

Response to Reviewer comments on manuscript egusphere-2026-582 formerly entitled “Different paths, same destination: similar functional outcomes in nitrogen cycling within artificialized coastal habitat” changed to “Different paths, same destination: similar benthic nitrogen cycling sustained by distinct biogeochemical mechanisms in artificialized coastal systems”.

R1-1: I reviewed also the previous manuscript version of (now) “Different paths, same destination: similar benthic nitrogen cycling sustained by distinct biogeochemical mechanisms in artificialized coastal systems” and I thank Pascal et al. for their efforts in answering all my question. The manuscript has significantly improved. I only have a few question / comments left regarding the revisions:

AC-R1-1: We thanks Reviewer1 for their encouraging comments on our manuscript. We would like to extend our warmest thanks to Reviewer1 for their thorough reading of the manuscript and their expert and constructive comments, which have greatly contributed to improving the manuscript. Their attention to details is much appreciated.

Please note that line numbers given here correspond to the manuscript with track change.

R1-2: AC-R1-4: I would recommend the authors to add the plot Fig. 1 showing the low variation between different sampling years to the appendix of the actual manuscript, as I am probably not the only reader thinking about annual variation. By showing this plot in the appendix, the authors would increase the readers confidence in the data.

AC-R1-2: We agree that showing the interannual comparison will allow readers to assess the temporal consistency of the observed spatial patterns more directly. We have therefore added the two plots previously presented in our response as Fig. S1 and S2 in the Supplement. We have also referred to this figure in Sect. 2.3, where the respective contributions of the 2021 and 2022 sampling campaigns are now described more explicitly.

R1-3: AC-R1-9: I understand why the authors did not take slurry samples for $^{29}\text{N}_2$, $^{30}\text{N}_2$ determination at the end of their ^{15}N -incubation period, given that they had started the incubations with a different analytical design. However, the fact remains that a lot of the produced $^{29}\text{N}_2$ and $^{30}\text{N}_2$ will remain in the porewater and not efflux sufficiently over the incubation time. By ignoring that fraction, you likely underestimate the denitrification rates, specifically the Dn fraction. As you focus especially on coupled nitrification-denitrification in the deeper root- and bioturbated zone, it seems a bit strange not to actually take the porewater fraction into account. Hence, I would suggest to at least add a sentence

regarding this issue to your text and maybe take your data as a minimum estimate that however could be higher.

AC-R1-3: We agree that our experimental design did not quantify the terminal inventory of 29N2 and 30N2 dissolved in sediment porewaters. Consequently, complete recovery of the labelled N2 produced during the incubations cannot be verified. We have now acknowledged this limitation in Sect. 2.10. The linear accumulation of 29N2 and 30N2 in the overlying water nevertheless indicated consistent transfer of labelled N2 across the sediment-water interface during the measurement period. Because all incubations were conducted and analysed using the same standardized protocol, the reported rates provide consistent measurements of labelled N2 accumulation across sites. Because the porewater inventory was not measured, the magnitude of any unrecovered fraction and whether it preferentially affected Dn cannot be determined from our dataset. We have also noted that future studies could quantify the terminal porewater inventory to assess its contribution to total labelled N2 production

L356-362:

“Denitrification rates were derived from the linear accumulation of 29N2 and 30N2 in the overlying water, indicating consistent transfer of labelled N2 to the overlying water during the incubation. Because the terminal inventory of labelled N2 dissolved in sediment porewaters was not quantified, we cannot exclude that some labelled N2 produced during the incubations had not reached the overlying water by the end of the experiment. Nevertheless, all incubations were conducted and analysed using the same standardized protocol, providing consistent measurements of labelled N2 accumulation across sites. Future studies could quantify the end point porewater inventory of labelled N2 to assess its contribution to total production.”

Furthermore, I have a few comments to the revised manuscript, line numbers refer to the clean manuscript version:

R1-4: L. 22: please add comma after “Oligochaetes”

AC-R1-4: This has been corrected

R1-5: L. 26: what about the intermediate site regarding potential differences?

AC-R1-5: Information about the intermediate site have been added

L22-27:

“The intermediate seaward site consistently resembled the unvegetated reference site across all variables measured at both sites, including sediment characteristics, oxygen dynamics and benthic fluxes, thereby showing reference-like conditions. Surprisingly, despite these substantial physical and biological changes, dark total benthic oxygen uptake,

nitrate and ammonium benthic fluxes showed no consistent separation among impacted, intermediate, and reference sites, while benthic denitrification rates were comparable among the impacted and reference sites where they were measured.”

R1-6: L. 26-27: I would suggest the authors to add at least one sentence to the abstract regarding the explanation of their functional convergence, such as the root oxygenation or fauna burrow oxygenation

AC-R1-6: We agree and have added a concise mechanistic explanation to the abstract. we now refer to the different combinations of sediment conditions, biological oxygenation pathways, and nitrate sources underlying the observed functional similarities across sites.

L28-30:

“This functional similarity reflects an apparent functional convergence arising from distinct combinations of abiotic and biotic factors, including root-mediated rhizosphere oxygenation, macrofauna-mediated sediment irrigation, and contrasting reliance on internally produced versus water-column nitrate.”

R1-7: L. 48: could you give some examples for the mentioned ecosystem services? Or are the following sentences the examples? Maybe edit a bit to make this clear.

AC-R1-7: The following sentences were indeed intended to provide examples of the ecosystem services delivered by salt marshes. We added “Indeed” at the beginning of the following sentence to make this connection explicit.

L51-54:

“Salt marshes are important coastal ecosystems developing in low-energy shorelines, in temperate and cooler regions. They provide some of the most valuable ecosystem services (Barbier et al., 2011). Indeed, these intertidal habitats play a crucial role in coastal protection by stabilizing sediments, attenuating wave energy, and enhancing sediment deposition, thereby helping to mitigate coastal erosion (Duarte et al., 2013; Gedan et al., 2009).”

R1-8: L. 61: denitrification does not oxidize organic matter but organic carbon (electron donor), please correct.

AC-R1-8: This has been corrected

L64-67:

“Two key microbial processes drive nitrogen removal from soft-sediment systems. (i) Denitrification which involves the oxidation of organic matter carbon (OC) using nitrate (NO₃⁻) as an electron acceptor under anaerobic conditions, resulting mainly in the production of dinitrogen gas (N₂), but can also release as nitrous oxide (N₂O), a potent greenhouse gas, together with carbon dioxide (CO₂).”

R1-9: L. 63: Anammox is the abbreviation for anaerobic ammonium oxidation, hence, please switch those around in your text, starting with the full name.

AC-R1-9: This has been done

R1-10: L. 75: habitats (in plural)

AC-R1-10: This has been corrected

R1-11: L. 83: “such changes may enhance...” as you mention earlier, that anammox is actually at a disadvantage in OM-rich sediments, I don’t think the here mentioned OM-enrichment will favour it. Maybe reformulate to sth like “...may enhance favourable conditions particularly for denitrification”.

AC-R1-11: This has been reformulated

L86-90:

“These conditions promote the sedimentation of fine, organic-rich particles (Gittman et al., 2016), which can substantially alter macrobenthic communities, and biogeochemical processes. Such changes may enhance favourable conditions for heterotrophic denitrification by increasing OC availability and the extent of anoxic sediment, potentially increasing nitrogen removal rates (Albert et al., 2021; Stief, 2013).”

R1-12: L. 87: “sedimentary biogeochemistry” commonly termed “sediment or benthic biogeochemistry”

AC-R1-12: “Sedimentary” has been replaced by “sediment”.

R1-13: L. 106-107: could the references for figure a) and b) be given directly at a) and b), not at the very end?

AC-R1-13: This has been done

R1-14: L. 108-110: sentence needs references

AC-R1-14: A reference has been added.

“MELCCFP: Ministère de l’Environnement, de la Lutte contre les changements climatiques, de la Faune et des Parcs, Hydrometric station 030240 (Rivière Rimouski), daily discharge data. Gouvernement du Québec., 2026.”

R1-15: L. 111: I would suggest to start a new sentence after 3.2m, as you start a different topic

AC-R1-15: This has been done

R1-16: L. 116: “which has not been compensated for” is this necessary information for the work?

AC-R1-16: We agree this information was not necessary for this work. This text has been removed from the manuscript.

R1-17: L. 119-120: sentence needs a reference

AC-R1-17: A reference has been added

“Zouaghi, F., Dansereau-Macias, E., Caulet, C., Dugas, S., Pascal, L., Chaillou, G., Sirois, L., Bernatchez, P., Nozais, C., Roberge, B., Noël, B., Bujold, É., and Didier, D.: Analyse, modélisation et suivi des interactions entre le briselames et la dynamique sédimentaire et végétale du marais le long de l’autoroute 20 à Rivière-du-Loup, Présenté au ministère des transports du Québec, 2024.”

R1-18: L. 127: “Two other sites, a vegetated (VS) and an unvegetated (US) sediment areas” maybe add “outside the breakwater” to explain why they can serve as reference

AC-R1-18: This has been added

L135-136:

“Two other sites outside the breakwater, a vegetated (VS) and an unvegetated (US) sediment areas served as “reference” conditions.”

R1-19: L. 132: You explain in your reviewer comments, why you sampled in 2 years but you did not add this information sufficiently here in the text. As a reader, I would need

directions as to which parameters were sampled in which years straight here at the start to better understand/follow, what you did. Please add a clear explanation as to what was sampled in which years and why.

AC-R1-18: We agree that this information is needed to better understand our work. The section 2.3 has been edited accordingly. *Italic* corresponds to the adjustments applied.

“The main sampling campaign was conducted in June 2021, at the beginning of the salt-marsh growing season following winter ice-cover breakup. An additional campaign was conducted in June 2022 to address the specific data gaps described below. 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 and Giblin, 2010; Zheng et al., 2016). Additionally, the relatively short leaf canopy at this stage allowed intact vegetated sediment cores to be incubated without clipping the aboveground shoots, thereby avoiding disturbance associated with shoot removal. Sea water temperature and salinity during sampling were similar between years: 9 °C and 19.6 in 2021 and 10 °C and 18.7 in 2022. A 20 L volume of seawater was collected at high tide on the sampling day in the bay. This seawater was filtered on-site using a GF/F filter and aerated in an opaque container for later use. 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. The sediment surface was immediately capped with Styrofoam to prevent disturbance during transport. Additionally, five small sediment cores (2.5 cm diameter, 1 cm long) were collected at each site for analysis of surficial sediment characteristics (see section 2.4), and stored at – 20°C until analysis. Six of the eight large sediment cores were kept intact and immediately transported to the laboratory to assess total benthic fluxes. Of the two remaining cores, one was used to obtain O₂ microprofiles at the sediment-water interface and subsequently sliced for particle phase analysis, while the other was dedicated for porewater extraction. To assess macrobenthic community structure, sediment from three box corers (40 × 40 × 15 cm depth) was collected at each site. This sediment was sieved through a 0.5 mm mesh and preserved in a 4% buffered formaldehyde solution for further analysis. Due to sampling failure at SBW, total benthic fluxes were assessed on only four sediment cores, and no isotope pairing incubation, porewater extraction, or macrofauna sampling were performed at this site. To address these data gaps, an additional campaign was conducted in June 2022 to obtain O₂ microprofiles at SBW and to provide a second measurement of selected sediment characteristics at all sites. All benthic flux and isotope-pairing incubations, porewater extractions, and macrofauna sampling were conducted in 2021. The sediment characteristics measured in both years showed consistent spatial patterns, supporting the temporal representativeness of the site comparisons (Fig. S1 and S2).”

R1-20: L. 136: “cutting shoots and associated artefacts”, needs editing

AC-R1-20: This has been done. See the edited text in our response AC-R1-19

R1-21: L. 137: “18.7 and 2022” please change “and” to “in”

AC-R1-21: This has been done. See the edited text in our response AC-R1-19

R1-22: L. 137-138: is this sentence needed?

AC-R1-22: This has been removed. See the edited text in our response AC-R1-19

R1-23: L. 152: is “perform O₂ microprofiles” the same as “vertical distribution of the dissolved O concentrations” (l. 146)?

AC-R1-23: This has been corrected. See the edited text in our response AC-R1-19

R1-24: L. 157: natural isotope “abundance” not “composition”

AC-R1-24: This has been done

R1-25: L. 158: I would suggest to start a new sentence after “peroxide”, as you start a new topic

AC-R1-25: This has been done

R1-26: L. 160: prior “to”

AC-R1-26: This has been corrected

R1-27: L. 161: consider simplifying the sentence, such as “Corg and TN was determined..., after which...delta 13C was measured...”

AC-R1-27: This sentence has been simplified

L177-179:

“Corg and, TN of the sediment were determined using an elemental analyzer (Carlo Erba NC2500), after which $\delta^{13}\text{C}_{\text{org}}$ was measured using a Micromass Isoprime 100 isotope ratio mass spectrometer coupled to an Elementar Vario MicroCube elemental analyzer.”

R1-28: L. 168: “the two cores”, maybe give some orientation what these two cores are, it comes a bit abrupt

AC-R1-28: The sentence has been edited to make it clearer.

L184-185:

“A 500 mL volume of aerated seawater was gently added at the top of the two large cores dedicated to O₂ microprofiles and porewater extraction and the overlying water was then aerated.”

R1-29: L. 169: “distribution” should be “concentration”

AC-R1-29: This has been corrected

R1-30: L. 170: please give the specification about the microelectrodes, given in line 173, already when mentioning the microelectrodes for the first time in line 170

AC-R1-30: This has been done

R1-31: L. 173, 174 etc: I don’t think you need to add a © behind the manufacturers of your equipment- I have never seen this in scientific publications

AC-R1-31: The “©” has been removed throughout the manuscript.

R1-32: L. 174: how did you identify the sediment surface during your microprofiling?

AC-R1-32: The sediment surface was identified from the position of the abrupt change in O₂ concentration. This is now mentioned in the section 2.5.

L191-192:

“The sediment surface was identified as the depth corresponding to the abrupt change in O₂ concentration and was assigned a depth of 0.”

R1-33: L. 186-188: please cut into two sentences; further, please edit “we cannot exclude that nutrient profiles in the uppermost cm of the sediment column are slightly smoothed due to limited smearing from adjacent layers”- if smearing would be limited, there wouldn’t be smoothing, or?

AC-R1-33: This sentence has been corrected

L205-207:

“Porewater extraction was limited to 8 mL per depth to reduce overlap between adjacent sampling volumes. Nevertheless, some overlap between the volumes sampled by neighbouring Rhizons cannot be excluded and may have smoothed concentration gradients in the uppermost centimetre.”

R1-34: L. 190: sediment samples from which core?

AC-R1-34: This information has been added

L212-213:

“Following O₂ microprofiling, the same large core was sectioned, and homogenized freeze-dried sediment from each depth interval was used for reactive Fe oxyhydroxide extraction using a buffered ascorbate solution.”

R1-35: L. 200-201: maybe there would be a better place for this sentence rather than straight at the beginning?

AC-R1-35: This sentence has been removed since and is now in the section 2.3 on sampling procedure. See our response AC-R1-19

R1-36: L. 205-208: this description should come much earlier in the text as it deals with the preparations of the incubation

AC-R1-36: The section 2.7 on sediment core incubation has been edited accordingly. Italic corresponds to the adjustments applied.

“Six sediment cores were fitted with PVC lids equipped with a magnetic stirrer. *Prior to the initial incubation, the overlying water in the sediment cores was carefully filled with previously collected and bubbled seawater, and the cores were left open and bubbled overnight for acclimation. The following day, the overlying water was completely replaced with fresh, previously collected and bubbled seawater, and left open while being gently bubbled with air for two hours before starting the experiment to restore initial oxygen and*

nutrient conditions. 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) connected to a multichannel O₂ meter (OXY-10 SMA; PreSens) through optical fibre cables. Incubations were performed in the dark at the in situ water temperature (i.e., 9 ° C). The incubation duration was adjusted so that O₂ saturation in the overlying water never dropped below 80% of its initial value (typically between 1.5 and 4 h). During the incubations, discrete water samples were collected each hour, filtered (0.2 µm) and stored at - 20 ° C for subsequent nutrient analyses. Water removed by sampling was automatically replaced with previously collected and bubbled seawater. Overall, cumulative volume of replacement waters corresponded to <5% of the overlying water and its dilution effect was thus considered as negligible. After the initial incubation period, two thirds of the overlying water volume in each sediment core was replaced with *freshly aerated site water to restore initial oxygen and nutrient conditions and minimize carry-over from the preceding flux incubation.* The sediment cores were then left open, and the overlying water was gently bubbled with air for two hours. Then, an initial water sample was extracted from the overlying water of all six sediment cores to determine the ambient NO₃⁻ concentration before the addition of 15N - NO₃⁻ stable isotope. A calculated volume of a 20 mM 15 N-NO₃⁻ solution (99 % atom 15N, Sigma-Aldrich) was then introduced into the overlying water of each core. The added volume was designed to achieve a nominal concentration equivalent to either 50% or 100% of the ambient concentration, with two sets of three cores. A 3 h pre-incubation period was implemented to ensure sufficient diffusion of 15N-NO₃⁻ to the sediment nitrate reduction zone (Nielsen, 1992), including shallow rhizosphere and bioturbated layers. During this period, an air bubbling system was used to keep the overlying bottom-water homogeneously saturated with air, maintaining the in situ conditions. Following the pre-incubation, water samples were collected to determine the exact concentration of 15N-NO₃⁻ and the initial 29N₂ and 30N₂ concentrations. Then, the incubations were performed as described above. Discrete water samples were collected *using 12 mL Exetainers, poisoned using ZnCl₂ and analysed within a week for 29N₂ and 30N₂ measurements.* 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 ZnCl₂ were added for further 15N-NH₄⁺ analysis to assess DNRA rates.”

R1-37: L. 214: please give a reason to the replacement with new seawater

AC-R1-37: This has been done. See AC-R1-36 for the edited text

R1-38: L. 224: 29N₂ and 30N₂ (not N)

AC-R1-38: This has been done. See AC-R1-36 for the edited text

R1-39: L. 231: please define “NO_x”

AC-R1-39: This has been done

R1-40: L. 234: “The isotope ²⁸N₂, ²⁹N₂ and ³⁰N₂ measurements were completed within one week of collection”, in line 224 you say that samples were directly analysed. If samples were in fact not immediately analysed, how did you conserve them, i.e. did you poison them?

AC-R1-40: Analyses were performed within one week of collection and samples were preserved using ZnCl₂. This has been corrected. See AC-R1-36 for the edited text

R1-41: L. 235: maybe combine the two sentences

AC-R1-41: This has been done

R1-42: L. 239, 240: ¹⁵N-NH₄ and NH₄

AC-R1-42: This has been corrected

R1-43: L. 248 ff: please add that you also tested the linear increase of D15 with increasing tracer concentration, which also is a test for the validity of IPT (relates to the assumption of necessary nitrate-limitation in the sediment). Further, please rearrange the sentences regarding anammox and DNRA. DNRA has nothing to do with the original IPT or its tests, whereas the presence /absence of anammox is an outcome of testing the assumption of tracer-independence of D14. Equations 3-6 are in my opinion not necessary, as IPT is well established and those equations can be found in Nielsen 1992.

AC-R1-43: We now mention that we tested the linear increase of D15 with the amount of added ¹⁵N-NO₃, we have rearranged the sentences regarding anammox and DNRA and now follow the logic on IPT validation, implication for anammox and the independent test for DNRA. Since IPT is well established and to reduce manuscript length, we have removed equations 3-6 as suggested by the reviewer.

L277-285:

“The validity of the original isotope pairing technique (Nielsen, 1992) was tested by checking the absence of increase of D14 with the amount of added $^{15}\text{N-NO}_3^-$ as well as the linear increase of the D15 with the amount of added $^{15}\text{N-NO}_3^-$ (Risgaard-Petersen et al., 2003). The tracer addition experiment provides no evidence of a substantial anammox contribution. DNRA was assessed independently from $^{15}\text{N-NH}_4^+$ production in the terminal sediment slurry. Since no $^{15}\text{N-NH}_4^+$ production was detected DNRA was considered negligible. Both processes are therefore not considered further in the manuscript and denitrification rates, including the contributions supported by NO_3^- from the overlying water (Dw) and by NO_3^- produced through sedimentary nitrification (Dn), were computed on the six sediment cores following Nielsen (1992).”

R1-44: L. 300: if you wanted to make this chapter a bit stronger you could add a plot to the appendix, showing the “consistencies” between the parameters

AC-R1-44: This has been done.

R1-45: L. 324: “surficial” sediments commonly termed “surface” sediments

AC-R1-45: This has been corrected

R1-46: L. 325: Corg and TN were already abbreviated earlier in the text

AC-R1-46: This has been corrected

R1-47: L. 335: “Oxygen in the sediment column” I guess this relates to porewater O_2 measured with microprofiles; maybe reformulate a bit, its dissolved O_2 in porewater. Further, OPD and DOU were abbreviated already earlier.

AC-R1-47: “Oxygen in the sediment column” has been replaced by “Vertical porewater O_2 concentrations” for consistency with the terms used in the Methods section. Definition of DOU and OPD were removed.

R1-48: L. 360: maybe consider changing “discriminated” to “separated / sorted” or similar

AC-R1-48: “discriminated” has been replaced by “separated”.

R1-49: L. 403 ff: a bit repetitive to the introduction

AC-R1-49: We agree that this paragraph repeated background information already presented in the Introduction. We have therefore removed it

R1-50: L. 407: influence “of”

AC-R1-50: This part of the manuscript has been removed

R1-51: L. 424: “consumption by the sediment”, not the sediment consumes NO₃⁻ but microbial processes, e.g. denitrification; please reformulate

AC-R1-51: We agree and have reformulated this passage to specify that the decrease in porewater nitrate concentrations indicates net nitrate consumption by microbial processes within the sediment.

L464-466:

“The decrease in porewater NO₃⁻ concentration with depth in all sites indicates a consumption by microbial processes within the sediment (Fig. 2d), consistent with our measured NO₃⁻ benthic fluxes (Fig. 6).”

R1-52: L. 430: “The absence of NO₃ peak co-located with the Feasc peak suggests a tight coupling between nitrification in the rhizosphere area.” This seems contradictory to the previous discussion. If the Feasc peak should be in the deeper rhizosphere zone and also nitrification can be found there due to oxygenation around the rhizons, shouldn’t the peaks co-locate?

AC-R1-52: We agree that nitrification is expected to occur in the oxygenated rhizosphere zone associated with the Feasc peak. Our intention was not to suggest the absence of nitrification, but rather that nitrate produced in this zone is likely consumed rapidly through coupled nitrification-denitrification in adjacent anoxic sediment. As a result, NO₃⁻ does not accumulate sufficiently to form a detectable concentration peak. We have revised the text to clarify this point.

L471-474:

“Although nitrification likely occurs within this oxygenated rhizosphere zone, the absence of a corresponding accumulation of NO₃⁻ at the depth of maximum Feasc suggests that newly produced NO₃⁻ is rapidly consumed by denitrification in adjacent anoxic sediment, preventing its accumulation in porewater.”

R1-53: L. 441: I am not sure “resilient” is the right word here

AC-R1-53: We agree and have removed the term “resilient” The sentence has been revised to:

L483-485:

“Despite these changes in sedimentary conditions, denitrification rates were not substantially altered, suggesting that distinct sediment biogeochemical conditions can sustain similar nitrogen removal rates.”

R1-54: L. 476, 482: “low organic carbon availability compared to impacted sites, which limits local nitrification fuelling denitrification” and “prevents nitrification despite the high availability of organic carbon, effectively limiting denitrification”: nitrification is commonly an autotrophic process and thus independent of OC availability; denitrification, however, being commonly heterotrophic in coastal sediments, needs OC as its electron donor. Please reformulate

AC-R1-55: We agree and have revised both sentences to avoid implying that nitrification is directly limited by organic carbon availability.

L520-523:

“The low denitrification rates at US likely reflect the relatively low organic carbon availability compared to impacted sites, which limits ammonium regeneration through organic matter mineralisation and consequently limits nitrate production through nitrification, thereby reducing coupled nitrification-denitrification.”

L528-531:

“In the absence of dense bioturbators, the high organic carbon content at LBWN promotes reducing conditions at LBWN (Fig. 2d and e) that limit oxygen availability and thereby nitrification, ultimately restricting the nitrate supply available for denitrification.”

R1-55: L. 489: “seascape-scale nitrogen-removal capacity”, please edit (do you mean: N removal capacity at the scale of the seascape?)

AC-R1-55: This has been corrected

L537-538:

“This reduces the nitrogen-removal capacity at the scale of the seascape, even if unit-area denitrification rates remain unaffected (O’Meara et al., 2015).”

R1-56: L. 499: flux rates of what?

AC-R1-56: Details have been added

L547-548:

“From a coastal-management perspective, one of the main issues is whether areal benthic fluxes and denitrification rates measured in LBWS can offset, at the seascape level, the functional loss caused by salt marsh vegetation removal.”

R1-57: L. 501 ff: it’s quite a stretch to assume that 6 flux cores capture a whole area’s heterogeneity; maybe add a disclaimer, like “first-order estimate.

AC-R1-57: A disclaimer has been added

L550-551:

“As a first-order estimate, and 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.”