and porosity was determined by measuring water loss after freeze-drying, corrected for sea salt content, using a dry sediment density of 2.65 g cm⁻³. Prior the Corg and $\delta^{13}C_{org}$ analyses, inorganic carbon was removed by fuming of sediments in a closed container in the presence of an open beaker of concentrated HCl. Elemental characteristics of the OM, (i.e., Corg, TN) of the sediment were determined using an elemental analyzer (Carlo Erba® NC2500). The $\delta^{13}C_{org}$ was then measured using a

Micromass Isoprime 100 isotope ratio mass spectrometer coupled to an Elemental Vario MicroCube elemental analyzer. Carbon isotope ratios are reported using the delta notation, relative to the Vienna Pee Dee Belemnite standard. The remaining sediment from this first core, along with the other four cores, was used to determine the OM content via loss on ignition (LOI, n=5), as the weight loss after combustion at 550 °C during 4 h. Due to storage failure, LOI was not assessed at US.”

RC2-7: Line 146: Same question as line 142.

AC-R2-7: See our answer to RC2-6.

RC2-8: Figure 2c. Please define the square data points above the 0cm depth line.

AC-R2-8: The squares represented the nutrient concentrations in the overlying water. It has been removed because not relevant in the context of this study (L380-383).

“Figure 2: Porewater solute and solide phase iron concentrations. (a) Diffusive O₂ uptake (DOU) by the sediment and (b) O₂ penetration depth (OPD). (c) O₂ microprofiles at each site (d) porewater NO₂⁻, NO₃⁻, NH₄⁺ and PO₄³⁻ concentrations at each site (n = 1 with 5 pseudo replicates for DOU and OPD). (e) Reactive fraction of iron-oxyhydroxides (Fe_{asc}) at each site. Different letters indicate significant (p < 0.05, Tuckey contrast) differences among the different levels of Site (a and b)”

RC2-9: Line 313: “densities tended to be higher at the sites LBWS and LBWN than at VS and US (Fig. 3).” This is a misleading statement since LBWN is statistically similar to VS and US according to Fig 3.

AC-R2-9: We agree and we have corrected the sentence which should now read as follows (L387):

“Total densities tended to be higher at the sites LBWS than at LBWN, VS and US (Fig. 3).”

RC2-10: Discussion: I would like to see more discussion regarding why the authors think that nitrogen cycling was similar between sites, despite differences in sediment characteristics and macrofaunal assemblages. The authors could compare results to other studies and provide hypotheses on why they are not seeing impacts from the

structure. This seems to be a significant missing piece given the emphasis on nitrogen throughout the rest of the manuscript, including the title.

AC-R2-10: We agree with the reviewer, and Reviewer 1 raised a similar concern (RC1-2). We have thoroughly revised Sections 4.1 and 4.3 to address this point. Briefly, we argue that similar denitrification rates across sites despite contrasting sediment characteristics and macrofaunal assemblages result from distinct but functionally equivalent biogeochemical pathways: at VS, rhizosphere oxygenation by *S. alterniflora* roots sustains sub surface coupled nitrification-denitrification, while at LBWS, high densities of subsurface bioturbators enhance sediment irrigation and oxygen supply, achieving comparable nitrogen removal through a bioturbation-driven pathway. We also now compare our denitrification rates with published values for temperate intertidal mudflats and salt marshes to contextualize our findings. We refer the reviewer to our detailed response in AC-R1-2 for a full account of the revisions made to the Discussion.

Technical Corrections:

RC2-11: 92: “south shore” instead of “South shore”.

AC-R2-11: This has been corrected

RC2-12: Figure 1a: Capital L for both Latitude and Longitude. Decimal Degrees on both axes (latitude currently says decimals degree). Capital G for Gulf of St. Lawrence.

AC-R2-12: This has been corrected, See our answer to reviewer 1 (AC-R1-29) for the new Fig. 1.

RC2-13: Lines 131-132: diameter, 1 cm long) “were” collected at each site for analysis “of” surficial sediment characteristics, and stored at -20C until “analysis”.

AC-R2-13: This has been corrected

RC2-14: Line 152: within an hour “of” sampling...

AC-R2-14: This has been corrected

RC2-15: Line 154: using ice “packs”...

AC-R2-15: This has been corrected

RC2-16: Line 185: A pre-incubation period of 3 “h”...

AC-R2-16: This has been corrected.

RC2-17: Line 240: gently bubbled with He “for” 5 min...

AC-R2-17: This has been corrected.

RC2-18: Line 267: measured on “a” separate single sediment core...

AC-R2-18: This has been corrected.

RC2-19: Line 323: “and” Gastropods).

AC-R2-19: This has been corrected.