

Review

RC1: '[Comment on egusphere-2026-991](#)', Nezha Mejjad, 20 Apr 2026

General Comments

The study provides valuable insights into understudied Nordic Blue Carbon processes. However, certain methodological choices regarding soil acidification require further justification, and the flow of the results could be improved for better clarity.

We would like to thank the reviewer for the comments to improve the quality of the manuscript. We have now further justified the methods and rewritten part of the text to improve clarity.

Abstract

Structure and Flow: I suggest restructuring lines 21–27 to first present the calculated carbon stocks, followed by the biotic and abiotic factors that influenced them. This "results-first" approach allows the reader to grasp the magnitude of the findings before examining into the drivers.

The abstract has been restructured as suggested, with soil OC stocks values presented first.

Methods

Quantification Formulas: Please include the specific equations used to calculate the aboveground, belowground, and soil organic carbon (SOC) stocks to ensure full transparency.

We have now included the explicit equations used to calculate aboveground, belowground, and soil organic carbon (OC) stocks in the Methods-section to improve clarity and reproducibility. For above- and belowground biomass, OC stocks were calculated by multiplying biomass by its corresponding OC content. For soil, OC stocks were calculated from TOC content, dry bulk density, and slice thickness.

Acidification Protocol: The authors report using 1.2 N HCl to remove carbonates, citing Kennedy et al. (2005). This concentration is relatively high; typical salt marsh protocols often favor more dilute solutions (e.g., 1–4% or ~0.1–0.5 N) to minimize the risk of hydrolyzing or leaching labile organic carbon.

Please clarify if this concentration was an intentional adaptation and provide a justification for its use over more dilute alternatives. Also provide additional details on the acidification procedure, including the volume of acid added, total reaction time, and drying protocols.

We acknowledge the reviewer's concern. We clarified the concentration of HCl used, which was 3 mol L⁻¹. This concentration was selected to ensure complete removal of carbonates in our samples, which may contain variable carbonate contents. The amount added to each sample was very small (50 µL), and the treatment was then not aggressive. We have now rewritten, in agreement with the research engineer who assisted during sample processing, the protocol and clarified in the Methods section, including the volume and concentration of acid added, the reaction time, and drying procedure after acidification. These details have been added to improve reproducibility and transparency.

Addition in the text: " Simultaneously, soil samples were acidified using 3 mol L⁻¹ HCl (10%) to remove inorganic carbon. Approximately 50 µL of acid was added to each sample and put in dry oven at 60°C during 2h. Thereafter, 50 µL of acid was added again, and samples were then dried

at 60°C over night before analysis. This concentration was selected to ensure complete carbonate removal in sediments with potentially variable carbonate content."

Discussion & Analysis

Grazer Specificity: Does the specific type of livestock (e.g., sheep vs. cattle) impact carbon storage differently? I suggest the authors expand on whether there is a known relationship between grazer identity, specific plant traits (beyond just Phragmites reduction), and the ecosystem's overall carbon capture capacity.

Thank you for this comment. We agree that grazer identity (e.g., cattle, sheep, horses) can influence vegetation structure, plant traits, and soil processes, and thereby affect carbon storage. In our study, grazer type and grazing intensity were recorded (Table 1). However, grazer type was not included as an explanatory variable because its effect could not be disentangled within our study design due to limited replication across grazer types. We have now clarified this point in the Discussion and acknowledge that grazer-specific effects may contribute to the context-dependent grazing impacts observed.

Citation: <https://doi.org/10.5194/egusphere-2026-991-RC1>

RC2: '[Comment on egusphere-2026-991](#)', Scott Jones, 07 May

General Comments

This manuscript presents biomass and soil carbon data from coastal wetlands across the Nordic region. The authors quantify carbon stocks and associated plant communities/environmental conditions at 12 areas, with paired grazed/ungrazed sites. They present a regional summary of carbon stocks for an understudied region that will be of interest to blue carbon folks, and give some discussion on potential drivers of those stocks. The sites vary along environmental gradients but in a systematic fashion that makes disentangling drivers seem like a stretch given the data collected. The title and abstract therefore may over-promise an elucidation of drivers, when there is discussion but not much in the way of strong conclusions. The work is still impressive and highly valuable, but perhaps as a beginning to regional understanding and a large contribution to global comparisons, more than for specific understanding of ecological drivers. Specific comments below.

Thank you for this general comment. We agree that it is difficult to disentangle the effects of the different environmental drivers since they are highly correlated in the studied large-scale gradient. We have therefore revised the text throughout the paper to better reflect this. See detailed replies to the specific comments below.

Specific Comments

-Difficult to interpret drivers when they are confounded (e.g., L270, L527). Please clarify how this could be addressed in the future (or current analysis if possible) for a better understanding of which drivers are important for C stocks in the region. Seems that current site distribution doesn't allow disentangling of variables.

Thank you for this important comment. We acknowledge that most of the environmental drivers co-vary across the study region (e.g., salinity, temperature, land uplift, and tidal influence),

which limits our ability to disentangle their individual effects on carbon stocks. This is an inherent constraint of large-scale observational studies conducted along natural environmental gradients, such as the Baltic–North Sea transition.

To acknowledge this, we have rephrased how we describe the study, from “evaluating abiotic factors” to “testing effects of management and soil characteristics in a large-scale gradient of environmental factors”. This is done throughout the text, including abstract, introduction, result description and discussion.

In the discussion, we now also bring up that future studies should aim to sample sites where environmental variables are contrasting and/or vary more independently from one another (e.g., salinity, temperature, and tidal influence), in order to better disentangle their respective effects on carbon stocks.

-Much of the paper focuses on plant community dynamics and responses. This is all good stuff, but it seems a little tangential to the stated focus on carbon stocks/processes. I understand that plant communities can be an important driver of stocks (biomass stocks most obviously). Section 4.1 leading the discussion felt strange given the set up of the paper focusing on carbon dynamics.

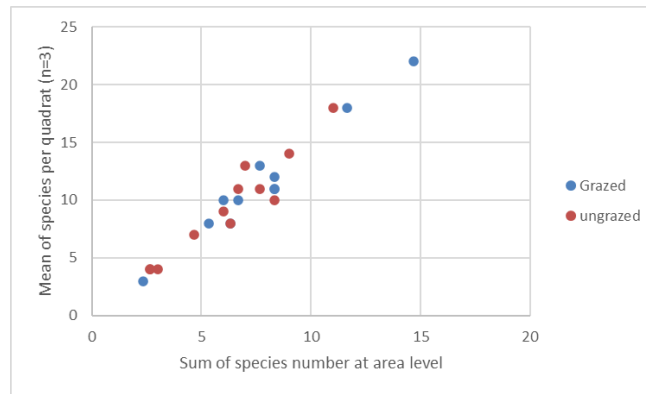
Thank you for this comment. Although the primary focus of the manuscript is on carbon stocks and their drivers, the section on plant community dynamics was included because plant communities represent a key biotic control on carbon storage, particularly through their influence on biomass production and belowground carbon inputs. We therefore considered it important to first characterize plant community structure in order to better interpret the observed patterns in carbon stocks.

To improve clarity and better align the discussion with the main focus of the study, we have revised the beginning of Section 4.1 to more explicitly link plant community patterns to carbon storage processes. This helps clarify the role of vegetation as a driver of carbon stocks rather than a separate line of investigation.

-Diversity is presented at quadrat scale as I understand. Given there are several quadrats at each site, have authors also calculated site-scale diversity? Soil carbon is likely controlled at the site scale, not quadrat scale, so understanding how plant diversity is structured across quadrats within sites may be helpful (e.g., are quadrats similar or dissimilar within sites comparing grazed and ungrazed as a simple measure of turnover).

Thank you for this insightful comment. We agree that site-scale diversity may be relevant when considering controls on soil carbon. To address this point, we calculated plant diversity at the site scale (i.e. pooled across quadrats within each site) and compared it to quadrat-scale diversity. These two measures were found to be strongly correlated (see the figure below), indicating that quadrat-scale diversity provides a reliable proxy for site-level diversity in our dataset.

Therefore, we retained quadrat-scale diversity in the analyses, as it allows direct coupling with biomass and soil measurements, while still capturing broader diversity patterns at the site level.



Addition in the text: “Plant diversity was assessed at the quadrat scale to match biomass and soil sampling. This approach was further supported by exploratory comparisons showing that diversity patterns were consistent when calculated at the site scale (i.e. pooled across quadrats), indicating that quadrat-scale diversity adequately represents site-level diversity in this dataset.”

-L76. If exposure and tidal fluctuations vary along the Baltic region, suggest including tide range/exposure metrics for all sites in site description tables.

Thank you for this suggestion. Tide amplitude was added in the description table. In addition, we added a verbal description of tidal conditions in section 2.1.

-Please clarify if grazed/ungrazed sites within each geographic area/location are otherwise similar in geomorphology/position/etc. More information on typical management of grazed wetlands for readers outside the region may be helpful (e.g., are these typically diked/hydrologically managed?). L160 for example, all sites grazed and ungrazed similar in relative tidal position? Any porewater salinity available and is porewater salinity similar comparing grazed/ungrazed within an area/location?

Thank you for this important comment. Within each geographic area, grazed and ungrazed sites were selected in close proximity and were chosen to be as similar as possible in terms of geomorphological setting and landscape position. This is now clarified in method section 2.1. Sampling was consistently conducted in the low marsh zone (approximately 20 cm above mean sea level) in both grazed and ungrazed sites. The relative tidal position of each quadrat was checked in the field to ensure that sampling occurred within the same elevation range in the grazed and ungrazed areas. We have clarified this in method section 2.2.

We did not measure porewater salinity. However, we acknowledge that it may vary locally between grazed and ungrazed sites due to differences in soil compaction and water content. Instead, mean annual sea surface salinity was used as a proxy to characterize the large-scale salinity gradient across sites, which is a major driver of environmental conditions in Nordic coastal marshes.

Regarding management, the studied grazed wetlands are not diked or artificially drained and remain under natural tidal influence. Grazing is the primary management practice, typically involving cattle, sheep, or horses, with no additional hydrological regulation. This is now clarified in section 2.1.

-Largest differences b/w grazed and ungrazed in plant communities and sediment properties seem to be from sites in close proximity (Tullgarn N, Tullgarn S, Naset). Ungrazed lower/wetter? Please clarify/discuss why this may be and if this region is 'driving' some of the pooled differences in the overall dataset.

Thank you for this observation. We acknowledge that some pronounced differences between grazed and ungrazed sites are observed in geographically close areas such as Tullgarn North, Tullgarn South, and Naset. However, similar patterns are also observed across other sites, and the overall results are not driven by a single region. These sites are characterized by relatively sheltered conditions and finer sediments, which may enhance the sensitivity of plant communities to grazing by reducing physical constraints and increasing the role of biotic interactions.

-Section 2.3.4. goes far beyond data analysis. Suggest breaking off plant community analyses as separate section for clarity.

Thank you for this helpful suggestion. The section has now been divided into two separate subsections to improve clarity and readability: one focusing on plant community analyses and the other on organic carbon stocks and statistical analyses.

-L246. Please include a more detailed justification for why these specific variables were included to explain variability, and not additional variables. Same for L276.

Thank you for this comment. The selection of explanatory variables for both the NMDS interpretation and the mixed-effects models was guided by a combination of ecological relevance and statistical considerations.

For the NMDS analysis, the variables included (salinity, temperature, latitude, longitude, land uplift, and key soil properties) were chosen because they represent the main large-scale environmental gradients and local soil conditions known to influence plant community composition in coastal marshes. These variables capture both regional drivers (e.g., sea salinity, which is also correlated with tidal amplitude, land-uplift, and water temperature across the Baltic–North Sea transition) and local abiotic conditions (e.g., soil moisture and sediment characteristics).

For the mixed-effects models, fixed factors were selected based on prior ecological knowledge and Spearman rank correlations to avoid multicollinearity among predictors. Only variables representing key, non-redundant drivers of carbon stocks were retained, including management type, plant species richness, salinity (as a proxy of the large-scale environmental gradient), and median grain size (as an integrative descriptor of sediment properties). Additional variables were excluded when they were strongly correlated with these predictors or did not improve model performance.

We have clarified this rationale in the Methods section to improve transparency in variable selection.

-L263. If one site and need to interpret cautiously, maybe better to just not interpret that site?

Thank you for this important comment. We agree that including a salinity category represented by a single site may limit the interpretation of salinity and its interaction with management in the PERMANOVA analyses.

To address this, we reran the PERMANOVA analyses excluding the euhaline site. The results were consistent with those obtained using the full dataset, indicating that the observed effects were not driven by this single site. This has now been clarified in the manuscript, and the interpretation of salinity effects has been strengthened accordingly.

-Please justify the use of SEM given the data density here.

Thank you for this comment. The use of Structural Equation Modelling (SEM) was motivated by our aim to disentangle direct and indirect effects of biotic and abiotic drivers on carbon stocks across ecosystem compartments, which cannot be fully addressed using univariate approaches alone. To ensure that the SEM was appropriate given the data structure, we used a piecewise SEM framework based on linear mixed-effects models, which is well suited for datasets of moderate size and allows the inclusion of random effects (here, site). Model complexity was limited by including only key variables identified through prior ecological knowledge and model selection, thereby avoiding overparameterization.

Furthermore, the final model showed a good fit to the data (Fisher's C = 9.086, $p = 0.696$), supporting the adequacy of the model structure relative to the available data.

-Please clarify the use of PERMANOVA among these groups when it appears that dispersion varies systematically between treatments.

Thank you for this important comment. We acknowledge that PERMANOVA can be sensitive to differences in within-group dispersion. To address this, we tested homogeneity of multivariate dispersion using *permutest* (Anderson, 2006) alongside PERMANOVA analyses, as already described in the Methods. These tests indicated some variation in dispersion among groups, which is now explicitly acknowledged in the manuscript. We have changed the description of PERMANOVA results to reflect both potential differences in group centroids and differences in within-group variability. Overall, the interpretation is supported by the NMDS ordination patterns. We have adjusted the text to reflect these nuances.

-Figure 5. Suggest graying out and not including estimate for non-significant paths.

We thank the reviewer for this suggestion. We have considered this point carefully. However, we decided to retain non-significant paths in the SEM figure, as they provide important information on the hypothesized model structure. In addition, significant and non-significant paths are already visually distinguished using solid and dashed lines, respectively.

-L478. Distributed how? Please be specific.

We have clarified that these sites are distributed across the large-scale salinity and biogeographical gradient, spanning low-salinity Baltic sites to more marine-influenced northern sites.

-L506. Slightly higher? Was that supported by analysis?

It has now been clarified in the text.

-L518. Seemed to? Please be cautious in discussing results given support (or lack of) from analyses.

It has been rephrased to better reflect the level of statistical support and to avoid over-interpretation.

-L556. Don't think it's necessary to include 'but remain slightly low in a global context'
We agree with that comment, and we have now removed this part of the sentence.

Citation: <https://doi.org/10.5194/egusphere-2026-991-RC2>