Response to Reviewers

*Manuscript: "**Temperature driven coastal processes and their far reaching effects on deep Baltic Sea biogeochemical dynamics**" by Mallissery et al.*

We thank the reviewer for the careful reading of the manuscript and for the constructive comments and suggestions. Below, each reviewer comment is reproduced in black, our response is provided in blue, and the corresponding changes made to the manuscript are shown in red.

Title

Comment: Please consider removing "far reaching" from the title. The wording is vague and open to interpretation (e.g., does the effect reach outside the Baltic, how far is far?).

Response: We have removed the term "far-reaching" as suggested

Changes to manuscript: the term "far-reaching" is removed from the title

Abstract

Line 2

Comment: The term "coastal filter" would benefit from clarification. It is often interpreted primarily as coastal nutrient retention, which may not fully reflect how the concept is applied in this study. Consider briefly explaining what is meant by the term in this context, or alternatively, focus more directly on the role and importance of the coastal zone and why it is critical for basin-scale biogeochemistry.

Response: In this study, we use the term in a broader biogeochemical sense, referring to the coastal nutrient (P and N) retention. We have revised the abstract and introduction to clarify this definition.

Changes to manuscript: The sentence in the abstract changed from

"The coastal zone of the Baltic Sea plays a critical role in shaping the biogeochemistry of the deep ocean mainly through the coastal filter"

to

"The coastal zone of the Baltic Sea plays a critical role in shaping the biogeochemistry of the deep ocean mainly through the coastal filter, the processes that retain, transform and remove nutrients and organic matter along the land–sea continuum."

Line 4

Comment: The model name MOM-ERGOM is introduced without explanation. Since abbreviations should generally be avoided in the abstract, consider writing out the full model name or rephrasing the sentence to avoid using the abbreviation.

Response: Thank you for pointing out

Changes to manuscript: The sentence changed from "In this study, we investigated the role of temperature driven biogeochemical processes in the sediment and water column in defining the biogeochemistry of the nine distinct basins of the Baltic Sea using a coupled physical biogeochemical model MOM-ERGOM"

to

"In this study, we investigated the role of temperature driven biogeochemical processes in the sediment and water column in defining the biogeochemistry of the nine distinct basins of the Baltic Sea using a coupled physical biogeochemical model."

Line 7

Comment: Please spell out and briefly define the Q10 temperature coefficient rather than using only the symbol. A short clarification would improve accessibility for readers who may not be familiar with this parameter.

Response: We agree that the Q10 parameter should be briefly defined in the abstract for readers unfamiliar with this temperature sensitivity formulation. We have revised the sentence to introduce the Q10 temperature coefficient and explain

Changes to manuscript: The sentence changed from " In ERGOM, the temperature driven biogeochemical processes are represented by the q10 parameterization, which is uniform in space and time and neglects that temperature sensitivities may differ with depth."

to

"In the model, temperature-driven biogeochemical processes are represented by the q10 temperature parameterization, which describes the temperature sensitivity of process rates as the factor by which a process rate changes for a 10 °C increase in temperature "

Line 12–14

Comment: This appears to be a key finding and would benefit from clearer wording. I suggest rephrasing for precision and impact. For example: "Our results suggest that, under a warming climate, enhanced temperature-driven remineralization in both sediments and the water column may substantially increase nutrient availability, with the potential to stimulate primary productivity." Ensuring the causal link and magnitude of the effect are clearly expressed will strengthen this important conclusion.

Response: We have revised the sentence to improve clarity and emphasize the relationship between temperature-driven remineralization and ecosystem responses under future warming conditions.

Changes to manuscript: The sentence "The rising nutrients from enhanced temperature driven remineralization indicate potential for higher primary productivity under a future warmer climate" deleted from the manuscript and a modified sentence added as "Our study reveals that, under a warmer climate, enhanced temperature-driven remineralization in both the sediments and the water column increases nutrient availability, with a potential to promote higher primary productivity. "

Line 17–18

Comment: The term "disproportionately" requires clarification. Disproportionate relative to what — the magnitude of the imposed temperature change, a linear expectation, or previous studies? Please specify the reference point.

More generally, I encourage the authors to carefully review their use of the term "sensitivity" throughout the manuscript. It is not always clear what "sensitive" refers to — sensitivity relative to which baseline, perturbation, or comparison? In some cases, "strong response" or "high impact" may be more appropriate if that is what is meant. A clearer and more consistent definition of "sensitivity" would improve interpretability. I have highlighted several instances, though likely not all.

Response: In this study, the term "disproportionately" is used to describe the fact that the coastal ocean exerts an influence on deep Baltic Sea (the 9 boxes in Figure 2) biogeochemistry that is disproportionate to its spatial extent.

Changes to manuscript: The sentence in the manuscript changed from

"The analysis shows that the nitrate cycle is disproportionately sensitive to coastal sediment recycling, with the Bothnian Bay displaying two to four fold stronger responses in nutrient cycles than basin wide perturbations, underscoring the disproportionate influence of coastal processes on basin biogeochemistry"

to

"The analysis shows that the nitrate cycle is highly sensitive to coastal sediment recycling, with the Bothnian Bay displaying two- to four-fold higher coastal sensitivity than expected from the relative spatial extent of the coastal zone, underscoring the high influence of coastal processes on basin biogeochemistry."

In addition, throughout the manuscript, the term "sensitivity" is used with caution. A clear definition of sensitivity is provided in Section 2.2.1.

Line 20

Comment: The phrase “dynamics depend on the spatial scope of sediment recycling” is unclear and would benefit from clarification. Please specify what is meant by “spatial scope” (e.g., coastal-only versus basin-wide enhancement). Similarly, the phrase “when enhanced” should explicitly state what is being enhanced (e.g., temperature sensitivity, remineralization rates) to avoid ambiguity.

Response: We agree that the terms “spatial scope” and “when enhanced” were not sufficiently clear. We have revised the text to explicitly clarify the term “spatial scope” . We have also specified that the “enhancement” refers to increased sediment recycling in the coast associated with the modified temperature sensitivity (q10 parameter)

Changes to manuscript: The sentences changed from “In the Bothnian Bay, phosphate dynamics depend on the spatial scope of sediment recycling. When enhanced only in coastal sediments, oxic conditions trap much of the released phosphate as iron–phosphate, strengthening the coastal filter and limiting export to the deep basin”

to

“In the Bothinia Bay, phosphate dynamics differ depending on whether the temperature sensitivity of sediment recycling is enhanced only in coastal sediments or throughout the basin. Under coastal-only sediment recycling enhancement, oxic conditins retain much of the released phosphate as iron-bound phosphate, strengthening the coastal filter and limiting phosphate export to the deep basin”

Line 23–24

Comment: A comparison with empirically derived temperature dependencies from laboratory or in situ studies would strengthen the interpretation of the results. If sufficient observational constraints are not available, this limitation should be clearly acknowledged, and it should be stated explicitly that the temperature perturbations applied here are experimental and intended to explore system behavior rather than to represent directly validated future scenarios.

Response: Thank you for this comment. We agree that the original wording could be interpreted as implying that the applied temperature perturbations represent future climate projections. To avoid this confusion, we have retained the statement on the importance of resolving coastal processes but added a clarification that the temperature perturbations used in this study are idealized sensitivity experiments designed to investigate system behavior, rather than direct projections of future climate conditions.

Changes to manuscript: A closing sentence was added to the Abstract: “The applied temperature perturbations in this study should be interpreted as experimental sensitivity scenarios designed to investigate system behavior, rather than as direct projections of future climate conditions.”

Introduction

Line 26–27

Comment: Remove “(97%….)” and replace “It” by “and acts as”.

Response: We have implemented both changes suggested by the reviewer.

Changes to manuscript: The sentence has been revised to remove the parenthetical phrase "(9.7% of the global ocean by area)" and to improve the grammatical flow by replacing "It acts as" by "and act as."

Line 29

Comment: Climate change is largely driven by human activities, and this should be reflected more clearly in the wording. Please rephrase accordingly.

In addition, the statement regarding eutrophication and hypoxia in the Baltic Sea would benefit from a broader and more balanced set of references. There are several important studies documenting these issues both prior to Almroth-Rosell (2016) and more recent contributions. Including a more comprehensive range of literature would strengthen the background section.

Response: We have revised the sentence to clarify that climate change is one of several anthropogenic pressures affecting coastal ecosystems, together with nutrient loading. In addition, we have expanded the references supporting the statement on eutrophication and hypoxia by including widely cited studies.

Changes to manuscript: The sentence changed from "The coastal ecosystems are highly affected by climate change and human activities (Steffen et al., 2015) and suffering from eutrophication and hypoxia (Almroth-Rosell et al., 2016, Wåhlström et., 2024, Kulinski et al., 2022). "

to

"Coastal ecosystems are highly affected by anthropogenic pressures including climate change and nutrient loading Steffen et al., 2015) and suffering from eutrophication and hypoxia (Almroth-Rosell et al., 2016, Conley et al., 2009; Carstensen et al., 2014)."

The references added are as follows.

1. Conley, D. J., Bjo¨rck, S., Bonsdorff, E., Carstensen, J., Destouni, G., Gustafsson, B. G., Hietanen, S., Kortekaas, M., Kuosa, H.,Markus Meier, H., et al.: Hypoxia-related processes in the Baltic Sea, Environmental science & technology, 43, 3412–3420, 2009.

2. Carstensen, J., Andersen, J. H., Gustafsson, B. G., and Conley, D. J.: Deoxygenation of the Baltic Sea during the last century, Proceedings of the National Academy of Sciences, 111, 5628–5633, 2014.

3. Wåhlström, I., Almroth-Rosell, E., Edman, M., Olofsson, M., Eilola, K., Fleming, V., ... & Meier, H. M. (2024). Increased nutrient retention and cyanobacterial blooms in a future coastal zone. *Estuarine, Coastal and Shelf Science*, *301*, 108728.

4. Kuliński, K., Rehder, G., Asmala, E., Bartosova, A., Carstensen, J., Gustafsson, B., ... & Undeman, E. (2022). Biogeochemical functioning of the Baltic Sea. *Earth System Dynamics*, *13*(1), 633-685.

Line 30

Comment: The phrase "Despite its smaller area" is not necessary in this context and can be removed without loss of clarity.

Response: Thank you for the comment.

Changes to manuscript: "Despite its smaller area" has been removed; the sentence now begins "The coastal ocean contributes significantly to the global marine productivity…"

Lines 26–34

Comment: This section could be substantially shortened or potentially removed. While briefly placing the study in a broader context can be valuable, the global ocean is not the primary focus of this manuscript. For example, carbon cycling is mentioned, yet it is not assessed in the study. I suggest either: (1) Condensing this part to briefly frame the Baltic Sea within a broader context and then expanding the discussion section to explicitly link the study's findings to relevant global-scale processes, or (2) Removing the broader global framing altogether and focusing the introduction more directly and concisely on the Baltic Sea. A clearer alignment between the introduction and the actual scope of the study would improve focus and coherence.

Response: Thank you for the useful comment. We agree that the initial paragraph focused too much on global coastal regions. We have therefore revised the paragraph to focus more specifically on the Baltic Sea coastal regions and their importance for the biogeochemical dynamics of the deeper basins, which directly links to the scope of our study.

Changes to manuscript: The entire paragraph rewritten as follows

"Coastal oceans form a link between the land and the open sea, play an essential role in marine biogeochemical cycles (Holt et al., 2017). In the Baltic Sea, this land–sea interface is particularly important because coastal regions receive substantial terrestrial and anthropogenic nutrient inputs and are strongly affected by eutrophication, hypoxia, and ongoing climate change (Conley et al., 2009; Carstensen et al., 2014a; Almroth-Rosell et al., 2016; Meier et al., 2012). Baltic coastal environments are highly heterogeneous, with strong spatial differences in hydrography (Placke et al., 2018), oxygen availability (Gustafsson et al., 2017), sediment characteristics (Carstensen et al., 2020), and nutrient inputs (Liu et al., 2017). These conditions promote intense benthic–pelagic coupling and rapid nutrient transformation, allowing coastal sediments and water-column processes to retain, recycle, or transform nutrients before they are

transported to adjacent offshore waters (Asmala et al., 2017; Voss et al., 2005; Almroth-Rosell et al., 2011). Thus, although coastal regions occupy a limited fraction of the Baltic Sea, their biogeochemical processes can influence nutrient dynamics beyond the shallow coastal zone and potentially affect the biogeochemical state of deeper Baltic Sea basins"

Line 39

Comment: Please rephrase "It helps" by for e.g., "This process mitigates".

Response: We have revised the sentence as follows

Changes to manuscript: The sentence changed from "One of the key ecosystem functions of the coastal ocean is the coastal filter, a biogeochemical buffer encompassing processes that retain, remove, and transform land derived nutrients on the way to the open ocean (Asmala et al., 2017). It helps to mitigate eutrophication and reduce nutrient export to the open ocean"

to

"One of the key ecosystem functions of the coastal ocean is the coastal filter, a biogeochemical buffer encompassing processes that retain, remove, and transform land derived nutrients on the way to the open ocean (Asmala et al., 2017). This coastal filter processes mitigate eutrophication and reduce nutrient export to the open ocean"

Line 51

Comment: Please consider rephrasing "crudely neglected," as the expression carries a somewhat negative connotation. There are valid reasons why models simplify or omit certain coastal processes, most notably the limited availability of observational data needed for robust parameterization, as well as spatial resolution constraints and computational limitations. A more neutral formulation would be appropriate.

Response: We agree that the term "crudely neglected" could convey an unintended negative connotation. We have revised the wording to a more neutral formulation, emphasizing that coastal processes are often simplified and insufficiently resolved in BGCMs due to the challenges associated with representing their complex dynamics and spatio-temporal heterogeneity.

Changes to manuscript: "crudely neglected" has been replaced with "often simplified and insufficiently resolved": "The coastal oceans are often simplified and insufficiently resolved in BGCMs…"

Line 55

Comment: Please revise "such as as q10 temperature coefficient" to e.g., "such as a temperature coefficient (commonly referred to as Q10)."

Additionally, it would be helpful to clarify whether this parameter is universally referred to as Q10 across models or whether this terminology is specific to your model formulation. Given that this parameter is central to the manuscript, it should be introduced thoroughly and clearly at first mention, including a brief explanation of its role and an appropriate reference describing its origin and theoretical basis.

Response: We have revised the description of the q10 parameter to clarify its terminology and role in biogeochemical models. Specifically, we replaced the previous wording with "temperature coefficient (commonly referred to as q10)" and added an explanation that q10 represents the factor by which process rates change for a 10C temperature increase. We have also added relevant references describing the origin and application of q10 formulations in ecosystem models.

Changes to manuscript: The sentences changed from "A common simplification in models is the use of constant process parameters, such as q10 temperature coefficient, remineralization rate, although biogeo-chemical processes can vary significantly between the coast and the offshore (McGuire et al., 2001; Ward et al., 2020). "

to

"A common simplification in models is the use of spatially uniform process parameters, such as temperature coefficients (commonly referred to as q10) and remineralization rates, although biogeochemical processes can vary significantly between the coast and the offshore regions (McGuire et al., 2001; Ward et al., 2020). The q10 parameter is an empirical formulation widely used in ecosystem models to represent the factor by which a process rate changes for a 10C increase in temperature. It is commonly applied to temperature-dependent processes, such as organic matter remineralization and biological growth rates (Aumont et al., 2015; Laufkötter et al., 2017; Segschneider and Bendtsen, 2013; Eppley, 1972)."

The references added

1. Aumont, O., Éthé, C., Tagliabue, A., Bopp, L., and Gehlen, M.: PISCES-v2: an ocean biogeochemical model for carbon and ecosystem studies, Geoscientific Model Development Discussions, 8, 1375–1509, 2015.

2. Laufkötter, C., John, J. G., Stock, C. A., and Dunne, J. P.: Temperature and oxygen dependence of the remineralization of organic matter,Global Biogeochemical Cycles, 31, 1038–1050, 2017.

3.Segschneider, J. and Bendtsen, J.: Temperature-dependent remineralization in a warming ocean increases surface pCO2 through changes in marine ecosystem composition, Global Biogeochemical Cycles, 27, 1214–1225, 2013.

4. Eppley: Temperature and phytoplankton growth, Fishery bulletin, 70, 1063–1085, 1972.

Line 78

Comment: The term "environmental conditions" is rather broad. Please specify which conditions are meant (e.g., sediment type, water depth, redox state, stratification, basin morphology, slope/steepness, etc.). Providing concrete examples would improve clarity. A stronger linkage between these environmental factors and the model results, supported by relevant references, would also strengthen the discussion section.

Response: We agree that the term "environmental conditions" was too broad. We have revised the text by specifying relevant environmental factors, including temperature, redox conditions, and sediment characteristics. We have also added an example explaining how variability in these factors affects remineralization and nutrient regeneration rates.

The discussion section is rewritten thorougly and relevent references added to the text

Changes to manuscript: The sentence was revised from "Moreover, many of these parameters depend on environmental conditions, which adds a layer of difficulty to deriving the parameters from the sparse observations."

to

"Moreover, many of these parameters depend on environmental conditions, such as temperature, redox conditions, sediment characteristics, which adds a layer of difficulty to deriving the parameters from sparse observations. For example, remineralization and nutrient regeneration rates can vary with temperature, oxygen availability, and sediment properties, introducing additional uncertainty in biogeochemical model parameterization.

The references added to the Discussion section to support how environmental factors affect biogeochemical process rates are as follows:

(1) Maksymowska-Brossard and Piekarek-Jankowska, 2001)

(2) (Kuli´nski et al., 2022): Added as part of revision

(3) (Jäntti et al., 2011; Jäntti and Hietanen, 2012): Added as part of the revision

(4) (Hietanen et al., 2012): Added as part of the revision

(5) (Jäntti et al., 2018): Added as part of revision

(5) (Carstensen et al., 2014b): Added as part of the revison

(6) (Carstensen et al., 2014b) : Added as part of the revision

(7) (Breitburg et al., 2018; Schmidtko et al., 2017) : Added as part of the revision

(8) (Wasmund et al., 1998; Meier et al., 2019a): Added as part of the revision

Line 88

Comment: The explanatory information about q10 (currently given in brackets) should be moved to its first occurrence in the manuscript to ensure clarity and logical flow. Alternatively, avoid mentioning it briefly in Line 55 and instead provide a complete and well-structured introduction to the parameter at this point. In either case, ensure that the parameter is introduced clearly, consistently, and with appropriate referencing.

Response: The explanatory information about q10 has been moved to its first occurrence in the manuscript to ensure a clearer and more logical introduction of the parameter.

Changes to manuscript: The sentence "(a parameter representing the temperature sensitivity of a process)" removed from the Line 88

An additional sentence added at Line 56 after "(McGuire et al., 2001; Ward et al., 2020)" as "The q10 parameter is an empirical formulation widely used in ecosystem models to represent the factor by which a process rate changes for a 10oC increase in temperature. It is commonly applied to temperature-dependent processes, such as organic matter remineralization and biological growth rates (Aumont et al., 2015; Laufkötter et al., 2017; Segschneider and Bendtsen, 2013; Eppley, 1972)."

The references added

1. Aumont, O., Éthé, C., Tagliabue, A., Bopp, L., and Gehlen, M.: PISCES-v2: an ocean biogeochemical model for carbon and ecosystem studies, Geoscientific Model Development Discussions, 8, 1375–1509, 2015.

2. Laufkötter, C., John, J. G., Stock, C. A., and Dunne, J. P.: Temperature and oxygen dependence of the remineralization of organic matter,Global Biogeochemical Cycles, 31, 1038–1050, 2017.

3.Segschneider, J. and Bendtsen, J.: Temperature-dependent remineralization in a warming ocean increases surface pCO2 through changes in marine ecosystem composition, Global Biogeochemical Cycles, 27, 1214–1225, 2013.

4. Eppley: Temperature and phytoplankton growth, Fishery bulletin, 70, 1063–1085, 1972.

Line 107

Comment: Replace all double "…., (…..))" by "(…, ...)", this is the first one, but there are more along the text.

Response: Thank you for pointing out. We have corrected all instances of double-parenthesis formatting throughout the manuscript and carefully checked the text to ensure that no remaining occurrences of "(())" are present.

Changes to manuscript: Double-parenthesis reference formatting corrected, e.g. "(ERGOM, Neumann et al. 2022)" and "(ERGOM; Neumann et al. 2022)" in the Introduction and Model description.

Model, Data and Methodology

Line 134

Comment: What does "partially" mean in this context? Please specify.

Response: The term "partially" used to highlight that the "S" cycle in the model is not a complete cycle. We have revised the sentence to clarify that the model includes sulfur processes associated with hydrogen sulphide (H_2S), rather than a complete S cycle.

Changes to manuscript: The sentence changed from "The ecosystem model used in this study is based on a Non-Redfield carbon model, the Ecological ReGional Ocean Model (ERGOM; (Neumann et al., 2022)), with elemental cycles of nitrogen, phosphorus, carbon, oxygen, and partially sulfur"

to

"The ecosystem model used in this study is based on a Non-Redfield carbon model, the Ecological ReGional Ocean Model (ERGOM; Neumann et al. 2022), with elemental cycles of nitrogen, phosphorus, carbon, oxygen, and sulfur cycling associated with hydrogen sulphide"

Line 138

Comment: This is the first occurrence of CDOM in the manuscript. Please spell out the full term at first mention and retain the abbreviation thereafter for clarity.

Response: The reviewer is right. We have not included the full form of CDOM in the revised manuscript because we decided to shorten this section and focus the model description more closely on the processes relevant to our experimental setup. Reviewer 2 also suggested shortening the Model Details section (first comment under "2 Model, Data and Methodology"), and more detailed information on the model is available in Neumann et al. (2022).

Changes to manuscript: CDOM removed from the manuscript

Line 148

Comment: This sentence could benefit from clarification. Does it mean that nutrients are assimilated by phytoplankton according to the Redfield ratio, but dissolved organic matter (DOM) is not? Please clarify to avoid ambiguity.

Response: The phytoplankton in the model remains in Redfield ratio, and under certain conditions extracellular DOM produced with flexible Redfield ratio (Neumann et al., 2022).

Changes to manuscript: The sentence rewritten from "The model maintains a Redfield elemental composition for phytoplankton with a non-Redfield elemental composition for dissolved organic matter (DOM) (Neumann et al., 2022)."

To

"The model maintains a Redfield elemental ratio for phytoplankton, whereas dissolved organic matter (DOM) produced through extracellular release has a non-Redfield elemental ratio"

Line 153

Comment: Please indicate that the equations relevant for this paper are described in Section 2.1.2. This will help guide readers to the detailed methodology.

Response: Thank you for the helpful comment.

Changes to manuscript: A pointer was added: "The parameterizations relevant to this study are described in section 2.1.2. The complete model equations, parameters, and detailed processes are given in Neumann et al. 2022."

Figure 1

Comment: It is unclear why the figure shows Equation 1 rather than Equation 2, especially since Equation 2 is the one relevant for ERGOM in this study. Additionally, more context is needed regarding the use of these equations in other models. You refer to Equation 1 as the "classical q10 concept," but it is not clear where this classical formulation originates or how widely it is applied. Please clarify the rationale for using Equation 2 in ERGOM, explain how it differs from Equation 1, and provide context on how broadly accepted these formulations are in the modeling community.

Response: We agree that the distinction between the classical Q10 formulation and the exponential q10 formulation used in ERGOM was not sufficiently clear. Figure 1 shows the general concept of temperature sensitivity using the classical Q10 formulation (widely used to represent the temperature dependence of process rates), rather than to represent the specific temperature sensitivity formulation in ERGOM. Therefore, we have retained Figure 1 for this

conceptual purpose but have clarified the distinction between the two formulations (Q10 and q10) in the revised manuscript.

The classical (Q10) formulation describes the multiplicative change in a process rate for a 10 °C increase in temperature. ERGOM instead uses an exponential formulation with (q10) as the exponential temperature coefficient. Both approaches using Q10 and q10 describe the same underlying assumption that process rates increase exponentially with temperature; however, they differ in their mathematical parameterization. And q10 = ln (Q10)/10 (This relationship is now described in the manuscript as new equation)

We have also clarified that Equation 2 represents the formulation actually used in ERGOM and provided references describing the model implementation. Thus, Figure 1 serves as conceptual background for the temperature-sensitivity framework, while Equation 2 defines its implementation in the model used in this study.

Changes to manuscript:

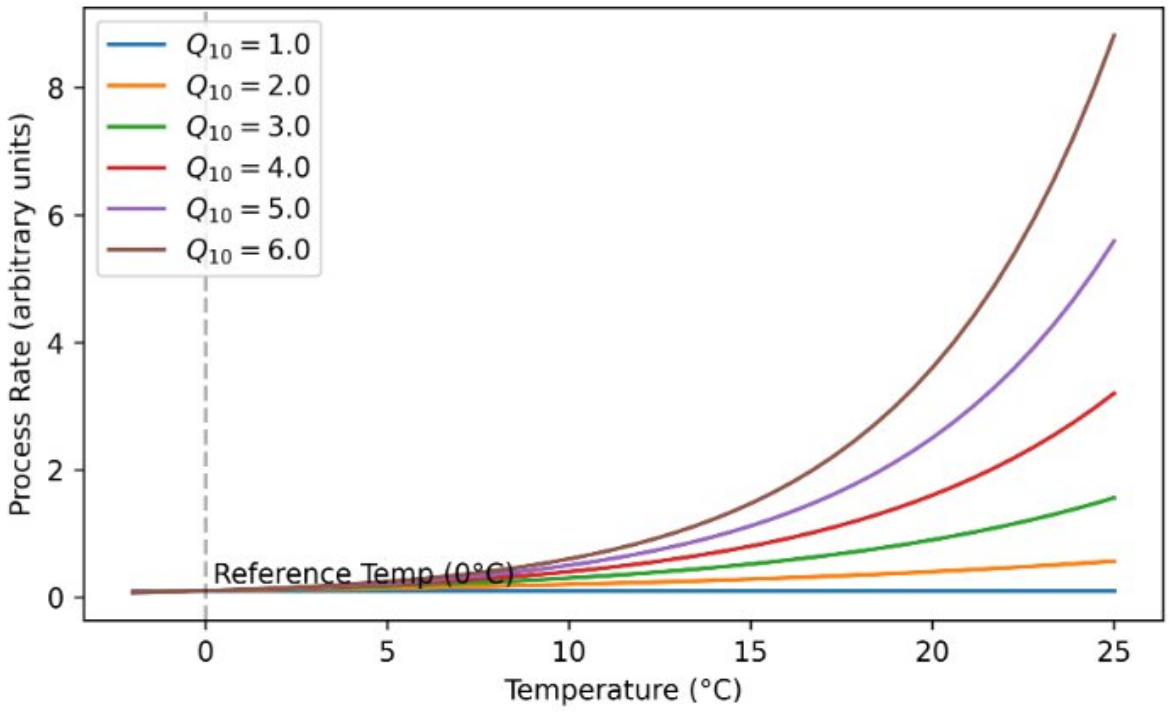
Figure 1: redrawn as follows: to include all ranges of q10 parameters used in our sensitivity analysis (Also suggested by reviewer 2, comments 4 and 5 under "2. Model Data and Methodology")

q10 for detritis recycling in the sediment = 0.175 K-1 (equvalent Q10 = 4.7569)

q10 for detritus recycling in the water column = 0.15 K-1 (equivalent Q10 = 4.0774)

q10 DOC recycling = 0.0693 K-1 (equivalent Q10 = 1.8837)

q10 Nitrification = 0.110 K-1 (equivalent Q10 = 2.9901)



The caption of Figure 1 changed to "Figure 1. Effect of Q10 parameter on the reaction rate across temperatures. The curves represent reaction rates for Q10 values of 1.0, 2.0, 3.0, 4.0, 5.0 and 6.0, showing how reaction rates increase exponentially with temperature relative to the reference temperature of 0°C, based on the classical Q10 formulation."

A new line added after “formulation (Equation 2)” in LIne 174 as “where (q10) is an exponential temperature coefficient controlling the sensitivity of the process rate to temperature changes. Both approaches describe the same underlying assumption that process rates increase exponentially with temperature; however, they differ in their mathematical parameterization.”

A new line added after “parameterization” at Line 181 as “The relationship between classical Q10 and the q10 used in ERGOM are given in equation 3. The process specific q10 values used in ERGOM are summarized in Table 1.”

Equation 3 added to the manuscript as q10 = ln (Q10)/10

Table 1

Comment: Please consider merging Table 1 with Table 2 so that all information regarding the experiments is presented together, which would improve clarity and readability.

Additionally:

(1) Table captions should be placed above the table, not below (this also applies to Table 2).

(2) Consider replacing the column heading “Parameter” with “Process” and updating the text accordingly.In reality, processes such as remineralization and nitrification represent dynamic biogeochemical processes rather than simple model parameters, so this terminology would more accurately reflect their nature. In addition, q10 itself is also a parameter so it becomes confusing at times to identify which parameter is referred to in the text.

(3) Change “Value” to “q10 Value” for clarity, if tables are merged.

Response: We have merged Table 1 and Table 2 into a single table containing all relevant information about the experiments. We have also moved the table caption above the table, revised the column heading from “Parameter” to “Process” to better represent the biogeochemical processes considered, and changed the column heading “Value” to “q10 Value” for clarity.

Changes to manuscript: Tables 1 and 2 have been merged into a single table combining the q10 default values with the experiment definitions, with a “Process” column, a “q10 value” column, and a “Perturbation” column

Table 2 (1)

Comment: Remove the brackets around the reference Neuman et al., 2022. This formatting issue appears throughout the text whenever a reference follows “in.”

Response: Thank you for pointing out this formatting issue.

Changes to manuscript: The reference formatting has been revised by removing unnecessary brackets around citations following “in” (e.g., “in Neumann et al., 2022”). The same correction has been applied consistently throughout the manuscript. In the revised manuscript Table 2 has been merged to Table 1

Table 2 (2)

Comment: I suggest renaming the experiments to make the processes more immediately clear (this is optional, but would improve readability). For example: E1_detsed, E2_detwater, E3_DOCsed, E4_waternit (instead of just E1–E4). Similarly, C1–C4 could be renamed accordingly. This would make it easier for readers to follow which processes are being referred to when reading the Results and Discussion sections, without having to constantly refer back to the table.

Response: Thank you for this helpful suggestion. We agree that more descriptive experiment names improve readability and make it easier for readers to identify the corresponding biogeochemical processes throughout the manuscript. We have therefore renamed all experiments using process-specific identifiers and updated these names consistently throughout the manuscript.

Changes to manuscript: The experiments were renamed throughout the manuscript: E1_DetSed, E2_DetWat, E3_DocSed, E4_NitWat and C1_DetSed, C2_DetWat, C3_DocSed, C4_NitWat. The new labels are used in the merged table, the text, and the figure axes through out the manuscript.

Table 2 (3)

Comment: Please clarify whether nitrification is also included in the sediments. Are E3 and C4 only applied in the water column? This should be explicitly stated to avoid ambiguity.

Response: We have clarified that the nitrification experiments considered in this study are applied only to the water column and do not include sediment nitrification.

Changes to manuscript: The process descriptions were made explicit in the merged table (Table 2), distinguishing "DOC recycling in the sediment" (E3_DocSed / C3_DocSed) and "Nitrification in the water column" (E4_NitWat / C4_NitWat).

Table 2 (4)

Comment: The choice of a 10% enhancement should be better justified in the text. You mention that the potential range could be as high as 50%, so it would be helpful to explain why 10% was selected—was this based on observations, literature, or is it an arbitrary value chosen for exploratory purposes?

Response: Thank you for this important comment. We have added an explanation that previous studies report a wider variability in q10 values (up to ±50%) and clarified that the 10% perturbation was selected as a conservative sensitivity scenario to evaluate whether relatively small changes in temperature sensitivity can influence Baltic Sea biogeochemical cycling. We also note that larger perturbations within the reported uncertainty range could potentially result in stronger ecosystem responses.

Changes to manuscript: A justification has been added to Line 216 after "selected processes." as follows

"Although previous studies have reported variability in q10 values of up to ±50% around the mean value, we selected a perturbation of 10% to assess whether relatively small changes in temperature sensitivity are already sufficient to induce notable changes in Baltic Sea biogeochemical cycling. Larger perturbations within the reported uncertainty range could potentially lead to even stronger ecosystem responses."

Line 221

Comment: It is unclear when, where, or by whom it was established that sensitivity analyses "typically involve two model simulations." I would argue that two simulations are the minimum for a basic sensitivity analysis, but in practice, the number of simulations depends on the question being tested and may often be higher. Please either rephrase this statement or provide a supporting reference or justification for the choice of two simulations.

Response: We agree that the original statement was too general. We have therefore removed this statement and revised the text to describe only the sensitivity analysis approach applied in this study, which compares a baseline simulation with a simulation using a perturbed q10 parameter.

Changes to manuscript: The entite section 2.2.1 revised to according to the suggestions of both reviewer 1 and 2

The sentences chaged from "A sensitivity analysis for an individual model parameter typically involves two model simulations, a baseline simulation and one with a modified parameter."

to

"A sensitivity analysis for an individual q10 parameter involves comparing the model state variable (V) from a baseline simulation (simulation with default q10 parameter as per Neumann et al. 2022) with that from a sensitivity simulation (a simulation with a perturbed q10 parameter). "

Line 222

Comment: The example of water column nitrate concentration at a selected station is confusing, as this does not appear to be part of the sensitivity test described. Consider clarifying why this example is included or removing it to avoid misleading the reader.

Response: We agree to the reviewer and therefore removed this example and revised the text to describe the model state variables and spatial domains directly evaluated in our sensitivity experiments.

Changes to manuscript: The example referring to water column nitrate concentration at a selected station has been removed. The sensitivity metric description has been revised to specify that it is applied to selected model state variables (nitrate, ammonium, phosphate, dissolved oxygen, and phytoplankton biomass) across the nine deep Baltic basins considered in this study by adding a sentence as follows

" To quantify the impact of q10 parameter perturbations on model behavior in the nine deep Baltic basins shown in Figure 2, we estimated a sensitivity metric S (Equation 4), representing the relative percentage change of each selected model state variable V between the sensitivity and baseline simulation. In this study V represents model state variables such as Nitrate (NO3), Ammonium (NH4), Phosphate (PO4), Dissolved Oxygen (O2) and phytoplankton biomass (phyto). "

Lines 223–224

Comment: The sentence beginning with "For a model…" up to the equation is currently unclear. It is not obvious whether you are calculating a change in sensitivity, a percentage change, or something else.

(1) The notation "i.e., ΔP" after parameter P is confusing, particularly because the equation defines S (sensitivity), not ΔP.

(2) The definitions of S (sensitivity) and ΔP appear too late (line 226), making this section difficult to follow.

(3) Symbols should only be used if they are clearly defined; otherwise, describing variables in words will improve readability.

(4) Prior to Equation 3, you define V, but Equation 3 uses Ve and Vo, whose descriptions appear only later. All variables, their definitions, and what is being calculated should be introduced before presenting the equation.

(5) It is also important to clarify whether V represents only a state variable (e.g., in concentration) or if it could also be a process rate, as this affects interpretation.

A clearer, stepwise explanation of what is being calculated and how each variable relates to the

sensitivity metric is essential to ensure readers can follow the Results and Discussion sections.

Response: Thank you for this detailed comment. We agree that the original description of the sensitivity metric was unclear and that the definitions of the variables were introduced too late. We have revised this section by removing the notation ΔP and P and now refer directly to the q10 parameter. We also clarify that S represents the relative percentage change in the selected model state variables between the sensitivity and baseline simulations and define all variables before Equation 3. Specifically, we now define V as the selected model state variable (e.g., nutrients, oxygen, and phytoplankton), V_0 as its value in the baseline simulation, and V_e as its value in the sensitivity simulation before presenting the equation.

Changes to manuscript: The methodology text preceding Equation 3 is rewritten to introduce the sensitivity metric S and all variables before the equation as follows

"To quantify the impact of q10 parameter perturbations on model behavior in the nine deep Baltic basins shown in Figure 2, we calculated a sensitivity metric S, representing the relative percentage change of each selected model state variable V between the sensitivity and baseline simulations. In this study, V represents model state variables such as Nitrate (NO3), Ammonium (NH4), Phosphate (PO4), Dissolved Oxygen (O2) and phytoplankton biomass (phyto). Here, hydrogen sulphide (H2S) is considered as a negative O2 concentration (2H2S=-O2). To estimate V, the model state variables were vertically integrated throughout the water column, averaged annually over the period 1989–2019, and then spatially averaged within each of the nine deep Baltic Sea basins shown in Figure 2. In Equation 4, V0 and VE represent the value of the selected state variable in the baseline simulation and the sensitivity simulation respectively."

Equation 3

Comment: Please clarify whether this equation has a reference or is commonly recognized as a standard sensitivity metric. To me, based on the equation itself, it appears more like a calculation of bias or percentage difference rather than a sensitivity measure. If it is being called "sensitivity," explain why, and provide context or reference so that readers can interpret it appropriately. Sensitivity can be estimated in different ways, so providing an explanation of the chosen method would improve the robustness of the analysis.

Response: We agree that sensitivity can be quantified using different approaches. In this study, we use the term sensitivity metric for "S" and "Sc" because the objective is to quantify how strongly model state variables respond to our parameter perturbations in the coast and the entire Baltic Sea. Equation 4 (for S) and equation 5 (for Sc) calculates the normalized relative change between the perturbed and baseline simulations, where the difference in the numerator directly represents the model response of our selected variable "V" induced by the parameter change. We have added clarification in the manuscript that this metric represents a relative response-based sensitivity measure.

Changes to manuscript: We have revised the manuscript to clarify the interpretation of the sensitivity metric S and Sc. Specifically, we now explain that "S" represents the relative percentage change of selected model state variables between the sensitivity and baseline simulations, and we have added definitions for V, V_0, V_E and V_c above Equation . The sentence added to the manuscript as follows

Please refer to the "changes to manuscript" section of above comment

Note: The equation for S and Sc now merged together follows

$S_X = (V_X - V_0) / V_0 * 100$

Where X=C and X=E for coastal only and entire Baltic Sea experiments.

Line 226

Comment: Please clarify what you mean by "parameter." In reality, processes such as nitrification and remineralization are biogeochemical processes, not simple parameters, which makes the current notation of ΔP=0.1×P somewhat confusing. It appears that the 0.1 represents your chosen 10% increase, but this could in principle take any value between 0 and 1. P corresponds to the temperature-dependent biogeochemical processes in your model (as listed in Table 2). Make sure this link between ΔP, P, and the 10% perturbation is clearly stated so that readers can interpret your calculations correctly.

Response: The term "parameter (P)" could be misunderstood as referring directly to the biogeochemical processes themselves. To avoid this confusion, we have therefore removed the notation "P" and "ΔP" from the analysis and now refer directly to the q10 parameter instead of using an additional notation "P"

Changes to manuscript:

The entire sentence "For a model state variable V of interest (such as water column nitrate concentration in a selected station), the percentage sensitivity to a change in parameter P (i.e. ΔP) can then be defined as" has been removed from the manuscript

Line 221: "individual model parameter" revised to "individual q10 parameter"

Line 227: "but depends both on ΔP" revised to "but depends both on Δq10"

The sentences "Here, we may assume that P (default parameter as per Neumann et al. 2022) may be the correctly chosen value for the offshore area, while for the coastal zone, a value of "P + ΔP " is closer to reality. " has been revised to "In these experiments, we assume that the default parameterization in Neumann et al. 2022 (i.e., the baseline simulation) is represents the open waters, while for the coastal zone a value of q10 +Δq10 is closer to reality. "

Line 243: "ΔP is still in the linear range." revised to "Δq10 is still in the linear range."

Lines 225–231

Comment: The relationship between ΔP and S (sensitivity) is not clearly explained. For example:

(1) Why does an S of 3% correspond to ΔP=0.1×P?

(2) What is the connection between V and P? Or are you referring to differences between experiments or different formulations?

Please clarify these relationships and rephrase the paragraph to explicitly define:

(1) What is being varied (ΔP, P, or a specific process)

(2) How this variation produces the sensitivity metric S

(3) The relevance of different S values across formulations, and

(4) The meaning of "in our example". Additionally, please clarify why the use of Celsius or Kelvin matters in this sensitivity analysis. As currently written, lines 225-231 are difficult to interpret and their relevance is unclear

Response: We thank the reviewer for this detailed comment. We have revised the paragraph to clarify the relationships between the model parameter "q10", the magnitude of parameter perturbation "Δq10", the model state variable "V", and the sensitivity metric "S". We have also revised the paragraph to explicitly describe how the perturbation is applied and how the resulting sensitivity value "S" is calculated.

(1) Why does an S of 3% correspond to ΔP=0.1×P?

To avoid the confusion we are not using "P" and "ΔP" in our revised manuscript,in the manuscript; instead, we directly use "q10" and "Δq10" to represent the "q10" parameter and changes in the "q10" parameter, respectively.

We have clarified that an "S" value of 3% does not directly correspond to the applied perturbation of (Δq10 = 0.1 . q10). Instead, "Δq10" defines the prescribed 10% increase applied to the selected "q10" parameter in the sensitivity simulation. The resulting "S" value represents the relative percentage change in the model state variable "V" caused by this perturbation. Thus, if a 10% increase in the "q10" parameter results in a 3% increase in "V" relative to the "V" in base simulation, the calculated sensitivity is (S = 3%).

(2) What is the connection between (V) and (P)?

We removed "P" from the revised manuscript, so now establishing the connection between "V" and "q10".

We have clarified that "V" and "q10" are connected through the model formulation. The parameter "q10" represents the temperature sensitivity of biogeochemical process rates. Changing "q10" modifies the corresponding process rates in the model, which subsequently affects the simulated state variables "V" such as NO3, NH4, PO4, O2 and Phytoplankton). The sensitivity metric "S" therefore quantifies the percentage change in "V" relative to V in the baseline simulation, resulting from the perturbation of "q10" between the sensitivity and baseline simulations.

(1) What is being varied (ΔP, P, or a specific process)

In this study, the perturbed quantity is the selected q10 parameter listed in Table 1. The biogeochemical process itself is not directly modified; rather, the parameter controlling its temperature dependence is perturbed.

2) How this variation produces the sensitivity metric S

We have clarified that "Δq10" defines the magnitude of the parameter perturbation, while "S" quantifies the resulting response of the model state variable "V" (in %) relative to "V" in the baseline simulation between the sensitivity and baseline simulations.

(3) The relevance of different S values across formulations, and

We have revised the text to clarify that the magnitude of "S" depends on both the applied perturbation and the mathematical formulation of the model equations. Therefore, "S" values should be interpreted within the context of the specific model formulation

(4) The meaning of "in our example". Additionally, please clarify why the use of Celsius or Kelvin matters in this sensitivity analysis.

We clarified that mathematically different formulations of the same process can result in different sensitivity values.

Changes to manuscript: The entire paragraph (lines 225 to 231) is revised to address the reviewer comments and the modifications in the manuscript are as follows:

"The positive and negative values of S indicate an increase and decrease of the selected state variable relative to the baseline simulation respectively. For example, an S of 3% indicates that V increased by 3% in the sensitivity simulation compared to the baseline simulation as a result of the applied 10% increase in the selected q10 parameter. In this study, the q10 parameter perturbation is defined as $\Delta q10 = 0.1 \cdot q10$, corresponding to a 10% increase in the default q10 parameter. Thus, the S describes the response of V to the prescribed paramater perturbation, rather than being directly determined by the magnitude of $\Delta q10$. It should be noted that this sensitivity measure is not objective but depends both on $\Delta q10$ and on subjective choices in the model formulation."

The following sentences removed from the manuscript

"In our example, it would depend on whether we give the temperature in Celsius or Kelvin. However, different but mathematically identical ways of writing the process equations will affect the value of S, such as using the square of a parameter rather than the parameter itself, or even logarithmic parameters. Therefore, a quantitative comparison between the S values for different parameters is not meaningful unless the exact formulation of the model equations is taken into account in the interpretation.""

Line 233

Comment: Please remove "The same holds for a measure …" and instead explicitly describe the goal of this calculation for the coastal zone. If you refer to "a third model run," please clarify which experiment or set of runs this corresponds to, as Table 2 shows multiple coastal simulations. Be precise so readers can reproduce or understand the analysis.

Response: We have removed the phrase "The same holds for a measure…" and revised the paragraph to explicitly describe the objective of the coastal sensitivity analysis. We now clarify that the coastal sensitivity metric (Sc) is calculated from additional sensitivity experiments in which the selected q10 parameter is perturbed only within the coastal zone, while the open-sea model parameter (q10) values remain unchanged. We have also specified that these experiments correspond to the coastal-only sensitivity simulations listed in Table 1 (C1_DetSed to C4_NitWat), allowing the experimental setup to be clearly identified.

Changes to manuscript: The revised text has been added to section 2.2.1 of the manuscript as follows:"To quantify how q10 parameter perturbations restricted to coastal regions influence the biogeochemical state of the nine deep Baltic Sea basins, we estimated an additional sensitivity metric called coastal sensitivity (Sc). For the calculation of Sc, additional sensitivity experiments were performed in which the q10 parameter is perturbed 10% only within the coastal zone (shaded region in Figure 2), while the open-sea q10 parameter values remained unchanged. This approach is motivated by previous studies showing that biogeochemical process rates can differ substantially between coastal and offshore environments (Singh et al., 2022; Ward et al., 2020; McGuire et al., 2001)."

Line 234

Comment: Please define what you mean by the "coastal zone," as there are multiple definitions for the Baltic Sea coastal areas. Include a reference for the one you consider in this study and add it also in the caption of Figure 2.

Avoid statements such as "we suspect," which are speculative. Instead, clearly explain the assumptions behind why you expect certain processes in coastal waters to differ from open waters. Reference the relevant literature and justify why these assumptions are applied to all shaded areas in your figures.

Finally, the repeated use of "parameter" is confusing. Consider using "process" or another term that better represents the real-world biogeochemical processes rather than model-specific terminology. Consistent and accurate wording will improve clarity throughout the manuscript.

Response: We have revised the manuscript to add the definition of the coastal zone, the rationale for applying coastal-only parameter perturbations, and the assumptions underlying our experimental design.

(1) Definition of the coastal zone: We have explicitly defined the coastal zone used in this study as model grid cells with water depths shallower than 20 m. This definition was chosen to represent the shallow, sediment-influenced region of the Baltic Sea and is supported by previous observations showing that sediment characteristics and macrofaunal communities can influence nutrient dynamics and coastal filter processes at depths extending beyond 20 m in Thoms et al., 2018.

(2) Rationale for the coastal only perturbation: We agree that the previous wording ("we suspect") was speculative and have therefore removed it. Instead, we now explain that the coastal-only perturbation is motivated by previous studies demonstrating that biogeochemical process rates can differ substantially between coastal and offshore environments (Singh et al., 2022; Ward et al., 2020; McGuire et al., 2001). We further clarify that the perturbation is applied uniformly across all coastal grid cells to represent a generalized contrast between coastal and offshore biogeochemical functioning, rather than resolving local-scale spatial heterogeneity within individual coastal regions.

We agree that the repeated use of the term "parameter" could be confusing without a clear definition. However, in this study, the perturbed quantity is the q10 model parameter controlling the temperature dependence of a given biogeochemical process, rather than the process itself. Therefore, replacing "parameter" with "process" would not accurately describe the methodology. Instead, we have clarified throughout the manuscript that q10 refers to the parameter controlling the temperature dependence of the corresponding biogeochemical process, thereby improving the consistency and clarity of the terminology.

Changes to manuscript: The lines 233 to 236 is rewritten as follows

"To quantify how q10 parameter perturbations restricted to coastal regions influence the biogeochemical state of the nine deep Baltic Sea basins, we estimated an additional sensitivity metric called coastal sensitivity (Sc; Equation 5). For the calculation of Sc, additional sensitivity experiments were performed in which the q10 parameter was perturbed 10% only within the coastal zone (shaded region in Figure 2), while the open-sea q10 parameter values remained unchanged. This approach is motivated by previous studies showing that biogeochemical process rates can differ substantially between coastal and offshore environments (Singh et al., 2022; Ward et al., 2020; McGuire et al., 2001). In this study, the coastal zone is defined as model grid cells with water depths shallower than 20 m representing the shallow, sediment-influenced region where benthic and sediment–water interactions are particularly relevant (Carstensen et al., 2020,Thoms et al., 2018). The perturbation was applied uniformly across all coastal grid cells to represent a generalized contrast between coastal and offshore biogeochemical functioning, rather than resolving local-scale spatial heterogeneity within individual coastal regions. The resulting model state variable (V) from these simulations in the nine deep Baltic Sea basins are denoted as Vc . The experiments correspond to the coastal-only sensitivity simulations listed in Table 1 (C1_DetSed to C4_NitWat).

Line 240

Comment: Is P always the default (original) parameter? This is not clear and needs to be defined well before this line. Maybe by adding this clarification "default parameter by Neuman et al..." where first mentioning P and ΔP (lines 225-230) and use same symbols or names in Table2 (baseline=P, 10%=0.1*P) Also, "we may assume" does not mean much, did you or did you not assume that the default P was correct for open waters? Is P the same as the baseline? Please rephrase by for example: "In this set of experiments, we assume that the default parameterization in Neumann et al..." i.e. baseline, is representative of open waters, while for the coastal zone a ... Is closer to reality" Also refer to Table 2 to better explain your set of experiments, instead of using "this model run".

Response: In the revised manuscript, we no longer use P and ΔP to represent the q10 parameter used in our analysis. Instead, we directly use q10 and Δq10.

We have revised the manuscript to explicitly state that q10 refers to the default q10 parameterization from Neumann et al. (2022), which is used in the baseline simulation. We further clarify that, in the coastal sensitivity experiments, we assume that the default q10 parameterization is representative of open-water conditions, whereas the perturbed parameter value (q10 + Δq10) better represents the coastal environment. We have also replaced the phrase "we may

assume" with a clear statement of this modelling assumption and revised the description to refer explicitly to the coastal sensitivity simulations rather than "this model run." In addition, we have clarified the notation for the baseline and perturbed parameter values throughout the manuscript and in Table 1

Changes to manuscript: The phrase “default parameter by Neumann et al” is added to the manuscript to show q10 is the default value

Table 2: Column “perturbation” gives details about the percentage of perturbation applied to each sensitivity simulation

Line 240-241: The sentence “Here, we may assume that P (default parameter as per Neumann et al. (2022)) may be the correctly chosen value for the offshore area, while for the coastal zone, a value of "P + ΔP" is closer to reality” is removed from the revised manuscript

Line 241: A sentence added to the revised manuscript as “In these experiments (Table 1 experiments C1_DetSed, C2_DetWat, C3_DocSed and C4_NitWat), we assume that the default q10 parameterization in Neumann et al. 2022 (i.e., the baseline simulation) is represents the open waters, while for the coastal zone a value of q10 + Δq10 is closer to reality ”

Line 243

Comment: Add a reference for the linear range.

Response: Thank you for the suggestion

Changes to manuscript: Since this assumption is not essential to the definition or interpretation of the coastal sensitivity metric, we have removed the sentence referring to the linear range.

Lines 245–259

Comment: If this represents a genuinely new methodological development (or at least its first application in this type of study), this should be clearly highlighted. If the approach has been used previously, please provide appropriate references. In either case, ensure that relevant sources are cited and that proper credit is given for the conceptual or statistical foundations of the method, even if they originate from well-established statistical framework

Response: The relative coastal sensitivity metric (Scr) presented in this study is a methodological development introduced specifically for this work to quantify the influence of q10 parameter perturbations restricted to the coastal zone of the Baltic Sea on the biogeochemical state of the deep Baltic Sea basins. To our knowledge, this formulation has not been applied previously in this context. We have revised the manuscript to explicitly state this. The metric can also be applied more broadly to assess the influence of coastal regions on biogeochemical conditions in other deep basins.

Changes to manuscript: The following line has been added to the manuscript at line 263 after “Equations 3, 4 and 5”

“To our knowledge, this coastal sensitivity metric is introduced here for the first time to quantify the influence of coastal parameter perturbations on the biogeochemical functioning of the deep Baltic Sea”

In addition a statement added to the revised manuscript as “ This approach is motivated by previous studies showing that biogeochemical process rates can differ substantially between coastal and offshore environments (Singh et al., 2022; Ward et al., 2020; McGuire et al., 2001). ”

Line 247

Comment: Please remove “may typically.” The statement would benefit from clearer and more explicit reasoning. For example, instead of “is smaller than one; in the coastal…,” consider rephrasing along the lines of: “we assume that this ratio is smaller than one because the parameter is only modified in the coastal region, which represents a limited fraction of the total basin area.”

Additionally, the transition toward Equation 5 could be smoother. Please explain more clearly why the ratio Sc/S alone is insufficient and why normalization by area is required. This will help justify the introduction of Equation 5 and improve the logical flow of the argment

Response: We have revised the paragraph to remove the wording "may typically" and to provide a clearer justification for the normalization by area. Specifically, we now explain that the ratio (Sc/S) is expected to be smaller than one because the q10 parameter perturbation in the coastal sensitivity experiments is applied only within the coastal region, which represents a limited fraction of the total Baltic Sea area, whereas the entire Baltic Sea sensitivity experiments apply the q10 parameter perturbation across the entire model domain. We further clarify that a small (Sc/S) ratio does not necessarily indicate that coastal processes are less important; rather, it may primarily reflect the smaller spatial extent over which the perturbation is applied. Therefore, we introduce the area normalization factor (Ac/A) to account for the different spatial coverage of the q10 perturbations and to obtain a metric that better represents the relative influence of coastal parameter perturbations.

Changes to manuscript:

The phrase "We may typically assume.." revised to "we expect that.."

The revised text has been added to the section 2.2.1 of the manuscript as follows

"We expect that the ratio Sc/S to be smaller than one because in the coastal sensitivity experiment (C1_DetSed to C4_NitWat) the parameter q10 is only modified in the coastal region, whereas in the entire Baltic Sea experiments (E1_DetSed to E4_NitWat) the perturbation is applied over the entire Baltic Sea. Since the coastal zone represents only a limited fraction of the total Baltic Sea area, Sc is expected to be much smaller than S. Consequently, the ratio Sc/S may become small because the q10 perturbation is applied over a much smaller area, rather than because the coastal processes are less important. Therefore, to account for the different spatial extent of the coastal zone relative to the entire Baltic Sea, we normalized the ratio Sc/S by the area ratio Ac/A (0.2884) , where Ac and A represents the area of coastal zone and the total area of the Baltic Sea respectively. The resulting metric is referred to as the Relative Coastal Sensitivity (Scr ; Equation 6)"

Lines 244–245

Comment: Please replace "the first" with a clearer and more neutral formulation, such as: "is an objective measure that allows comparison between…" You may also add that it "enables assessment of the relative impact of …" to better articulate its purpose.

Response: We have revised the sentence to use a more neutral formulation by removing "the first" and clarified that this ratio (Sc/S) provides an objective measure for comparing the relative impact of coastal parameter perturbations with perturbations applied across the entire model domain.

Changes to manuscript: The revised text has been added to the manuscript as follows;

"Therefore, this ratio is an objective measure comparable between the different model parameters. It allows us to assess the impact of changes in a specific parameter in the coastal zone on the ecosystem functioning of the deep Baltic Sea basins, relative to perturbations applied across the entire model domain."

Line 252

Comment: Why write "We assume that V is measured outside the coastal zone"? Is it measured outside or not? V comes from your model, you should know where V is extracted from your results. Can you refer to your map to explain this or simply remove this sentence?

Response: We agree that the statement "we assume that V is measured outside the coastal zone" is unclear and could be misleading. Therefore, we have removed this sentence from the manuscript to avoid confusion.

Changes to manuscript: The sentence has been removed from the manuscript.

Line 254–255

Comment: Please rephrase to be more specific on what is meant, for example, when using "system". Is it the entire system, including both coastal and open waters? And for clarity "in the parameter everywhere in the same way". Do

you mean that if S=0 the implemented changes in coastal waters affect only coastal waters, and if S=1 the entire system including open waters are affected by the coastal processes?

Response: We have revised the text to explicitly clarify that the metric (Scr) describes the response of the deep Baltic Sea basin ecosystem to parameter perturbations applied either only within the coastal zone or across the entire Baltic Sea domain. Here, the term "system" refers specifically to the nine deep Baltic Sea basins shown in Figure 2 and does not include the coastal waters. We now explain that (Scr=0) indicates that coastal parameter perturbations do not influence the corresponding deep basin ecosystem response, whereas (Scr ~ 1) indicates that the deep basin responds similarly to coastal-only and entire Baltic Sea domain q10 parameter perturbations. We have also added interpretations for (Scr>1) and negative (Scr) values to clarify cases where coastal perturbations produce stronger or opposite-sign responses compared with perturbations applied across the entire Baltic Sea domain.

It should also be noted that we did not assess how the sensitivity experiments affect biogeochemical conditions in the coastal regions themselves. Our analysis focuses on how perturbations to coastal q10 parameters influence the biogeochemical state of the deeper Baltic Sea basins.

Changes to manuscript: The entire paragraph rewritten for clarity as follows

"A value of Scr equal to zero indicates that perturbations applied to the parameter within the coastal zone do not influence the ecosystem functioning of the corresponding deep Baltic Sea basin. A value of Scr close to 1 means that the deep Baltic sea basin responds in the same way to the parameter perturbations both in the coastal zone and the entire Baltic Sea. So, in such cases, using identical parameter values for both coastal and offshore regions will not make substantial differences in deep Baltic Sea ecosystem responses. If the value of Scr is larger than one, this indicates that the deep Baltic Sea basin exhibits a stronger response to paramater perturbations applied witin the coastal zone compared with equivalent perturbations applied across the entire Baltic Sea domain. This suggests that the coastal paramaterization may require further investigation. If the value of Scr is negative, it indicates that the coastal-only and entire Baltic Sea q10 perturbation experiments produce responses of opposite sign in the corresponding V of deep Baltic Sea basin"

Line 255–256

Comment: "So, ignoring a deviation of this parameter…" unclear what is meant with this sentence. Which deviation and which parameter? And the coastal area is anyway small compared to the open ocean so why the if? Please rephrase.

Response: We agree that the previous statement regarding "ignoring a deviation of this parameter" was unclear because it did not explicitly define which parameter deviation was being referred to and could lead to confusion. We have removed this statement and replaced it with a clearer one

Changes to manuscript: The sentence was rephrased: "So, in such cases, using identical parameter values for both coastal and offshore regions will not make substantial differences in deep Baltic Sea ecosystem responses."

Line 256

Comment: Replace "we should make the effort…, since" by "that the process in the coastal zone requires further constraint, since ...". Also what errors? They are not errors per say; parametrisation may not capture all, but I would not call them errors. Please also rephrase the end of the sentence.

Response: We have revised the sentence to clarify that a value of (Scr>1) indicates a disproportionately strong response of the corresponding deep Baltic Sea basin to q10 parameter perturbations applied in the coastal zone relative to the spatial extent of the perturbed coastal area. We have also replaced the subjective wording "we should make an effort" with a more objective description that the coastal process parameterization may require further constraint.

Changes to manuscript: The sentence revised from

"If the value of Scr is larger than one, this means we should make an effort to specifically constrain the parameter in the coastal zone, since the system reacts with a higher sensitivity to errors we make in the parameterization here."

To

" If the value of Scr is larger than one, this indicates that the deep Baltic Sea basin exhibits a stronger response to paramater perturbations applied within the coastal zone compared with equivalent perturbations applied across the entire Baltic Sea domain. This suggests that the coastal paramaterization may require further investigation."

Line 259

Comment: Please replace "below zero" by "negative".

Response: "below zero" replaced to "negative"

Changes to manuscript: "substantially below zero" has been replaced with "negative":

Lines 258–260

Comment: I do not fully understand the distinction between an Scr value greater than 1 and one that is substantially negative. Don't both cases indicate a need for further constraints on the processes, since they both reflect differences between coastal and open-ocean conditions? How are the strict thresholds defined, and on what basis? Additionally, how can we be confident that these values are not ecologically significant—can this be supported with references?

Response: We agree that the previous explanation did not sufficiently clarify the distinction between (Scr>1) and negative (Scr < 1), and that the rationale for the thresholding procedure required further explanation.

A value of (Scr>1) indicates that the deep Baltic Sea basin exhibits a stronger relative response to q10 parameter perturbations applied within the coastal zone compared with equivalent perturbations applied across the entire Baltic Sea domain, suggesting that the coastal parameterization may require further constrain. Similarly, negative (Scr) values indicate that coastal-only and full-domain perturbation experiments produce responses of opposite sign in the corresponding deep Baltic Sea basin. The magnitude of a negative (Scr) value further indicates the relative strength of the coastal response compared with the full-domain response; therefore, a negative value with an absolute magnitude greater than one represents a stronger coastal response occurring in the opposite direction compared with the full-domain perturbation.

We have also clarified the thresholding procedure. The 1% cutoff is not intended to represent an ecological threshold; rather, it is a practical numerical criterion used to exclude very small responses relative to the applied perturbation that may be dominated by numerical variability. The revised text now reflects this interpretation.

Changes to manuscript: The negative-Scr case was elaborated with a mechanistic example as follows:

"If the value of Scr is negative, it indicates that the coastal-only and full-domain perturbation experiments produce responses of opposite sign in the corresponding deep Baltic Sea basin. The magnitide of Scr further indicates the relative strength of the coastal response compared with the full-domain response. Thus, a negative value with an absoulte magnitude greater than one indicates that the coastal perturbation produces a stronger response than the full-domain perturbation, but in the opposite direction. For example, a parameter increase applied only within the coastal zone may increase a state variable in the deep basin, whereas the same parameter increase applied across the entire Baltic Sea domain may decrease it. Such behavior suggests mechanistic differences between coastal and offshore biogeochemical controls on the basin-scale ecosystem response. A threshold is applied as a practical criterion to exclude very small responses that may be dominated by numerical variability. The Scr values are masked when the absolute value of S is less than 1. This cutoff eliminates responses that are small relative to the applied perturbation."

Line 264

Comment: "five targeted biogeochemical variables" could be replaced by "five key biogeochemical variables".

Response: the modification made in the text as follows

Changes to manuscript: "five target biogeochemical variables" has been changed to "five key biogeochemical variables."

Line 265

Comment: Please replace “we assume” with “we analyze” or a similar phrasing, since in a model the source of your variables is known. This is the foundation of your analysis, so it should not be presented as an assumption; otherwise, the validity of the method could be called into question.

Response: Thank you for poiniting out. We have replaced “we assume” with “we analyze”

Changes to manuscript: The entire sentence revised to “To estimate V, the model state variables were vertically integrated throughout the water column, averaged annually over the period 1989–2019, and then spatially averaged within each of the nine deep Baltic Sea basins shown in Figure 2”

Results

General comment

Comment: While the results are interesting and innovative, providing valuable insights into basin-scale nutrient dynamics, the result section could benefit from a better structuring. The figures are rich and informative but relatively complex, so it sould be helpful to first provide the reader with a overview or “roadmap” before diving into the details.

The results spans multiple basins, nutrients and experiments, and the current presentation can feel a bit fragmented.

Structuring the results more clearly—for example, by briefly outlining how they will be described and the rationale for the order (e.g., starting with NH4 if it shows the most variation across basins and experiments)—would greatly enhance readability.

Additionally, providing more precise references to figures, experiments, and specific basins throughout the text will help guide the reader and make it easier to follow the narrative. Overall a clearer structure and more explicit connections between text and figures would strengthen the presentation of your results

Response: We thank the reviewer for this valuable suggestion. We agree that a clearer overview of the structure and rationale of the Results section improves readability. we have added a new introductory paragraph at the beginning of the Results section (section 3) to provide a roadmap of the analysis and clarify the progression from basin-wide perturbations to coastal perturbations and their relative impacts. The revised structure first describes the response of the Baltic Sea ecosystem to basin-wide parameter perturbations (E1_DetSed to E4_NitWat), followed by the responses to coastal perturbations (C1_DetSed to C4_NitWat), the quantification of relative coastal sensitivity using the Scr metric, and finally the spatial patterns and mechanisms across individual Baltic Sea sub-basins.
In addition, we have revised the Results section to include more explicit references to the relevant figures, sensitivity experiments, and specific basins throughout the text. These changes provide clearer links between the described results and their corresponding visual representations

Changes to manuscript: A new paragraph is added to the text in section 3 as follows;

“To systematically assess the impacts of enhanced temperature sensitivity of biogeochemical processes, we first evaluate the response of the Baltic Sea ecosystem to basin-wide parameter perturbations in different sensitivity experiments (E1_DetSed to E4_NitWat; section 3.1) We begin by describing changes in nutrient dynamics, including nitrogen and phosphorus cycling (section 3.1.1), followed by associated responses in oxygen and phytoplankton (section 3.1.2). We then focus on the effects of enhanced process rates in the coastal zone in different sensitivity experiments (C1_DetSed to C4_NitWat, section 3.2) and examine how these coastal localized changes influence the basin-scale biogeochemical state. Since the magnitude of coastal impacts can be masked by the smaller spatial extent of shallow coastal regions, we subsequently introduce the relative coastal sensitivity metric (Scr , section 3.3) to quantify the contribution of coastal processes relative to basin-wide enhancements. Finally, we investigate the spatial patterns and underlying mechanisms responsible for differences between coastal and basin- wide process enhancements across individual Baltic Sea sub-basins (section 3.4).”

References to the figures have been added throughout the Result section. The corresponding line numbers are provided below.

(1) references added to Figure 3: Line 275-276, 278, 284, 288, 291, 293, 297, 299

(2) references added to Figure 4: Line 306

(3) references added to Figure 5: Line 326, 332, 339, 341

Line 272

Comment: Missing ")" after Figure 3.

Response: Added the missing ")"

Changes to manuscript: Added the missing ")"

Line 273

Comment: A tracer is not the same as a nutrient, phytoplankton or oxygen and before you were referring to these as variables. Please be more specific on the wording and also avoid the term "tracers" when referring to biogeochemical variables throughout the manuscript.

Response: The term "tracer" has been revised to "biogeochemical variables" through out the manuscript

Changes to manuscript: The manuscript was revised to refer to "biogeochemical variables" rather than "tracers": Changes applied to following line numbers

Line 264, 273, 275

Line 274

Comment: Is "base" the same as "baseline"? If so, please be consistent with the wording. Also, why specify that they are not converted to inventories? Why not simply say "expressed in concentrations" or similar? Now the reader wonders why inventories would be necessary, without any further explanation.

Response: Here the "base" means "baseline" and we revised the manuscript to use the term "baseline" consistently. We also agree that the previous wording regarding inventories was unclear. The intention was to clarify that the depth-integrated values were calculated for comparison between simulations and were not converted into total basin inventories. We have revised the text to more clearly describe these values as depth-integrated biogeochemical variables rather than referring to inventories.

Changes to manuscript: The sentence changed from

"Values are for comparison between base and sensitivity simulations and are not converted to inventories"

To

"Values are for comparison between baseline and sensitivity simulations."

Line 276

Comment: Please remove ")" after values.

Response: The ")" removed from line 275

Changes to manuscript: The ")" removed from line 275

Line 277

Comment: The reader may find it difficult to follow which experiment is being discussed, as the names in Table 2 do not match those used in the figures. It would be helpful to update the figure labels to correspond directly to Table 2 for consistency.

Response: (1) We thank the reviewer for highlighting this inconsistency. Following the previous reviewer suggestion, Tables 1 and 2 have been merged into a single table (now Table 1) in the revised manuscript. In addition, the experiments E1–E4 and C1–C4 have been renamed as E1_DetSed, E2_DetWat, E3_DocSed, E4_NitWat and C1_DetSed, C2_DetWat, C3_DocSed, and C4_NitWat, respectively. The same experiment names are now used

consistently in the table, figures, and text throughout the manuscript, which improves readability and helps readers identify the corresponding process modifications and experiments more easily.

Changes to manuscript: The experiments E1–E4 and C1–C4 have been renamed as E1_DetSed, E2_DetWat, E3_DocSed, E4_NitWat and C1_DetSed, C2_DetWat, C3_DocSed, and C4_NitWat, respectively, in the table, text, and figure labels of Figures 3, 4, and 5.

Regarding the reference to "30–45%" in the text, these values are not immediately evident in Figure 3, and lower values (eg. <10% for NH4 in Arkona and Bornholm for E1 and E2) are also present. Please be more specific and clarify where these numbers can be found in the figure (e.g. in which experiment).

We have revised the text to clarify that the reported 30–45% sensitivity corresponds specifically to the **Gotland Basins** in the detritus recycling experiments **(E1_DetSed and E2_DetWat)**. We have also added a comparison with the lower sensitivities observed in other basins to better describe the spatial variability shown in Figure 3.

Changes to manuscript:

To clarify the reported reference of 30-45% in the text, we modified the text from

"The strong positive sensitivity is highly significant, as a 10% change in the q10 parameters for detritus recycling in the water and sediment results (E1 and E2) in a 30–45% change in water column NH4, especially in the Gotland Basins (Figure 3). "

To

"The strong positive sensitivity is particularly evident in the Gotland Basins, where a 10% change in the q10 parameters for detritus recycling in the water and sediment (E1_DetSed and E2_DetWat) results in a 30–45% relative increase in water column N H4 compare with the baseline simulation (Figure 3). In comparison, lower sensitivities (< 10%) are observed in all other Basins."

Additionally, it may be more effective to start by highlighting which basins show the strongest positive sensitivity. Additionaly indicate whether these percentages corresponds to actual increase in NH4 concentrations or simply relative changes, and ensure this is clearly explained in the methods section. Providing this clarity will make it easier for readers to interpret the results accurately

We have revised the text to begin by identifying the Gotland Basins as the regions exhibiting the strongest positive NH_4 sensitivity. In addition, we have clarified that the reported 30–45% values represent relative increases in NH_4 compared with the baseline simulation, rather than absolute changes in concentration. The definition and interpretation of the sensitivity metric (S), which represents the relative percentage change between the sensitivity and baseline simulations, are already described in Section 2.2.1. The revised wording in the Results section now explicitly links the reported sensitivity values to this S metric in the methodology.

Changes to manuscript:

The text revised from "The strong positive sensitivity is highly significant, as a 10% change in the q10 parameters for detritus recycling in the water and sediment results (E1 and E2) in a 30–45% change in water column NH4, especially in the Gotland Basins(Figure 3)"

To

"The strong positive sensitivity is particularly evident in the Gotland Basins, where a 10% change in the q10 parameters for detritus recycling in the water and sediment (E1_DetSed and E2_DetWat) results in a 30–45% relative increase in water column N H4 compare with the baseline simulation (Figure 3). In comparison, lower sensitivities (< 10%) are observed in all other Basins. "

To make the definition of sensitivity metric "S" in equation 4 (the equation number changed in the revised manuscript), we have added a text in section 2.2.1 as follows

"To quantify the impact of q10 parameter perturbations on model behavior in the nine deep Baltic basins shown in Figure 2, we calculated a sensitivity metric S (Equation 4), representing the relative percentage change of each selected model state variable V between the sensitivity and baseline simulations. In this study, V represents model state variables such as Nitrate (NO3), Ammonium (NH4), Phosphate (PO4), Dissolved Oxygen (O2) and phytoplankton biomass (phyto). "

Line 278

Comment: The statement "is evident in E4" is not clear. Is evident where? In all basins? Please rephrase "A strong negative sensitivity of NH4 is evident in E4, which is a response to basin wide enhanced nitrification" by for example, "E4 shows the strongest negative sensitivity in all basins." In addition, the last part of the sentence should be better explained as it is not obvious. Why would enhanced nitrification increase the negative sensitivity?

Response: We agree that the original wording did not clearly indicate the spatial extent of the negative NH_4 response or explain the underlying mechanism. The text has been revised to clarify that E4_NitWat is the only sensitivity experiment that produces negative NH_4 sensitivity across all Baltic Sea basins. We have also explained that this response results from enhanced nitrification, which accelerates the conversion of NH_4 to NO_3 and consequently reduces NH_4 concentrations relative to the baseline simulation.

Changes to manuscript: The sentence revised from "A strong negative sensitivity of NH4 is evident in E4, which is a response to basin wide enhanced nitrification"

To

"Among the sensitivity experiments, E4_NitWat is the only experiment that produces negative NH4 sensitivity (S) across all **Baltic Sea Basins**, with the smallest negative sensitivities occur in the Gotland basins. Enhanced nitrification increases the conversion of NH4 to NO3, thereby reducing NH4 concentrations relative to the baseline simulation(Figure 3)."

Line 281–284

Comment: This is interesting, but it is hard to follow if the meaning of these % is not better explained.

Response: We agree that the interpretation of the reported sensitivity percentages was not sufficiently clear in the Results section. To make these percentages easier to understand, we have now clearly described the calculation of S and Sc in Section 2.2.1. In addition, we have revised the text to explicitly clarify the meaning of the reported NH_4 sensitivity values. Specifically, we now indicate that the reported 48% sensitivity in the Northern Gotland Basin represents a relative increase in NH_4 concentration compared with the baseline simulation.

Changes to manuscript: The text in the manuscript revised from

"For instance, in the Northern Gotland Basin, NH4 sensitivity to the enhanced detritus recycling in the sediment (i.e, E1) exceeds 48%, indicating that even a moderate increase in detritus recycling substantially increases the NH4 concentration in the basin. The accumulation of NH4 is observed with a significant decline in water column NO3. In the oxic basins (southern and northern basins), the sensitivity of NH4 (2 to 7%) is weaker (but still significant) compared to the Gotland Basin."

To

"For instance, in the Northern Gotland Basin, NH4 sensitivity to the enhanced detritus recycling in the sediment (i.e, E1_DetSed) exceeds 48%, indicating that even a moderate increase in detritus recycling substantially increases the NH4 concentration in the basin **compared with the baseline simulation**. The accumulation of NH4 is observed with a significant decline in water column NO3. In the oxic basins (southern and northern basins), the sensitivity of NH4 (2 to 7%) is weaker compared to the Gotland Basin **where the response reaches values above 48%. (Figure 3)**."

In addition, the definition and interpretation of the sensitivity metric "S" and "Sc", which represents the relative percentage change between the sensitivity and baseline simulations, are already described in Section 2.2.1

Figure 3

Comment: In the caption, because this was not very clear from the methods "Percentage sensitivity" remains a vague wording. Make sure it is better explained and maybe think of renaming it to something more meaningful. Some additional points for this figure:

(1) Please replace "(q10 sed rec, …)" by the actual names of the experiments used in Table 2 and in the figure itself as well. (2) Please rephrase "10% increase in biogeochemical parameters" as it is misleading, it is a 10% increase in the temperature sensitivity of the biogeochemical parameters.

(3) What does "negative sensitivity" means? What does "increased target variable values" mean? Please clarify. Comments also valid for Figure 4 and 5.

Response: Thank you for the comment. The figure caption (for Figure3, 4 and 5) has been revised accordingly. Specifically, the term "Percentage sensitivity" has been replaced with "The sensitivity metric S" to be consistent with the definition introduced in the Methods (section 2.2.1). Similar changes applied to Figure 4 and 5 also.

(1) the q_{10} parameter names have been replaced with the corresponding experiment names (E1_DetSed, E2_DetWat, E3_DocSed, and E4_NitWat) used consistently throughout the revised manuscript. Similar changes applied to Figure 4 and 5.

(2) The description of the q10 perturbation has been clarified to state that the experiments represent a 10% increase in the q_{10} temperature sensitivity of biogeochemical processes, and

(3) the meaning of positive and negative values of the sensitivity metric S and Sc has been explicitly explained by stating that positive values indicate an increase, and negative values indicate a decrease, in the corresponding ecosystem variable V relative to the baseline simulation.

Changes to manuscript: The Figure 3 caption was revised to

"Figure 3. The sensitivity metric S for five key ecosystem variables V (Phytoplankton, NO3, NH4, PO4, and O2) to a 10% increase in the q10 temperature sensitivity of biogeochemical processes across four sensitivity experiments (E1_DetSed, E2_DetWat, E3_DocSed and E4_NitWat) across nine basins of the Baltic Sea. Red and blue shades indicate positive and negative values of S respectively. Positive and negative values of S indicate an increase and decrease in corresponding ecosystem variable V relative to the baseline simulation respectively. Units are in %.

The Figure 4 and 5 captions are revised in the similar way

Lines 286–287

Comment: Is this in general or in specific experiments? Please specify.

Response: This mixed response is evident in E1_DetSed and E2_DetWat. We intended to clarify that, in E1_DetSed, the **S** values for NO_3 are positive in the Arkona Basin, Bornholm Basin, and Bothnian Bay, whereas all other basins show negative **S** values.
Similarly, in E2_DetWat, all basins show negative **S** values except the Eastern Gotland Basin, Northern Gotland Basin, and Western Gotland Basin.
Here this mixed positive and negative responses are visible in both E1_DetSed and E2_DetWat

Changes to manuscript: The sentence changed from

"Nitrate (NO3) exhibits mixed responses to enhanced detritus recycling rates, showing a pronounced negative sensitivity in E1 for the Gotland Basin (-6 to -11%) as well as in the Gulf of Riga, Gulf of Finland, and Bothnian Bay (-2 to -3%)."

To

"Nitrate (NO_3) exhibits mixed responses to enhanced detritus recycling rates in both the sediment and water column. In E1_DetSed, NO_3 shows pronounced negative sensitivity in the Gotland Basins, with S values ranging from −6% to −11% in the Western, Eastern, and Northern Gotland Basins, while weaker negative sensitivities (−2% to −3%) are

observed in the Gulf of Riga, Gulf of Finland, and Bothnian Bay. In contrast, all other basins show positive sensitivity. In E2_DetWat, NO_3 shows positive sensitivity in the southern and northern basins (Arkona, Bornholm, Bothnian Sea, and Bothnian Bay) and negative sensitivity in the Northern, Western, and Eastern Gotland Basins, which is consistent with the spatial pattern observed in E1_DetSed (Figure 3)."

Line 290

Comment: Remove "Case" in "(i.e. Case-E2)".

Response: The word "Case" removed

Changes to manuscript: "(i.e, Case-E2)" has been changed to "(i.e, E2_DetWat)".

Line 292

Comment: Missing space in "...Bornholm).T".

Response: Missing space added to the text

Changes to manuscript: "Bornholm).T" replaced to "Bornholm). T"

Line 303

Comment: Please replace "key ecosystem variables" by "the five key ecosystem variables" as they are the same as the ones described before.

Response: Revised the text as per the suggestions

Changes to manuscript: The text was revised to "Figure 4 shows the sensitivity of five key ecosystem variables to increased temperature dependence in process rates."

Line 304

Comment: The text does not specify what is meant by "strong" or "weak" sensitivity. For example, is a value of ~2% considered weak? The figure for PO4 shows values close to 2%. Please clarify your criteria and indicate whether this classification applies across all cases and basins.

Response: We agree with the reviewer that the terms "weak" and "strong" were not sufficiently defined, particularly because the coastal perturbation affects only a fraction of the Baltic Sea domain. A coastal response of ~2% should therefore not be classified as "weak" based on its magnitude alone. The coastal region occupies 28.84% of the Baltic Sea area in our model (Ac/A = 0.2884). If the response to a coastal perturbation scaled proportionally with the affected area, the expected coastal sensitivity would be Sc = 0.2884 × S. Thus, for example, a 2–3% basin-wide sensitivity would correspond to an expected coastal sensitivity of approximately 0.58–0.87%.

In the original manuscript, we used the term "weak" based only on the absolute magnitude of the coastal response and did not explicitly consider this area-proportional expectation. We agree that this could be misleading, since a relatively small coastal sensitivity can still represent a substantial influence when the much smaller spatial extent of the coastal region is taken into account. We have therefore revised this section to remove the qualitative classification of responses as "weak" or "strong" and instead report the sensitivity values explicitly.

Changes to manuscript: The sentence "All the targeted variables show weak responses to enhanced process rates except NH4 (a positive sensitivity 3% in the western and northern Gotland basin, and the Gulf of Finland in C2" removed from the revised manuscript.

The entire paragraph rewritten to include "Among the five biogeochemical variables considered, NH4 exhibits the highest coastal sensitivities (Sc) in the C2_DetWat experiment, with values of approximately 3% in the western and northern Gotland basin, and the Gulf of Finland (Figure 4). The corresponding basin-wide sensitivities (S) for NH4 in the western and northern Gotland Basin and the Gulf of Finland are 36.29%, 43.79%, and 24.04%, respectively (Figure 3), corresponding to expected coastal sensitivities of 10.47%, 12.63%, and 6.94%, respectively, if the basin-wide response scaled proportionally with the coastal area fraction (i.e., 28.84). The observed Sc of approximately 3% (for NH4) is therefore substantially lower than these expected values. Conversely, the smaller Sc values of the other

biogeochemical variables (NO3, NH4, PO4 and phyto in C1_DetSed and C2_DetWat, Figure 4) do not necessarily indicate weaker coastal influences. This is particularly evident in the Bothnian Bay for C1_DetSed and C2_DetWat, where relatively small Sc values correspond to a strong coastal influence after accounting for the spatial extent of the perturbed region, as discussed in Section 3.3."

Also see reviewer 2 comments for Line 304.

Line 305

Comment: Replace "targeted" by "the five key" here and throughout the manuscript.

Response: We agree that the term "targeted variables" was not the most appropriate description, as the analysis focuses specifically on five selected ecosystem variables V (phytoplankton, NO_3, NH_4, PO_4, and O_2). Therefore, we have replaced "targeted variables" with "five key variables" throughout the manuscript for consistency and clarity.

Changes to manuscript: The following revisions have been made

1. Figure 3 caption "targeted ecosystem variables" has been replaced with "key five ecosystem variables"

2. Figure 3 caption: "The targeted variables are depth" has been replaced with "The key five variables are"

3. Line 304 " All the targeted variables show" has been replaced with "All the five key variables show"

4. Figure 4 caption: "targeted ecosystem variables" has been replaced with "five key ecosystem variables"

5. Figure 4 caption: "The targeted variables are depth" has been replaced with "The five key variables are depth"

Line 511: "reach the deep location at which the targeted variables" has been changed with "reach the deep location at which the five key variables"

Line 524: "change the parameter such that the observations of targeted variables are matched better" has been replaced with " change the parameter such that the observations of five key variables are matched better"

Line 308–310

Comment: The areas should not be specified in results, but where coastal areas are defined. This statement should come before, not here.

Response: We agree that the definition of the coastal region, including its depth criterion and spatial extent, is a methodological detail and should be introduced in the Methods section rather than the Discussion. Therefore, we have moved this information from the Discussion to Section 2.2.1 (Sensitivity analysis methodology), where the coastal sensitivity experiments are described.

Changes to manuscript: The words "depth less than 20m; 9.976 × 10^4 km2" and "3.459 × 10^5 km2" removed from the sentence "It is crucial to note that the coastal region (depth less than 20m; 9.976 × 10^4 km2) constitutes a small fraction of the total area of the Baltic Sea (3.459 × 10^5 km2). " Additionaly a sentence added to section 2.2.1 of the methodology as "In this study, the coastal zone is defined as model grid cells with water depths shallower than 20 m and have an area of 9.976×10^4 km2 a small fraction of the total area of the Baltic Sea (3.459 × 10^5 km2). "

Line 331

Comment: Please consider rephrasing or removing the last sentence. Alternatively, make it more specific. For example, do you mean that an Scr=0 indicates that the enhanced coastal processes do not create a significant bias in the five analyzed key variables for these specific basins? Additionally, the phrase "spatial differences" may be misleading, as your analysis compares coastal versus open-ocean conditions rather than a continuous gradient.

Response: We agree that the original statement was too general and that the term "spatial differences" could be misleading. We have therefore revised the sentence to clarify that this interpretation applies only to the analyzed basins and key ecosystem variables.

Changes to manuscript: The sentence rephrased from

"So ignoring the spatial differences in coastal v/s deep-sea process rates will not cause much bias in the model."

To

"Therefore, for these specific basins and variables, neglecting the distinction between coastal and open-ocean process rates introduces only a minor model bias (Figure 5)."

Line 335

Comment: For clarity, add the scenario(s) that you are referring to here in parenthesis. Also, please rephrase to ". This highlights a significant ...."

Response: We have revised the sentence by specifying the corresponding scenarios in parentheses (E1_DetSed and C1_DetSed) and rephrased the text as suggested.

Changes to manuscript: The sentence have been revised from

"In contrast, the Gulf of Riga and Gulf of Finland show an elevated Scr of 1.76 and 1.055 for NO3, respectively, for the recycling in the sediment, highlighting a significant influence of coastal sediment recycling in the NO3 dynamics in thesebasins."

to

"In contrast, the Gulf of Riga and Gulf of Finland show an elevated Scr of 1.76 and 1.055 for NO3, respectively, for the recycling in the sediment (E1_DetSed and C1_DetSed). This highlightsing a significant influence of coastal sediment recycling in the NO3 dynamics in these basins."

Line 336

Comment: The figures show only the overall effect of all coastal areas. It would be helpful to discuss which specific coastal regions might drive the responses—are basins mainly influenced by nearby coasts or by multiple regions? Even if this cannot be fully quantified with the current results, a brief discussion would add valuable context. Would it be possible to show nutrient transport or current patterns from your model results to support this discussion?

Response: We thank the reviewer for this valuable comment. We agree that identifying the contribution of individual coastal regions would provide additional insight into the mechanisms controlling the basin-scale responses. However, the current sensitivity experiments were designed to quantify the integrated influence of coastal process parameterizations by applying the perturbation uniformly across all coastal grid cells. Therefore, the calculated (Scr) values represent the combined effect of all coastal regions influencing the respective deep basins and cannot distinguish the contribution of individual coastal areas.

We have clarified this limitation in the revised manuscript and added a discussion that the observed responses may reflect the integrated influence of multiple coastal regions and their subsequent effects on basin-scale biogeochemistry. A detailed attribution of the responses to specific coastal regions would require additional analyses of nutrient transport pathways and circulation patterns, which are beyond the scope of the present study.

Changes to manuscript: The following clarification has been added in Line 337 after "... In these deep domains" as "However, In this study the sensitivity experiments (C1_DetSed to C4_NitWat) perturb all coastal regions simultaneously, the present analysis cannot distinguish the contribution of the individual coastal areas to the observed basin-scale response. The estimated Scr therefore represents the integrated influence of all coastal regions affecting these offshore basins, including the effects of coastal processes and their subsequent transport within the Baltic Sea circulation. "

Line 344

Comment: The phrasing "respond opposite" is unclear. Does it mean that phytoplankton and PO4 decrease while NO3 increase? Can you explain this better? How can this be?

Response: We agree that the phrase "respond opposite" was unclear and could be interpreted as opposite responses among different variables (e.g., PO4 and NO3). Our intention was to indicate that the negative (Scr) values represent opposite-sign responses between the coastal-only sediment recycling perturbation (C1_DetSed) and the corresponding basin-wide sediment recycling perturbation (E1_DetSed). We clarify that this interpretation is not related to a contrasting response between NO3 and PO4 concentrations. Instead, the negative (Scr) values indicate that the response of the same variable (PO4) or phytoplankton differs in sign between the coastal-only and basin-wide sediment recycling perturbation experiments.We have revised the text to clarify that the contrasting response is between the two spatial configurations of the perturbation rather than between different biogeochemical variables.

Changes to manuscript: The sentence changed from "The phytoplankton and PO4 show a strongly negative Scr values of −2.98 and −3.35 respectively, indicating that the basin PO4 cycle and primary production respond opposite to the enhanced sediment recycling in the coast from that of the entire Baltic Sea"

to

"The phytoplankton and PO4 show strongly negative Scr values of −2.98 and −3.35 respectively, indicating that the response to enhanced coastal sediment recycling (C1_DetSed) is opposite in sign to the response obtained from enhanced sediment recycling across the entire Baltic Sea (E1_DetSed). The coastal-only (C1_DetSed) and basin-wide (E1_DetSed) perturbations therefore produce contrasting responses in PO4 and phytoplankton, indicating that the spatial extent of sediment recycling enhancement influences nutrient availability and primary production responses in the Bothnian Bay."

Line 350

Comment: Please add the scenario that is referred to in parenthesis for clarity. Also what do you mean by "basin wide", the open ocean, specific basin or entire Baltic Sea? Please specify and make sure this is clear throughout the text.

Response: The (Scr) values represent the relative coastal sensitivity calculated from the comparison between the corresponding coastal-only and entire Baltic Sea domain perturbation experiments. We have clarified that the reported (Scr) values for DetWat are derived from the E2_DetWat and C2_DetWat experiments. In addition, We have now clarified that the term "basin-wide" refers to sensitivity experiments in which the q10 parameters are perturbed across the entire Baltic Sea domain. This definition has been added at the first occurrence of the term in the manuscript, and the uses of "basin-wide" have been revised throughout the text where appropriate to improve clarity.

Changes to manuscript: The sentence replaced from

"For detritus recycling in the water column, Scr values near unity for phytoplankton (0.93) and PO4 (1.00) indicate that the coastal and basin wide enhanced water column recycling exerts comparable influences on these variables. "

to

"For detritus recycling in the water column, Scr values (DetWat in Figure 5 calculated from the experiments E2_DetWat and C2_DetWat) near unity for phytoplankton (0.93) and PO4 (1.00) indicate that enhancing water-column detritus recycling in the coastal zone and across the entire Baltic Sea domain exerts comparable influences on these variables"

The "basin-wide" replaced with more clear wording as follows

Line7: the sentence "..processes by increasing the q10 parameter both basin wide and selectively in the coastal zone of the Baltic Sea" have been revised to "We conducted two sets of sensitivity experiments to examine the effect of enhancing the temperature driven biogeochemical processes by increasing the q10 parameters across the entire Baltic Sea domain and selectively within the coastal zone. "

Line13-14: the sentence "Basin wide enhanced water column remineralization also caused increased phosphate concentrations in the Bothnian Bay" have been revised to "Enhanced water column remineralization across the Entire Baltic Sea domain also caused increased phosphate concentrations in the Bothnian Bay"

Line 17-18: the sentence “The analysis shows that the nitrate cycle is disproportionately sensitive to coastal sediment recycling, with the Bothnian Bay displaying two to four fold stronger responses in nutrient cycles than basin wide perturbations” have been revised to “The analysis shows that the nitrate cycle is disproportionately sensitive to coastal sediment recycling, with the Bothnian Bay displaying two to four fold stronger responses in nutrient cycles than the corresponding entire Baltic Sea perturbations”

Line 21-22: the sentence “In contrast, basin wide enhancement releases ..” have been revised to “In contrast, nhancement across the entire Baltic Sea domain releases”

Line 270: the heading changed from “3.1 Impact of basin wide enhanced biogeochemical process rates” to “Impact of enhanced biogeochemical processes rated across the entire Baltic Sea domain”

Line 302: the sentence changed from “Following the basin wide assessment of enhanced temperature sensitivity on biogeochemical variables, ” to “Following the basin wide assessment of enhanced temperature sensitivity on biogeochemical variables, (hereafter "basin wide" refers to sensitivity experiments in which q10 parameters are perturbed across the entire Baltic Sea domain), ”

Line 350–351

Comment: This statement is important and may explain why different models struggle to capture the Bothnian Bay nutrient dynamics. It should come back in discussions with references and comparison to other models.

Response:

The reviewer asks that we put more emphasis on the Bothnian Bay in the Discussion section of the paper.

Indeed the case of the Bothnian Bay is complex. We have a long list of differences to other Baltic Sea basins:

- regularly ice covered
- oligothrophic, as river inputs are pristine
- P-limited rather than N-limited
- permanently oxic sediments
- low salinity means little S concentration
- Fe does therefore not easily form FeS, the point at which sedimentary bound P would typically be released
- so we get permanent burial of Fe-bound P, partly as vivianite

This is why most Baltic Sea models struggle in representing the Bothnian Bay N and P dynamics. In a BGC model intercomparison, Meier et al. (2018) found that at station F9 only 10 of 29 ensemble members reached acceptable skill for DIN and only 7 for DIP.

Our ERGOM model includes a latitude-dependent P retention efficiency during sedimentary OM recycling, increasing the amount of P kept in Bothnian Bay sediments. This is a parameterization for the unresolved iron cycle: Iron concentrations relative to phosphate are important for the P binding efficiency of sediments, but iron loads of Baltic Sea rivers are poorly constrained, which is why we did not include an explicit iron cycle in ERGOM.

We did already discuss the Bothnian Bay case in Lines 552-567, but without referring to the difficulties in modelling, and the background situation. Now we add both of that, but for model intercomparison, we rather just refer to an existing model intercomparison study. Doing a targeted model intercomparison, mechanistically looking at the Bothnian Bay and its reactions to coastal parameter changes in different models, would be exciting but is beyond the scope of this article.

Changes to manuscript:

Before line 552, we add:

The Bothnian Bay is known to differ from other Baltic Sea basins in its specifics: Pristine rivers deliver small amounts of N and P only, but large amounts of Fe. This leads to an oligotrophic situation that keeps the sediments permanently oxic. The rather small concentrations of sulfate, controlled by the low salinity, lead to slower sulfate reduction even deeper in the sediments. The conversion of iron oxides to iron sulfides (FeS or FeS$_2$) slows down, and Fe-bound P can be permanently buried, partly as vivianite. This leads to a P-limited, rather than N-limited, situation. This difference to other basins leads to difficulties for biogeochemical models targeted at the whole Baltic Sea: A BGC model intercomparison by Meier et al. (2018) for the Baltic Sea showed found that at station F9 only 10 of 29 ensemble members reached acceptable skill for DIN and only 7 for DIP.Our ERGOM model addresses this difference by a latitude-dependent P-binding efficiency which is enhanced in the Bothnian Sea and again in the Bothnian Bay. This is a parameterization for the unresolved iron cycle in the model, as the ratio between Fe and P is larger in the Bay of Bothnia river loads.

Section 3.4 – general

Comment: The presentation of the results would benefit from reordering to match the sequence of the figures, or alternatively, the figures could be rearranged to follow the order of the text. Additionally, the text should provide more explicit references to the specific experiment(s) being discussed to guide the reader and improve clarity. While I have highlighted some examoles below, but careful attention should be paid to this throughout the entire Results section.

Response: We agree that explicitly linking the described responses to the corresponding sensitivity experiments improves the clarity and readability of the Results section. Therefore, we have thoroughly revised Section 3.4 and reorganized the presentation of results to generally follow the sequence of the figures (Figure 6 to Figure 10), starting with nitrate (NO_3), followed by ammonium (NH_4), phosphate (PO_4), oxygen (O_2), and phytoplankton responses. In some cases, cross-referencing previous figures was retained where necessary to explain mechanistic links between different ecosystem variables (for example, discussing the relationship between NH_4 accumulation and NO_3 depletion), as these connections are important for interpreting the biogeochemical responses. In addition, throughout the revised section, we have ensured that each sensitivity response is explicitly linked to the corresponding experiment. Whenever a new experiment is introduced or discussed, the experiment name is provided in parentheses

Changes to manuscript: The entire Section 3.4 has been rewritten to improve the logical flow and to follow the order of the corresponding figure panels. Additionally, experiment names have been added throughout the section wherever individual sensitivity simulations are discussed. For example:

Line 357: revised from “column nitrate concentrations respond similarly to both coastal and basin wide enhancement in sediment detritus recycling” to “column NO3 concentrations respond similarly to bothcoastal (C1_DetSed) and basin wide (E1_DetSed) enhancement in sediment detritus recycling.”

Line 362: revised from “Basin wide enhanced sediment recycling leads to an increase in water column NH4” to “Basin wide enhanced sediment detritus recycling experiment (E1_DetSed) leads to an increase in water column NH4”

Figure 6 (and 7–10)

Comment: In caption, remove “Entire”. In figure, experiment names should be the same as those in Table 2 (E1, E2…etc). Also valid for figures 7, 8, 9 and 10.

Response: We have revised the figure caption accordingly. The word “Entire” has been removed from the caption, and the experiment names have been updated to match the nomenclature used throughout the revised manuscript

Changes to manuscript: “Entire” was removed from the captions of Figures 6–10, and the figure panel labels were relabeled to the new experiment names (C1_DetSed–C4_NitWat and E1_DetSed–E4_NitWat). Figure 6 caption revised to “Figure 6. Depth integrated and annually averaged difference in water column nitrate concentration (NO3, µmol N m−2) between the sensitivity and base simulations. Red and blue colors indicate an increase and decrease in N O3 relative to the base simulation, respectively. The top row shows results from the coastal only sensitivity ex-

periment (C1_DetSed to C4_NitWat, area weighted), while the bottom panels represent the entire Baltic Sea experiment (E1_DetSed to E4_NitWat)."

The same modifications applied to the captions of Figure7 to 10 to ensure consistent terminology

Line 363

Comment: Keep figure order as mentioned in the text (7e is mentioned before 6, it should then be 6).

Response: We have reordered the figure citations throughout the Results section so that they follow the sequence in which they are discussed in the text wherever possible. This improves consistency between the narrative and the figure references. For the specific sentence highlighted by the reviewer, Figure 6 is now cited before Figure 7. In some cases, however, cross-references to previously cited figures were intentionally retained where they are necessary to explain mechanistic relationships between different ecosystem variables

Changes to manuscript: The entire Section 3.4 has been rewritten to improve the logical flow and to follow the order of the corresponding figure panels.

Specific to this comment, the sentence revised from "The accumulation of NH4 (Figure 7e) coincides with a decline in NO3 (Figure 6e) along the anoxic band of the Gotland Basin. " to "The decline in NO3 (Figure 6e) coincides with accumulation of N H4 (Figure 7e) along the anoxic band of the Gotland basin"

Line 369

Comment: I would describe everything related to NO3 before mentioning primary productivity and this sentence is confusing, is it for all experiments or only for coastal ones. Also, if mentioning figure 10 before 8 and 9, Figure 10 should be 8. Please be more specific when describing the results and improve the structure following the correct order of the figures.

Response: We agree that the organization of the results section can be improved by presenting the responses in the same sequence as the figures and by explicitly linking each described response to the corresponding sensitivity experiments. Therefore, we have thoroughly revised Section 3.4 and reorganized the presentation of results to generally follow the order of Figures 6–10. The revised section now discusses nitrate (NO_3) responses first (Figure 6), followed by ammonium (NH_4; Figure 7), phosphate (PO_4; Figure 8), oxygen (O_2; Figure 9), and phytoplankton responses (Figure 10). In addition, we have ensured that each response is explicitly associated with the corresponding sensitivity experiment. Whenever an experiment is introduced or discussed, the full experiment name (e.g., C1_DetSed, E1_DetSed, C2_DetWat, E2_DetWat) is provided in parentheses to improve clarity. The experiment names have also been added directly to the figure panels (corresponding to panels a–h) to make it easier for readers to identify the sensitivity experiments represented in each figure. The sentence describing primary production has also been revised to explicitly indicate that it refers to the coastal sediment recycling experiment (C1_DetSed; Figure 10a)

Changes to manuscript: Section 3.4 has been substantially revised. The discussion was reorganized to follow the figure sequence (Figures 6–10), beginning with NO_3, followed by NH_4, PO_4, O_2, and phytoplankton responses. The corresponding sensitivity experiment names were added throughout the section to clarify which experiments are being discussed.

The sentence revised from "Figure 10a indicates a decline in primary production in the northern basins (Bothnian Sea and Bothnian Bay), with only a slight increase in the other deeper basins." to " In the coastal sediment recycling experiment (C1_DetSed; Figure 10a), primary production declines in the northern basins (Bothnian Sea and Bothnian Bay), with only a slight increase in the other deeper basins"

Line 372

Comment: Please convert all your units to something more conventional. HELCOM uses for example, µmol L-1 for nutrients, µg L-1 for chlorophyll and mg L-1 for oxygen. Your units are difficult to compare with other studies—are they really mol x meters per kg? These are odd units for concentrations.

Response: The variables analyzed in this study are depth-integrated water column quantities derived from model output, rather than point concentrations. Therefore, the resulting units represent areal inventories. To make the results more comparable and easier to interpret, we have converted the units into more conventional depth-integrated forms commonly used for biogeochemical analyses.

Changes to manuscript: The units in the figure captions were revised to conventional area-integrated units, e.g. Figure 6 nitrate to "µmol N m^-2", Figure 8 phosphate to "µmol P m^-2", Figure 9 oxygen to "µmol O m^-2", and Figure 10 phytoplankton to "mg C m^-2" (replacing "mol.m/kg"). Accordingly, all corresponding values and units in the text have been updated, and Figures 6–10 have been replotted with the revised units and updated values.

Figure 8

Comment: In caption, Are the units of PO4 in P?

Response: Yes the units are in the units of P

Changes to manuscript: The Figure 8 caption units were revised to "µmol P m^-2".

Figure 9

Comment: In caption, it says nitrate but should be oxygen. Please correct.

Response: Thank you for pointing out and revised the same

Changes to manuscript: The Figure 9 caption corrected from "nitrate" to "oxygen" concentration (O2), with units "µmol O m^-2".

Figure 10

Comment: Are the units correct in the caption?

Response: We thank the reviewer for pointing out and the units are now updated to "mg C m^-2"

Changes to manuscript: The Figure 10 caption units revised to "mg C m^-2".

Discussion

General comment

Comment: As noted previously, this section would benefit from further development. The Discussion has strong potential but currently reads more like an extension of the Results. To fully realize its value, It should provide concrete and robust interpretations of the interesting results presented, clearly highlighting their contribution to scientific knowledge and how they compare with previous studies—both modeling and field-based. Some of the content currently included could be integrated into the Results section, and few statements would benefit from greater specificity.

I have added some targeted comments as guidance, but the section would benefit from a thorough rewrite. In particular, emphasizing comparisons with other models and observational studies, and discussing how these findings can inform the modeling community and improve understanding of Baltic Sea biogeochemistry, would significantly strengthen the manuscript. With these additions, the Discussion could fully showcase the relevance and impact of your work for the scientific community.

In addition, I recommend including the following citations (though not limited to these) in the

Discussion, as they represent some of the most recent modeling studies for the Baltic Sea, to

provide context and allow for meaningful comparisons.

Lignell et al 2025: https://doi.org/10.1016/j.jmarsys.2025.104049

Muller-Karulis et al., 2024: https://doi.org/10.1016/j.jenvman.2024.122478

Ruvalcaba Baroni et al., 2024: https://doi.org/10.5194/bg-21-2087-2024

Neumann et al., 2025: https://doi.org/10.5194/egusphere-2024-3734

Samlas et al., 2025: https://doi.org/10.1016/j.marpolbul.2025.117975

See also copernicus products and how these could benefit from your study would also be useful:
https://data.marine.copernicus.eu/product/BALTICSEA_ANALYSISFORECAST_BGC_003_007/description?utm_source=chatgpt.com

Response: Thank you for this constructive comment.

Changes to manuscript: Section 4 (Discussion) has been substantially restructured. Repetitive descriptions of the Results and methodology were removed, and additional discussion and references were added to compare our findings with recent Baltic Sea modelling and observational studies, including those suggested by the reviewer. The implications for model parameterization, coastal processes, and regional Baltic Sea modelling and monitoring were also expanded.

Line 407

Comment: This sentence is too general, maybe remove and start first by cases where NH4 has decreased followed by cases where NH4 has increased, or give more information regarding why this is relevant.

Response: We have revised the paragraph to begin with the scientific interpretation of the observed NH_4 response, emphasizing that the spatial variability in NH_4 concentrations reflects differences in remineralization pathways and oxygen availability across the Baltic Sea basins. The discussion now directly links enhanced temperature-dependent remineralization to NH_4 accumulation in the anoxic Gotland Basins and contrasts this with the weaker NH_4 response in the more oxygenated basins, where coupled nitrification–denitrification limits NH_4 accumulation. Rather than restructuring the discussion according to decreases and increases in NH_4, we chose to reorganize the paragraph around the underlying biogeochemical mechanisms, emphasizing the contrasting responses between anoxic and oxic basins. We believe this structure better highlights the processes controlling the observed NH_4 dynamics

Changes to manuscript: The opening of this discussion paragraph has been revised to improve the scientific interpretation of the results. The original introductory sentence,
"As expected, the basin-wide enhancement of temperature-sensitive process rates had a substantial influence on the biogeochemical cycles of the Baltic Sea and its deep basins (Neumann, 2010; Meier et al., 2011),"

has been removed and replaced with the following

"The response of water column NH_4 concentrations to enhanced temperature sensitivity of biogeochemical prcesses varied among Baltic Sea basins, reflecting differences in remineralization pathways and oxygen availability."

The remainder of the paragraph has been retained to explain the mechanisms responsible for the contrasting NH_4 responses observed between anoxic and oxic Baltic Sea basins.

Line 411

Comment: This is based on your results, why add a citation here? Are you comparing results? In that case be more specific.

Response: We agree with the reviewer that the original wording did not clearly distinguish between our model results and the literature context. We have revised the sentence to clarify that the observed NH_4 accumulation in the Gotland Basin is a result of our sensitivity experiments, while the cited study is used only to support the underlying nitrogen cycling mechanism under oxygen-depleted conditions.

Changes to manuscript: The text was revised to make the comparison explicit: the sentence has been revised from “This accumulation was most pronounced in the anoxic basins of the Baltic Sea, particularly in the Gotland Basin (Kulinski et al., 2022)” to “This accumulation was most pronounced in the anoxic basins of the Baltic Sea, particularly in the Gotland Basins, which is in agreement with the nitrogen cycling mechanism described by Kuli´nski et al., 2022. ”

Line 412

Comment: For all cases?

Response: The weaker NH_4 accumulation in the more oxic regions is observed specifically in the basin-wide enhanced detritus recycling experiments (E1_DetSed and E2_DetWat), where NH_4 accumulation is substantially smaller than in the anoxic Gotland Basins. This statement does not apply to all sensitivity experiments; E3_DocSed shows only negligible NH_4 changes, while E4_NitWat results in a decrease in NH_4 due to enhanced nitrification. To avoid overgeneralization, we have revised the manuscript to explicitly refer to the E1 and E2 experiments.

Changes to manuscript: The sentence have been changed from "In contrast, ´ NH4 accumulation was weaker in the more oxic regions, such as the Gulf of Riga, Bothnian Bay, and Bothnian Sea" to "In contrast, during the enhanced detritus recycling experiments (E1_DetSed and E2_DetSed) NH4 accumulation was weaker in the more oxic regions, such as the Gulf of Riga, Bothnian Bay, and Bothnian Sea compared to the anoxic Gotland basins. "

Line 419

Comment: This is too generic if referring to denitrifying bacteria. Either mention the correct name of such bacteria or mention the process alone.

Response: Since our study focuses on ecosystem-scale biogeochemical processes and does not resolve microbial communities, we have revised the text to describe denitrification as a biogeochemical process rather than referring to specific organisms

Changes to manuscript: The sentence have been revised from "This can be explained as a combination of high temperature-driven remineralization rates and the anoxia in the basin, which inhibits nitrification (a process that is a sink for NH4) and favors the anaerobic pathway, such as the denitrification process (a process that is a sink for NO3)."

To

"This can be explained as a combination of high temperature-driven remineralization rates and the anoxia in the basin, which inhibits nitrification (a process that is a sink for NH4) and favors the denitrification process (a process that is a sink for NO3). "

Line 421

Comment: Add references for this mechanism.

Response: two references added to support the mechanism, Jäntti et al., 2011 describes the nitrogen cycle of the Baltic and and Jäntti et al., 2012 supports that the low oxygen conditions favours nitrogen reduction pathways

Changes to manuscript: A refernce added to the manuscript as follows

".. while facilitating the production of N H4 (Jäntti et al., 2011; Jäntti and Hietanen, 2012). "

Jäntti, H., Stange, F., Leskinen, E., & Hietanen, S. (2011). Seasonal variation in nitrification and nitrate-reduction pathways in coastal sediments in the Gulf of Finland, Baltic Sea. *Aquatic Microbial Ecology*, *63*, 171-181.

Jäntti, H., & Hietanen, S. (2012). The effects of hypoxia on sediment nitrogen cycling in the Baltic Sea. *Ambio*, *41*(2), 161-169.

Line 422

Comment: This is the consequence of the "chosen" parametrisation in your model, it is not really a result "supporting" a process, since the parametrization is based on what we know about how the process works. The sentence should be rephrased as it is misleading.

Response: We agree that the previous wording was misleading, as the simulated NH_4 increase and NO_3 decline should not be interpreted as independent evidence supporting the underlying nitrogen cycling mechanisms. Instead, these

responses represent the expected outcome of the implemented biogeochemical parameterizations under oxygen-limited conditions.

Changes to manuscript: The sentence was rephrased to avoid "support" from "The simultaneous enhancement of NH4 and the decline in NO3 concentration in the anoxic basins of the Baltic Sea support this mechanism."

to

"The simultaneous enhancement of NH4 and the decline in NO3 concentration in the anoxic basins of the Baltic Sea are consistent with the expected effects of enhanced remineralization, and reduced nitrification under oxygen-limited conditions"

Line 426

Comment: Similar comment as above, here "support" may not be the correct wording.

Response: We agree that the term "support" could imply that the model results independently demonstrate the occurrence of nitrification–denitrification processes, whereas these processes are already represented within the model formulation. We have revised the wording to indicate that the simulated NH_4 response is consistent with the expected behaviour of nitrogen cycling processes under oxygenated conditions.

Changes to manuscript: The sentence have been revised from "Here, the oxygen-rich environment can support the coupled nitrification and denitrification, .."

to

"Here, the oxygen-rich environment can favours the coupled nitrification and denitrification, ..."

Line 441

Comment: Why not plot denitrification maps in the same way as Figure 8-10? This will show what you are writing here, giving a tangible proof (instead of using "may"). Also valid for line 449.

Response: Thank you for this helpful suggestion. We agree that showing the spatial distribution of denitrification provides more direct evidence for the interpretation of the NO3 response. We have therefore added the denitrification response as a supplementary figure, using the same approach as for Figures 8–10.

Changes to manuscript: Figure added to suppliment

Line 451

Comment: Understanding the statement "stronger stratification and anoxia conditions" is not straightforward, please explain how stratification can lead to such results and how anoxia, if no big changes are seen in oxygen concentrations in such area, influences this. Please also add a reference.

Response: We have revised the text to clarify that persistent stratification in the central Baltic basins reduces vertical oxygen replenishment and maintains oxygen-depleted conditions, which influence redox-sensitive nitrogen transformation pathways. We have also clarified that the relatively small changes in basin-averaged O_2 concentration in the sensitivity experiments do not imply an absence of oxygen limitation, because the Gotland Basin is already characterized by pre-existing low-oxygen conditions. Therefore, even small changes in oxygen availability can affect nitrification efficiency and the balance between nitrogen transformation pathways. Additionally, references describing the relationship between hypoxia, stratification, and nitrogen cycling in the Baltic Sea have been added (Hietanen et al., 2012; Jäntti and Hietanen, 2012).

Changes to manuscript:

The sentences have been revised from "In contrast, the Gotland basins show a similar decline in NO3 as in E1, with a significant accumulation of NH4 and decline of NO3, suggesting that in the central stratified anoxic basins, the

biogeochemical cycling is characterised by stronger stratification and anoxic conditions, which amplify the NO3 loss through denitrification."

To

"In contrast, the Gotland basins show a similar decline in NO3 as in E1_DetSed with a significant accumulation of NH4 and decline of NO3. This response is consistent with the influence of the strongly stratified and oxygen-depleted conditions that characterize the central Baltic basins. Persistent stratification restricts vertical oxygen replenishment, maintaining redox conditions favourable for anaerobic nitrogen transformation pathways, including denitrification (Hietanen et al., 2012; Jäntti and Hietanen, 2012). Although the sensitivity experiments show only minor changes in the basin-averaged O2 concentration, the pre-existing low-oxygen conditions in the Gotland Basin limit nitrification and promote nitrate consumption through anaerobic processes"

The references added to the text are

Hietanen, S., Jäntti, H., Buizert, C., Jürgens, K., Labrenz, M., Voss, M., & Kuparinen, J. (2012). Hypoxia and nitrogen processing in the Baltic Sea water column. *Limnology and Oceanography*, *57*(1), 325-337.

Jäntti, H., & Hietanen, S. (2012). The effects of hypoxia on sediment nitrogen cycling in the Baltic Sea. *Ambio*, *41*(2), 161-169.

Line 457

Comment: Which one is more realistic between E1 and E2 and between C1 and C2? Can you add discussion on this and base it on literature?

Response: We thank the reviewer for this valuable comment. We agree that further discussion is needed to explain the contrasting NO3 responses between the E1_DetSed and E2_DetWat experiments and to clarify the underlying mechanisms. The opposite responses observed in the Gulf of Riga and Bothnian Bay are not related to one experiment being more realistic than the other, but rather reflect the different pathways through which enhanced remineralization affects nitrogen cycling depending on its location. Enhanced sediment detritus recycling modifies benthic remineralization and sediment–water nutrient exchange, whereas enhanced water-column detritus recycling directly affects pelagic nutrient regeneration and subsequent nitrification processes under oxic conditions. Therefore, the contrasting responses highlight the importance of the spatial location of remineralization processes and their interaction with basin-specific environmental conditions, such as oxygen availability and sediment–water coupling.

We have added a new paragraph in the Discussion section to explain the mechanisms responsible for the opposite NO3 responses between sediment and water-column detritus recycling experiments and included relevant references describing nitrogen transformation pathways in Baltic Sea sediments and water columns (Hietanen et al., 2012; Jäntti and Hietanen, 2012).

Changes to manuscript: A new paragraph added to the manuscript after Line 457 as follows

"The contrasting NO3 responses between E1_DetSed and E2_DetWat in the Gulf of Riga and Bothnian Bay indicate that the location of enhanced remineralization strongly influences the pathway through which regenerated nitrogen is processed. Enhanced sediment detritus recycling (E1_DetSed) increases benthic remineralization and modifies sediment–water nutrient exchange, where regenerated NH4 may be subjected to sedimentary nitrification and nitrate reduction processes. In contrast, enhanced water-column detritus recycling (E2_DetWat) directly increases pelagic regeneration of NH4, which can subsequently be converted to NO3 through nitrification under oxic conditions. Therefore, sediment and water-column remineralization represent distinct controls on nitrogen availability, and their relative importance depends on basin-specific environmental conditions, particularly oxygen availability and sediment–water interactions (Hietanen et al., 2012; Jäntti and Hietanen, 2012)"

Lines 459–462

Comment: I think the relevance of experiment E3 and C3 should be more detailed and highlighted, maybe in this paragraph, otherwise I fail to see its importance. For example, is increased DOC remineralization due to temperature

more realistic than that of detritus in sediments? In other words would DOC be easier to remeneralized than al, detritus particles (some of which may take longer time to decomposed) Why is this experiment important? Why did you choose it? Of course the motivation for this experiments should not come up in the discussions, but introduction or methods

Response: We agree that the motivation for including the E3_DocSed and C3_DocSed experiments should be made clearer. These experiments were not intended to imply that DOC remineralization is more important or more realistic than detritus remineralization. Rather, they were included because sediment DOC remineralization is one of the temperature-dependent biogeochemical processes parameterized by a q10 coefficient in ERGOM. To clarify this, we have added a statement to the Methods section explaining the rationale for selecting the four investigated processes. We have also expanded the Discussion to highlight that, although E3_DocSed represents an important temperature-dependent pathway included in the model, its comparatively weak sensitivity indicates that uncertainties in the temperature dependence of sediment DOC remineralization have a smaller influence on basin-scale phosphorus cycling than uncertainties associated with temperature-dependent detritus remineralization within the current ERGOM formulation.

Changes to manuscript: A paragraph added in Line 203 after ".. given in Neumann et al. (2022))" as "These processes were selected because they represent key temperature-sensitive pathways controlling organic matter remineralization and nutrient cycling in ERGOM. In particular, they include the major pathways of organic matter degradation (detritus and DOC recycling) and nitrogen transformation (nitrification), allowing us to assess how uncertainties in their temperature sensitivity influence Baltic Sea biogeochemical cycling."

A sentence added after Line 462 as follows "In contrast, the response to enhanced temperature sensitivity of sediment DOC remineralization (E3_DocSed) was comparatively small across all basins. This suggests that, within the current ERGOM formulation, uncertainties in the temperature sensitivity of sediment DOC remineralization exert a smaller influence on basin-scale phosphorus cycling than uncertainties associated with detritus remineralization, despite representing an important temperature-dependent remineralization pathway included in the model."

Figure 11

Comment: Nice figure, but why are the little blue and yellow fishes upside down? Are they dead? Also, the arrows and pools are a bit messy, as they do not follow a scale in sizes. Please make the arrows and pools indicating the same thing (e.g., "smaller" or "larger") the same size and shape. It will make the figure more readable.

Response: We have revised Figure 11 by standardizing the size and shape of arrows and pools representing similar processes and responses. We have also corrected the orientation of the fish

Changes to manuscript: The Figure 11 has been revised accordingly.

Line 478

Comment: But to me this scenario seems more likely, or? Please explain more on how realistic each scenario is and based on literature.

Response: We have added a clarification in the Discussion explaining why the response of E4_NitWat is weaker compared with the detritus recycling experiments. Although nitrification is a temperature-sensitive process, its influence on basin-scale nutrient dynamics is strongly constrained by oxygen and ammonium availability. In the Baltic Sea, nitrification is primarily restricted to oxic regions and oxic–anoxic transition zones, while large parts of the deep basins experience persistent oxygen limitation. Therefore, the limited response in E4_NitWat reflects the spatial and environmental constraints on nitrification rather than indicating that this process is unimportant. In contrast, detritus recycling represents a broader remineralization pathway that operates across both oxic and oxygen-depleted environments and directly regulates the regeneration of inorganic nutrients.

Changes to manuscript: New sentences added to the mauscript at line as follows

"Although nitrification is a temperature-sensitive process, its response to warming is strongly regulated by oxygen availability (Jäntti et al., 2018) and substrate availability. In the Baltic Sea, nitrification is mainly restricted to oxic

water layers and sediments, while large regions of the deep basins experience persistent oxygen limitation (Hietanen et al., 2012; Jäntti and Hietanen, 2012; Carstensen et al., 2014b). Therefore, the limited basin-scale nutrient response in E4_NitWat reflects the spatial constraint of nitrification rather than indicating that the process is unimportant. In contrast, detritus recycling affects a broader range of environments because organic matter remineralization occurs throughout both oxic and oxygen-depleted conditions (Rolff et al., 2022; Carstensen et al., 2014b)."

The following references added to the text as follows,

Jäntti, H. and Hietanen, S.: The effects of hypoxia on sediment nitrogen cycling in the Baltic Sea, Ambio, 41, 161–169, 2012.

Hietanen, S., Jäntti, H., Buizert, C., Jürgens, K., Labrenz, M., Voss, M., and Kuparinen, J.: Hypoxia and nitrogen processing in the Baltic860

Sea water column, Limnology and Oceanography, 57, 325–337, 2012

Rolff, C., Walve, J., Larsson, U., & Elmgren, R. (2022). How oxygen deficiency in the Baltic Sea proper has spread and worsened: The role of ammonium and hydrogen sulphide. *Ambio*, *51*(11), 2308-2324.

Jäntti, H., Ward, B. B., Dippner, J. W., & Hietanen, S. (2018). Nitrification and the ammonia-oxidizing communities in the central Baltic Sea water column. *Estuarine, Coastal and Shelf Science*, *202*, 280-289.

Carstensen, J., Conley, D. J., Bonsdorff, E., Gustafsson, B. G., Hietanen, S., Janas, U., ... & Voss, M. (2014). Hypoxia in the Baltic Sea: biogeochemical cycles, benthic fauna, and management. *Ambio*, *43*(1), 26-36.

Line 492

Comment: Is the increase in phytoplankton large or significant in E1, E2 and C1, C2? This is not necessarily the same. Maybe for this plot, plotting percentages will be better. How does it compare to the real world? Primary productivity is an important topic in the Baltic Sea, so more focus on phytoplankton should be given.

Response: We have revised this section to better highlight the magnitude and spatial variability of the phytoplankton response among the sensitivity experiments. The phytoplankton response shown in the figure is already presented as relative percentage changes, calculated using the sensitivity metric (S, Equation 3), which represents the percentage difference between each sensitivity experiment and the baseline simulation. To make this clearer, we have added the approximate range of phytoplankton responses in the revised manuscript. The results show that the positive phytoplankton response is mainly associated with E1_DetSed and E2_DetWat, with increases of approximately 0.8–4.0% and 1.6–6.8%, respectively, whereas E3_DocSed and E4_NitWat produce negligible changes (<0.3%). We have also added a discussion explaining that the stronger response in E2_DetWat is related to the direct availability of regenerated nutrients in the water column, where they can more immediately support phytoplankton growth. Furthermore, we have clarified that these results represent the sensitivity of phytoplankton biomass to individual biogeochemical process parameterizations rather than direct projections of future primary production.

Changes to manuscript: The sentences have been replaced from "The phytoplankton, on the other hand, shows a basin-wide increase in all cases, indicating that the enhanced process rates in the entire Baltic Sea enhance the nutrient cycling and nutrient availability, which promotes phytoplankton growth. The negligible response in E3 and E4 implies that the processes altered in these scenarios are less critical for both O2 and phytoplankton dynamics."

To

"Phytoplankton biomass showed a positive response primarily in E1_DetSed and E2_DetWat, with increases of approximately 0.8–4.0% and 1.6–6.8% across the Baltic Sea basins, respectively. The stronger response in E2_DetWat indicates that enhanced water-column detritus recycling directly increases pelagic nutrient regeneration, thereby providing a more immediate nutrient source for phytoplankton growth. In contrast, the response to enhanced sediment detritus recycling was weaker compare to that of enhanced water column detritus recycling, but still consistent across most basins. The negligible response in E3_DocSed and E4_NitWat (<0.3%) suggests that uncertainties in DOC recycling and nitrification temperature sensitivity have a limited influence on basin-scale phytoplankton biomass

within the current model formulation. These responses should be interpreted as sensitivity to individual process parameterizations rather than direct projections of future primary production, as Baltic Sea phytoplankton dynamics are additionally controlled by light availability, grazing and physical conditions (Wasmund et al., 1998; Meier et al., 2019a).

The references added to the text are as follows:

Wasmund, N., Nausch, G., & Matthäus, W. (1998). Phytoplankton spring blooms in the southern Baltic Sea—spatio-temporal development and long-term trends. *Journal of plankton research*, *20*(6), 1099-1117.

Meier, H. M., Dieterich, C., Eilola, K., Gröger, M., Höglund, A., Radtke, H., ... & Wåhlström, I. (2019). Future projections of record-breaking sea surface temperature and cyanobacteria bloom events in the Baltic Sea. *Ambio*, *48*(11), 1362-1376.

Line 478–492

Comment: Somewhere in this paragraph (or elsewhere in the discussion), it should be mentioned that even if not analysed specifically in these experiments, oxygen is less soluble at high temperatures, and will thus likely further affect most of your results (explain in which way).

Response: We thank the reviewer for highlighting this important aspect. We agree that warming-induced changes in oxygen solubility represent an additional mechanism that can influence oxygen availability and subsequently affect biogeochemical cycling. The sensitivity experiments presented in this study were designed to isolate the effects of changes in the temperature sensitivity of selected biogeochemical processes through q10 perturbations; therefore, they do not explicitly account for the direct effect of increasing temperature on oxygen solubility. We have added a clarification in the Discussion section stating that warmer waters have a lower oxygen-holding capacity, which may contribute to future oxygen decline independently of changes in biological oxygen consumption. We further discuss that reduced oxygen availability could amplify the responses observed in our sensitivity experiments by expanding oxygen-limited conditions, reducing nitrification, and enhancing anaerobic nitrogen transformation pathways. Thus, the combined effects of warming-induced changes in process rates and reduced oxygen solubility may result in stronger impacts on nutrient cycling than estimated from q10 sensitivity alone.

Changes to manuscript: A new paragraph has been added to the Discussion section following the discussion of oxygen responses at Line 490 as follows

"However, our experiments isolate the effect of changes in temperature sensitivity of biogeochemical processes and do not explicitly account for the direct effect of warming on oxygen solubility. Since oxygen solubility decreases with increasing temperature, future warming may reduce dissolved oxygen concentrations (Breitburg et al., 2018; Schmidtko et al., 2017) independently of changes in biological oxygen consumption. Such a decline in oxygen availability could further intensify the biogeochemical responses observed in our sensitivity experiments by expanding oxygen-limited conditions, reducing nitrification, and enhancing the importance of anaerobic nitrogen transformation path ways. Therefore, the combined effects of warming-induced changes in process rates and reduced oxygen solubility may lead to stronger impacts on nutrient cycling than estimated from q10 sensitivity alone"

Additional references added to the manuscript are as follows:

Breitburg, D., Levin, L. A., Oschlies, A., Grégoire, M., Chavez, F. P., Conley, D. J., ... & Zhang, J. (2018). Declining oxygen in the global ocean and coastal waters. *Science*, *359*(6371), eaam7240.

Schmidtko, S., Stramma, L., & Visbeck, M. (2017). Decline in global oceanic oxygen content during the past five decades. *Nature*, *542*(7641), 335-339.

Line 499

Comment: "Here", where? Entire Baltic Sea, its coastal waters in this experiment, in this study, ...?

Response: We agree that the reference of "Here" was unclear and could lead to confusion regarding the spatial scale being discussed. We have revised the sentence to explicitly indicate that the statement refers to the response of the deep Baltic Sea basins analyzed in the coastal sensitivity experiments.

Changes to manuscript: The sentence have been revised from "Here, the absolute changes on biogeochemical cycles appear minor due to the small area of the coastal zone compared to the entire Baltic Basin."

To

"At the scale of the deep Baltic basins analyzed here, the absolute changes in biogeochemical variables appear minor because the coastal zone where the parameter perturbation was applied represents only a small fraction of the entire Baltic Sea area. "

Lines 503–504

Comment: I have a problem the way it is described. The coastal process do not change because you normalise it with the area. Their impact on the basin remains the same. It may become more visible. Is that what you mean? Please rephrase and explain better. Also, has this been done before, can you refer to similar work (may be in other basins and not necessarily through modelling)? The entire paragraph lacks references and comparison to previous work.

Response: We agree that the coastal processes themselves do not change as a result of the area normalization. The purpose of the normalization is not to modify or amplify the physical response of the coastal processes, but rather to account for the difference in spatial extent between the coastal and basin-wide perturbations and to provide a comparable measure of their relative influence on basin-scale biogeochemical responses. We have revised the text accordingly to avoid implying that the normalization increases the actual coastal impact.

In addition, the relative coastal sensitivity metric (Scr) introduced in this study is a new diagnostic approach developed to compare the sensitivity of parameter perturbations applied over spatial domains with different extents (coastal versus entire Baltic Sea). The motivation for developing this metric is based on previous studies demonstrating that coastal regions, despite their limited spatial coverage, can have a disproportionate influence on marine biogeochemical cycling through enhanced nutrient transformation, retention, and exchange processes with adjacent offshore waters. We have revised the text to distinguish between the motivation provided by previous studies and the new methodology introduced in this study.

Changes to manuscript:

The sentence "In order to understand the real potential of coastal changes on the basin scale, we have normalized the coastal sensitivity with the area; then the coastal influence becomes more significant and comparable with that of the entire Baltic Sea experiments"

revised to

"In order to understand the real potential of coastal changes on the basin scale, we have normalized the coastal sensitivity with the area ratio; this accounts for the difference in spatial extent between the coastal and entire Baltic Sea perturbations and allows a direct comparison with the basin-wide sensitivity experiments. This approach is motivated by previous studies showing that coastal regions, despite their limited spatial extent, can play a disproportionate role in marine biogeochemical cycling through intense nutrient transformation, retention, and exchange processes with adjacent offshore waters (Mathis et al., 2024; Asmala et al., 2017)."

The references added are as follows

Mathis, M., Lacroix, F., Hagemann, S., Nielsen, D. M., Ilyina, T., & Schrum, C. (2024). Enhanced CO2 uptake of the coastal ocean is dominated by biological carbon fixation. *Nature Climate Change*, *14*(4), 373-379.

Asmala, E., Carstensen, J., Conley, D. J., Slomp, C. P., Stadmark, J., & Voss, M. (2017). Efficiency of the coastal filter: Nitrogen and phosphorus removal in the Baltic Sea. *Limnology and Oceanography*, *62*(S1), S222-S238.

Lines 511–512

Comment: "Here, it would not make much ...specifically". Why? Do you mean that the focus is on the impact that coastal zone exerts in the deep basins and not in the coast itself? This sentence is not very clear. Please rephrase it to for example: "Because our focus is on the impact of the coastal processes on deep basins, we do not assess …"

Response: We agree that our intended meaning was not that evaluating coastal parameters themselves is unimportant, but rather that the focus of the relative coastal sensitivity (Scr) metric is to assess the influence of coastal processes on the deep-basin biogeochemistry. We have therefore revised the sentence to clarify that when (Scr) values are close to zero, the corresponding coastal parameter perturbations have limited influence on the deep basins analyzed in this study.

Changes to manuscript: The sentence " Here, it would not make much sense to assess this parameter in the coastal zone specifically" have been revised to "Because our focus is on the influence of coastal processes on deep-basin biogeochemistry, such parameters are less relevant for explaining the variability in the deepbasinsanalyzed in our study. "

Line 512–513

Comment: "It rather ...substantially." Please be more specific. Also, when referring to the "system", is it the entire Baltic Sea or coastal waters? Please specify (here and through the text).

Response: We now specify that a (Scr) value close to zero indicates that perturbations of the q10 parameter confined to the coastal zone produce little or no detectable response in the five key biogeochemical variables (nutrients, O_2, and phytoplankton) in the deep Baltic Sea basins. Consequently, constraining these parameters in the coastal zone would have a limited impact on reducing uncertainties in the simulated deep-basin biogeochemistry, whereas constraining them in the open Baltic basins would be more relevant for these deep-basin variables.

In addition, the word "system" replaced with appropriate word through out the manuscript, and the specific changes applied are given below in the section "changes to manuscript"

Changes to manuscript: The sentence "It rather means that confining this parameter in the open sea will reduce model uncertainty more substantially" revised to "This indicates that, for reducing uncertainties in simulated deep-basin biogeochemical variables, constraining these parameters in the coastal zone would have a limited impact, whereas constraining them in the open Baltic basins would be more relevant."

The "system" replaced throughout the text as follows

Line 400: system replaced with "Baltic Sea"

Line 402: system replaced with "Baltic Sea

Line 461: system replaced with Baltic Sea

Line 513: system replaced with "deep Baltic Sea basin"

Line 519: system replaced with deep Baltic Sea basins

Line 522: system replaced with deep baltic sea basins

Line 527: system replaced with deep Baltic Sea basins

Line 548: systems replaced with ecosystems

Line 566-567: the line ".. depending on whether parameter changes are confined to the coast or applied system-wide." replaced with ".. depending on whether parameter changes are applied only in the coastal zone or across the entire Baltic Sea"

Line 599: the line "Our experiments revealed that coast-only q10 perturbations did not always lead to substantial system-wide changes." revised to "Our experiments revealed that coast-only q10 perturbations did not always lead to substantial changes across the entire Baltic Sea"

Line 601: system replaced with "deep Baltic Sea"

Line 514

Comment: Remind the reader what parameter P is for this sentence to be more understandable. You are describing several parameters at the same time, it gets confusing and difficult to follow.

Response: We have added a clarification to specify that (P) refers to the selected (q_10) temperature sensitivity parameter evaluated in the sensitivity experiments.

Changes to manuscript: The sentence have been revised from "A value of one means that the system reacts identically to local changes in the parameter P"

to

" A value of one means that the deep Baltic Sea basins reacts identically to the parameter (i.e. the selected q10 temperature sensitivity) perturbations, independent of whether these perturbations occur at the coast or in the open basins. "

Line 515–516

Comment: How come? where does this come from? Add reference. The sentence "So, ignoring a deviation…" is not clear. Please be more specific.

Response: We agree that the previous wording was unclear and did not sufficiently explain the basis for this statement. We have revised the text to make the interpretation more clear. The statement in line (515-516) follows directly from our definition of the relative coastal sensitivity (Scr). Specifically, when (Scr=1), the relative sensitivity arising from perturbations confined to the coastal zone (S_c/S) is equal to the fraction of the Baltic Sea area occupied by the coastal zone (A_c/A). Thus, when the coastal zone represents a small fraction of the total Baltic Sea area, its contribution to the deep-basin response is proportionally small under this condition.

Changes to manuscript: The sentences have been revised from "Applying the same relative change over the same number of square meters will have a similar effect on the biogeochemical cycles in the deeper basins. So, ignoring a deviation of this parameter in the coastal zone from the open sea will have a small effect if the coastal area is small."

To

" Scr value of one indicates that the contribution of the coastal zone to the deep Baltic sea basin response is proportional to its spatial extent, i.e., the sensitivity ratio (Sc/S) is equal to the fraction of the Baltic Sea area occupied by the coastal zone (i.e., Ac/A) Thus, ignoring a deviation of this q10 parameter in the coastal zone from the open Baltic basins will have a proportionally small effect on the deep Baltic basin response if the coastal area is small. "

Line 517

Comment: Please replace "we have" by "the coastal sensitivity is high." and explain what is actually meant by coastal sensitivity. Do you mean that the coast is more prone to nutrient recycling (but then you are making it more sensitive due higher temperature dependency?) or do you mean that the coast will have more impact to the open waters? This is not clear. Also the word "differs" is confusing in this context. Differs from what? from the open ocean? Please specify and give some more input on why a smaller or larger values is relevant.

Response: We agree that the previous wording could give the impression that "coastal sensitivity" refers to higher temperature sensitivity or stronger nutrient recycling in the coastal zone. This is not the intended meaning of (Scr). In our analysis, (Scr) quantifies the relative influence of a parameter perturbation confined to the coastal zone on the targeted deep-basin biogeochemical variables, after accounting the area of the coastal zone.

Specifically, an (Scr) value greater than one indicates that the coastal perturbation produces a disproportionately large sensitivity in the targeted deep-basin biogeochemical variable relative to the spatial extent of the coastal zone. Thus, it indicates that changes in the selected parameter within the coastal zone can have a stronger influence on the corresponding deep-basin variable than would be expected from the area of the coastal zone. We have revised the text accordingly and removed the wording "we have" and "differs."

Changes to manuscript: The sentence replaced from. "A value larger than one means that we have an enhanced coastal sensitivity: If the parameter differs in the coastal zone, this has an even larger impact on the open-sea conditions than a local change there."

To

"A Scr value larger than one indicates that the coastal q10 parameter perturbation has a disproportionately large influence on the biogeochemistry of the deep Baltic sea basin relative to the spatial extent of the coastal zone."

And removed the sentence "If the parameter differs in the coastal zone, this has an even larger impact on the open-sea conditions than a local change there."

Line 518

Comment: If I understand correctly, you get one single parameter value for the entire coastal area for each experiment (C1, C2, C3, C4), correct? Do you calculate also one for the open ocean? Or how do you compare to coastal impact? Or are you referring to Scr factor? Please, be more specific.

Response: We thank the reviewer for requesting clarification. We realize that our wording may have caused confusion. By stating that we should "specifically constrain the parameter in the coastal zone," we do not mean that a separate parameter value is calculated for the coastal and open-ocean regions. Rather, we refer to the importance of accurately constraining the parameter in the coastal zone.

When (Scr>1), the perturbation of the q10 parameter confined to the coastal zone produces a disproportionately large response in the corresponding deep-basin biogeochemical variable relative to the spatial extent of the coastal zone. Therefore, although the coastal zone represents only a small fraction of the Baltic Sea area, uncertainties in the parameterization within this region can have a comparatively large influence on the biogeochemistry of the deeper basins. In this sense, measurements of such parameters in the coastal zone are particularly valuable for understanding and reducing uncertainty in deep-basin biogeochemistry.

Changes to manuscript: The sentence has been revised from "This means we should make an effort to specifically constrain the parameter in the coastal zone, since the system reacts with a higher sensitivity to errors we make in the parameterization here."

To

"Within our model framework, this means we should make an effort to specifically constrain the parameter in the coastal zone, since the deep Baltic Sea basins reacts disproportionately to uncertainities in the coastal parameterization"

Line 519

Comment: Is this only valid for your model then? Add more comparisons to other models. Also, I would not call them "errors", these are choices hopefully based on best information available at the time of parametrization.

Response: We thank the reviewer for this comment. We agree that the term "errors" was not appropriate, as the parameter values represent choices based on the best available information and are subject to uncertainty rather than necessarily representing errors. We have therefore replaced "errors" with "uncertainties" in the revised manuscript.

we clarify that the (Scr) metric is newly introduced in this study and is specific to our analysis and should not be interpreted as universally applicable. Our intention is to introduce (Scr) as a diagnostic metric that can be used to

evaluate whether the influence of a coastal parameter perturbation on deep-basin biogeochemistry is proportional to the spatial extent of the perturbed coastal area, or whether the influence is disproportionately larger or smaller than expected from its spatial extent.

Thus, (Scr>1) indicates that, within our model framework, the coastal parameter perturbation produces a disproportionately large influence on the corresponding deep-basin biogeochemical variable relative to its area. The Scr metric could, in principle, also be applied to other spatially modelling systems to assess the proportionality between the spatial extent of a parameter perturbation and its downstream influence, although the resulting values would depend on the model configuration and processes represented.

The motivation for introducing (Scr) is based on previous studies showing that coastal regions, despite their limited spatial extent, can exert a disproportionate influence on marine biogeochemical cycling. We have added references to these studies in the revised manuscript.

Changes to manuscript: a new sentence added at Line after "for the basin-wide biogeochemistry. " as follows

"The Scr metric provides a useful framework for evaluating whether the influence of a coastal parameter perturbation on deep-basin biogeochemistry is proportional to the spatial extent of the coastal zone or is disproportionately larger or smaller than expected from the area affected. "

The sentence "This means we should make an effort to specifically constrain the parameter in the coastal zone, since the system reacts with a higher sensitivity to errors we make in the parameterization here." revised to "Within our model framework, his means we should make an effort to specifically constrain the parameter in the coastal zone, since the deep Baltic Sea basins reacts disproportionately to uncertainities in the coastal parameterization. "

Another sentence added to the revised manuscript to show the motivation to introduce Scr metric as follows

"This approach is motivated by previous studies showing that coastal regions, despite their limited spatial extent, can play a disproportionate role in marine biogeochemical cycling through intense nutrient transformation, retention, and exchange processes with adjacent offshore waters (Mathis et al., 2024; Asmala et al., 2017)"

Line 522

Comment: Please better define what is meant by "parameter", otherwise it is difficult to follow your descriptions.

Response: In this study, the parameter that perturbed in the sensitivity experimentsis the **(q10) temperature-sensitivity parameter associated with the respective biogeochemical process**. We have therefore revised the manuscript to explicitly refer to the **(q10) parameter** where appropriate, rather than using the more general term "parameter." We have also clarified the definition of (P) in the description of the relative coastal sensitivity (Scr) so that it is clear that (P) represents the (q10) temperature-sensitivity parameter of the respective process being perturbed. These revisions have been made throughout the manuscript to ensure consistent terminology

Changes to manuscript: Line 510 "parameter" revised to "q10 parameter"

Line 521: the term "parameter" revised to "q10 parameter"

Line 522: the term "parameter" revised to "q10 parameter"

Line 523

Comment: "we should try to understand the mechanisms why this happens". Yes, why not do it here then? This is the correct place to start a discussion on this.

Response: We thank the reviewer for this suggestion. We agree that the contrasting responses associated with negative Scr values warrant consideration of their underlying mechanisms. However, we have intentionally kept this paragraph focused on the definition and interpretation of the Scr metric. The detailed discussion of the mechanisms underlying the contrasting coastal and basin-wide responses is provided later in the Discussion section (lines 538 - 567 in the submitted manuscript), where we specifically examine the large negative Scr for PO_4 in the Bothnian Bay and formulate a possible mechanistic hypothesis (Figure 12).

Negative Scr values are also observed for NO_3 and NH_4 in the Arkona Basin and for phytoplankton in the Bothnian Bay. Understanding the mechanism behind these responses would require additional analyses of coastal and deep Baltic Sea biogeochemical processes, transport and circulation which are beyond the scope of the present study.

Changes to manuscript: No changes applied to the manuscript.

Line 526

Comment: "These should not be interpreted quantitatively". Why not? Please explain, not obvious in this context.

Response: We agree that our original statement that "very large (Scr) values should not be interpreted quantitatively" was not sufficiently clear. The reason is that, when the basin-wide sensitivity (S) is close to zero while the coastal sensitivity (Sc) remains finite, the ratio (Sc/S) becomes highly sensitive to small changes in (S), which can result in very large (Scr) values. We have therefore clarified this point in the revised manuscript. We now state that very large (Scr) values should be interpreted with caution for this reason, while still indicating that the deep Baltic Sea basins may be much more sensitive to changes in the coastal zone than would be expected from the spatial extent of the coastal perturbation alone.

Changes to manuscript: The sentences revised from

"Very large values of Scr may occur if the sensitivity S is close to zero, but the coastal sensitivity Sc is not. These should not be interpreted quantitatively, but they indicate that the system may be much more sensitive to changes in the coastal zone than one might expect from the classical sensitivity analysis, where the parameter is changed everywhere in the model domain."

To

"Very large values of Scr should be interpreted with caution, as they may occur when the sensitivity S is close to zero, while the coastal sensitivity Sc is not,making the ratio (Sc/S) highly sensitive to small changes in S. At the same time, very large Scr values indicate that the deep Baltic Sea basins may be much more sensitive to changes in the coastal zone than one might expect from the classical sensitivity analysis, where the parameter is changed throughout the model domain."

Lines 514–526

Comment: I think this entire paragraph is an important part of the discussions, but it is too vague, not clear enough or specific enough. More than mentioning "parameter", it should be more clear what they actually mean and possible consequences for general modelling. It is not clear whether this affects only their model or all models. Model tuning/calibration is very model specific. How this can be useful to other models. This is when validation of results should come in and comparison to other studies.

Response: We have thoroughly revised this paragraph to clarify the meaning of q10 parameter and the interpretation of Scr and its implications for model sensitivity, and the discussions of the model specific results obtained in this study and the potential broader applicability of our Scr metric for other modeling studies.

Regarding the applicability to other models: The values of (Scr) obtained in this study are specific to our model configuration, parameterization, and sensitivity experiments. We therefore do not intend to suggest that the specific (Scr) values reported here are universally applicable to other models. Rather, we introduce (Scr) as a metric that could also be applied to other spatially resolved biogeochemical and ocean general circulation models to evaluate whether the influence of a coastal parameter perturbation on offshore or deep-basin conditions is proportional to the spatial extent of the perturbed region. We have clarified this distinction in the revised manuscript.

Regarding validation and comparison with other studies/models:, we agree that an inter-model comparison would be valuable for evaluating the broader applicability of the (Scr) metric. However, (Scr) is newly introduced in this study, and a systematic comparison would require additional sensitivity experiments with other models, which are beyond the scope of the present study. We have therefore clarified the model-specific nature of the numerical results

and the potential broader use of the Scr metric, while avoiding claims that its applicability has already been demonstrated across different models.

We have also clarified that the implications for model tuning and calibration discussed here are specific to our model framework. Specifically, we have added "within our model framework" to the calibration statement to avoid implying that the described calibration consequence is universally applicable to other models.

Changes to manuscript: The paragraph has been thoroughly revised to clarify the meaning and scope of the Scr metric. Specifically:

(1) The term "parameter" has been revised to "q10 paramater" thoroughout the paragraph

(2) The interpretation of Scr has been revised to explicitly explain the relationship between the coastal perturbation, its spatial extent, and its influence on deep-basin biogeochemistry.

(3) The meanings of Scr values close to zero, equal to one, greater than one, and below zero have been clarified.

(4) We have clarified that the **numerical Scr values obtained in this study are specific to our model configuration and sensitivity experiments**, while the metric itself may provide a diagnostic framework applicable to other biogeochemical and ocean circulation models

(5) The statement concerning model calibration/tuning has been clarified by adding "within our model framework" to emphasize that this implication is model-specific.

Line 539 and Line 542

Comment: Please add "(figure 5, e.g., top-left panel)" and add "(Figure 5, bottom-right panel)", respectively.

Response: The figure references added to the manuscript

Changes to manuscript : Line-539: (Figure 5, top-left panel) added to the sentence

: Line 542: (Figure 5, bottom-right panel) added to the sentence

Line 540

Comment: Why sediments more than water column? Negative versus positive Scr shows different responses but not necessarily best results, or?

Response: We agree that the original wording could be interpreted as suggesting that a negative (Scr) necessarily identifies sediment recycling as a preferred calibration target. This was not our intention. In the Arkona Basin, the negative (Scr) for NO_3 results specifically from the contrasting responses to the coastal-only and Baltic Sea-wide perturbations of the (q10) controlling sediment detritus recycling. We have therefore revised the text to make this connection explicit and to clarify that the negative (Scr) indicates opposite responses to the two spatial perturbation experiments, rather than implying that one perturbation produces a better model result than the other. We now emphasize that this contrasting response highlights the need for careful consideration of the spatial parameterization of sediment detritus recycling and its associated (q10) parameter when calibrating our model for the Arkona Basin.

Changes to manuscript: The original statement revised from

"In contrast, the Arkona Basin shows a distinct mechanistic sensitivity, particularly with detritus recycling in the sediment. The observed negative Scr for NO3 suggests divergent responses between coastal only and basin-wide parameter perturbations, highlighting the need for cautious and targeted calibration of sediment nitrogen recycling processes in this basin"

to

"In contrast, the Arkona Basin shows a distinct sensitivity, particularly with detritus recycling in the sediment. The observed negative Scr for NO3 (Figure 5; top-left panel) suggests that the coastal-only and Baltic Sea wide pertur-

bations of the q10 controlling sediment detritus recycling produce responses in opposite directions, highlighting the need for careful consideration of the spatial parameterization of sediment detritus recycling and its associated q10 parameter when calibrating the model for Arkona Basin."

Line 548

Comment: This is too vague, you should find more specific information in the literature. Why is this basin so different?

Response: We expanded the discussion to provide a more specific, literature-supported interpretation of why the Bothnian Bay may respond differently from the southern Baltic basins. We now discuss the pronounced differences between the Bothnian Bay and the Baltic Proper, particularly its weak stratification, predominantly oxic bottom-water and sediment conditions, and strong phosphorus limitation**.** We also discuss the contrasting phosphorus retention under oxic versus hypoxic/anoxic conditions and the role of inter-basin advective phosphorus transport

Changes to manuscript: The sentence at Line 548 "This pattern implies that the ecosystem's response in the Bothnian Bay may be governed by distinct feedbacks or transport processes, where coastal parameter changes lead to contrasting outcomes compared to changes applied basin-wide" removed from the manuscript

A sentence has been added at Line 456 after "basin" as "However, the present analysis does not allow us to identify the specific processes responsible for these responses. A detailed understanding would require further analysis of local remineralization and nutrient transformations, as well as riverine inputs and inter-basin nutrient transport. "

A new paragraph added at Line 550 as "The contrasting response arised can be related to the distinctive physical and biogeochemical characteristics of the Bothnian Bay compared to the Baltic Proper. The weak stratification of the Bothnian Bay promotes well-oxygenated bottom waters and oxic sediment conditions (Algesten et al., 2006; Malmaeus and Karlsson, 2012; Rydin et al., 2023), which facilitate the retention of iron bounded PO4 in the sediments and contribute to PO4 limitation (Savchuk, 2018; Tamminen and Andersen, 2007). In contrast, under hypoxic and anoxic conditions, sedimentary PO4 retention is reduced and PO4 can be released to the wa ter column, as ocuurs particularly in the Baltic Propoer (Conley et al., 2009; Carstensen et al., 2014b; Conley et al., 2002). Thus, coastal-only (C1_DetSed) and Baltic Sea-wide (E1_DetSed) perturbations of sediment detritus recycling can affect PO4 cyc ling differently because they modify sediment processes under contrasting redox conditions. In addition, advective PO4 transport among the Baltic basins provides a pathway through which changes in PO4 cycling in the other Baltic basins can influence PO4 dynamics the Bothnian Bay (Savchuk, 2018, 2005). Together, these spatial differences in sediment PO4 cycling and inter-basin transport provide a possible explanation for the contrasting response of PO4 to Baltic sea wide perturbations (E1_DetSed; Figure 3) and coastal-only (C1_DetSed; Figure 4) observed in our study."

Figure 12

Comment: Nice figure, but needs to be put further into context of what has been published before and what is actually new.

Response: In the revised manuscript, we added relevant published literature to distinguish established knowledge from the findings of our study. Our sensitivity experiments suggest that the spatial location of enhanced temperature-sensitive sediment recycling can modify the basin-scale phosphorus response, particularly in the Bothnian Bay. Specifically, enhancing sediment recycling only in the coastal zone produces a contrasting response in the Bothnian Bay compared with applying the same perturbation throughout the Baltic Sea. Figure 12 now presents our hypothesized mechanism for this contrasting PO_4 response in the deep Bothnian Bay, incorporating the role of the coastal filter and inter-basin phosphorus transport based on evidence from previous studies.

Changes to manuscript: An entire paragraph added before introducing Figure 12 as follows

Please refer to the "changes to manuscript" of above comment for the new paragraph added at line 550.

The Figure 12 has been redrawn as per Reviewer 2 is suggestion

Lines 552–556

Comment: Is this supported by published evidence? How robust is this? If no comparison is made with previous findings, it is difficult to judge the likelihood of this mechanism.

Response: Thank you for the comment. In the revised manuscript, we have added published evidence to support pur hypothsis. We now clarify that the mechanism shown in Figure 12 is a hypothesis rather than a directly demonstrated process in our simulations. Specifically, we cite previous studies showing that the coastal Bothnian Sea is characterized by well-oxygenated bottom waters and oxic sediments that favour P retention in sediments, as well as observational evidence linking higher bottom-water oxygen concentrations with higher water-column N:P ratios. These findings support the plausibility of our interpretation that enhanced coastal sediment detritus remineralization can release additional N and P, while preferential P retention under oxic sediment conditions may reduce P availability relative to regenerated N. We have therefore revised the text to distinguish clearly between processes established by previous studies and the mechanism hypothesized from our sensitivity experiments.

Changes to manuscript: The discussion of Figure 12 has been revised to place the proposed mechanism in the context of previous findings. New supporting statements added to the section as follows

At line 554 after "additional NH4 and PO4" as "The coastal Bothnian Sea is characterised by well-oxygenated bottom waters and oxic sediments (Caballero-Alfonso et al., 2015), conditions that favour the retention of phosphate through binding to iron oxides (Savchuk, 2018; Tamminen and Andersen, 2007; Van Helmond et al., 2020). Thus, it is plausible that a substantial fraction of the additional PO4 released by enhanced coastal remineralization is retained in the sediment (Spilling et al., 2018) rather than exported to the open Bothnian Bay. "

At line 556, after "trapped in the sediments" as "Observational evidence from the coastal Baltic Sea demonstrates a positive correlation between bottom-water oxygen levels and water-column N:P ratios, indicating that sediment phosphorus retention under oxic conditions drives severe phosphorus depletion relative to regenerated nitrogen (Van Helmond et al., 2020)."

References added to support the mechanisms are as follows:

1. Caballero-Alfonso, A. M., Carstensen, J., and Conley, D. J.: Biogeochemical and environmental drivers of coastal hypoxia, Journal of Marine Systems, 141, 190–199, 2015

2. Savchuk, O. P.: Large-scale nutrient dynamics in the Baltic Sea, 1970–2016, Frontiers in Marine Science, 5, 95, 2018.

3. Tamminen, T. and Andersen, T.: Seasonal phytoplankton nutrient limitation patterns as revealed by bioassays over Baltic Sea gradients of salinity and eutrophication, Marine Ecology Progress Series, 340, 121–138, 2007.

4. Van Helmond, N. A., Robertson, E. K., Conley, D. J., Hermans, M., Humborg, C., Kubeneck, L. J., Lenstra, W. K., and Slomp, C. P.: Removal of phosphorus and nitrogen in sediments of the eutrophic Stockholm archipelago, Baltic Sea, Biogeosciences, 17, 2745–2766, 2020.

5.Spilling, K., Olli, K., Lehtoranta, J., Kremp, A., Tedesco, L., Tamelander, T., Klais, R., Peltonen, H., and Tamminen, T.: Shifting diatom—dinoflagellate dominance during spring bloom in the Baltic Sea and its potential effects on biogeochemical cycling, Frontiers in Marine Science, 5, 327, 2018.

Lines 552–567

Comment: This entire paragraph contains no citation and most of what is described here is not new. Please refer to the corresponding papers describing these mechanisms and highlight what is actually new in this manuscript.

Response: We agree that the original paragraph did not sufficiently distinguish between previously established mechanisms and the interpretation emerging from our sensitivity experiments. In the revised manuscript, we have therefore added references to previous studies documenting the relevant processes, including phosphorus retention under oxic sediment conditions, phosphorus release under hypoxic/anoxic conditions, and inter-basin phosphorus transport within the Baltic Sea. Our results show that, In the Bothnian Bay the same temperature-sensitive sediment-recycling perturbation produces contrasting basin-scale PO_4 responses depending on whether it is confined to the coastal zone or applied across the entire Baltic Sea. We propose that the spatial extent of the perturbation, together with the con-

trasting redox conditions and inter-basin transport pathways, provides a possible explanation for this contrasting response in the Bothnian Bay.

Changes to manuscript: In addition to the references listed in above comment we have cited

1. Conley, D. J., Bjorck, S., Bonsdorff, E., Carstensen, J., Destouni, G., Gustafsson, B. G., Hietanen, S., Kortekaas, M., Kuosa, H.,Markus Meier, H., et al.: Hypoxia-related processes in the Baltic Sea, Environmental science & technology, 43, 3412–3420, 2009

2. Carstensen, J., Conley, D. J., Bonsdorff, E., Gustafsson, B. G., Hietanen, S., Janas, U., Jilbert, T., Maximov, A., Norkko, A., Norkko, J.,et al.: Hypoxia in the Baltic Sea: biogeochemical cycles, benthic fauna, and management, Ambio, 43, 26–36, 2014b.

3. Conley, D. J., Humborg, C., Rahm, L., Savchuk, O. P., and Wulff, F.: Hypoxia in the Baltic Sea and basin-scale changes in phosphorus biogeochemistry, Environmental science & technology, 36, 5315–5320, 2002.

4. Savchuk, O. P.: Resolving the Baltic Sea into seven subbasins: N and P budgets for 1991–1999, Journal of Marine Systems, 56, 1–15, 2005

5. Liu, Y., Meier, H. M., and Eilola, K.: Nutrient transports in the Baltic Sea–results from a 30-year physical–biogeochemical reanalysis,Biogeosciences, 14, 2113–2131, 2017.

Summary and Conclusions

General comments

Comment: The section will need to be rephrased according to new discussion section, but some problems in this section can be already notified so that they are not reproduced later on.

Response: We thank the reviewer for this comment. The Discussion section has been substantially restructured and extensively rewritten in the revised manuscript to address the reviewer's comments and to improve the clarity, interpretation, and context of the results.

Changes to manuscript: The section have been revised accordingly

Line 580

Comment: This is not clearly mentioned/highlighted in the discussions, if mentioned here, it should be better highlighted.

Response: Thank you for pointing this out. The strong sensitivity of NH_4 to changes in the q10 parameter was already presented in the Results (Lines 276–277). To ensure consistency between the Results and Summary, we have revised the reported percentages accordingly.

Changes to manuscript: The percentage in line 276-277 revised from "30-45%" to "20-50%"

Line 580: "50%" revised to "20-50%"

Line 584

Comment: Accumulation where? Not clear, rephrase the sentence.

Response: Thank you for pointing this out. We agree that the sentence was unclear and potentially misleading, and we have revised it as follows:

Changes to manuscript: The sentence revised from "The relatively low accumulation of ammonium and nitrate compared to the anoxic basins indicates an enhanced remineralization of detritus and organic matter in the sediment and water column" to "As a result, ammonium accumulation remains relatively weak while nitrate increases in oxic basins, whereas anoxic basins show stronger ammonium accumulation and declining nitrate concentrations. "

Line 594

Comment: Please add the cause, e.g., "due to enhanced remineralization…"

Response: Thank you for the suggestion. The text has been revised accordingly.

Changes to manuscript: The cause "due to enhanced remineralization" added to the revised manuscript

Line 594 and 597

Comment: Care must be taken with the use of significant or disproportionately, if no statistics or comparisons are given.

Response: Thank you for pointing this out. The text has been revised accordingly.

Changes to manuscript: The "significant accumulation of phosphate" revised ro "The accumulation of phosphate"

Line 600

Comment: Is "coastal sensitivity" a name you chose for the method? Why call it sensitivity? The term sensitivity does not say much about what you are trying to estimate. In my understanding, this method represents the relative coastal impact. It should be clarified in methods and introduction, but here it becomes more obvious that the term can mean different things, if not well defined.

Response: Thank you for pointing this out. We have retained the term "the relative coastal sensitivity (Scr)"as its definition and purpose are explicitly described in the Methodology of the revised manuscript. We define (Scr) as the relative sensitivity of deep Baltic Sea biogeochemistry to a coastal perturbation, normalized by the relative area of the coastal zone. Specifically, Scr = (Sc/S)/(Ac/A), where (Sc) is the sensitivity of the deep basin to the coastal-only perturbation, (S) is the sensitivity of the deep basin to the Baltic Sea-wide perturbation, (Ac) is the coastal area, and (A) is the total Baltic Sea area. Thus, "relative" refers to the comparison between the responses to the coastal-only and Baltic Sea-wide perturbations, while the area normalization accounts for the smaller spatial extent of the coastal zone.

Changes to manuscript: The Methodology has been thoroughly revised, and the term relative coastal sensitivity (Scr) has been clearly defined and introduced.

Lines 601–603

Comment: "Using this metric…". This sentence needs to be more specific, for example: "that nitrate in (which basin? all?) is consistently impacted by coastal sediment recycling. This suggests that basin wide enhancement of (what? water column, sediment recycling, DOC recycling, nitrification?) ..."

Response: Thank you for the comment. Here, we mean that enhanced coastal sediment detritus recycling has a disproportionately strong impact on nitrogen cycling, relative to the corresponding basin-wide perturbations, in the Arkona Basin, Gulf of Riga, and Bothnian Bay.

Changes to manuscript: The text has been revised to clearly specify that the relative coastal sensitivity of nitrate is associated with coastal sediment detritus recycling and to identify the basins where this effect is most pronounced (ie. Scr>1): the Arkona Basin, Gulf of Riga, and Bothnian Bay. The comparison with the corresponding basin-wide sediment detritus recycling perturbation has also been clarified.

The sentence has been revised to " Using this metric, we observed that nitrate showed consistently greater relative sensitivity to coastal sediment detritus recycling than to the corresponding basinwide enhancements, particularly in the Arkona Basin, Gulf of Riga, and Bothnian Bay"

Line 602

Comment: This is a very important part of your work and needs to be absolutely clear. As currently written, the sentence in the Conclusion is open to interpretation, which should be avoided, since readers may not refer back to earlier sections to fully understand its meaning.

Response: Thank you for pointing this out. We agree that the implication of this result should be clear We have therefore further clarified the sentence to explicitly state that the greater relative sensitivity of nitrate to coastal sediment detritus recycling indicates that changes in coastal sediment remineralization, including those associated with warming, may exert disproportionately strong effects on basin-scale nitrate dynamics in these sensitive basins.

Changes to manuscript: The sentence has been revised to "Using this metric, we observed that nitrate showed consistently greater relative sensitivity to coastal sediment detritus recycling than to the corresponding basinwide enhancements, particularly in the Arkona Basin, Gulf of Riga, and Bothnian Bay, indicating that changes in coastal sediment detritus remineralization, including those associated with warming, can exert disproportionately strong effects on basin-scale nitogen dynamics in these sensitive basins"

Line 604

Comment: The "2-4 times greater" is not highlighted properly in discussions.

Response: We agree that the magnitude of the relative coastal sensitivity (Scr) in the Bothnian Bay is an important result and should be highlighted more clearly in the Discussion. We have revised to highlight this 2-5 fold responses, while keeping the real magnitudes reported in the results section. We clarify that, In the Bothnian Sea these high Scr values indicate a relative coastal influence substantially greater than expected from the spatial extent of the coastal zone.

Changes to manuscript: The sentence at line of discussion has been revised from

"The Bothnian Bay exhibits the most complex pattern of Scr. Very high values of Scr for nutrients (NH4, NO3, and PO4) and phytoplankton for sediment detritus recycling indicate that coastal processes significantly impact the bio-geochemical cycles of this basin"

to

"The Bothnian Bay exhibits the most complex pattern of Scr . Very high values of Scr for nutrients (N H4, N O3, and P O4) and phytoplankton for sediment detritus recycling, reaching approximately 2–5 times the relative response expected from a basin-wide perturbation, indicate that coastal processes can exert a particularly strong influence on the biogeochemical cycles of this basin.(Figure 5, bottom-right panel)"

Line 604, the "2-4 times greater" has been revised to "2-5 times greater" as

Lines 613–614

Comment: Interesting finding and likely very important, but evidence from other studies is lacking. Evidence from previous studies (the Baltic Sea being a widely if not the most studied basin), must exist and will supports this in the real world. Please add references to studies on e.g., sediments in coastal waters by Slomp, Gustafsson, Jilbert, Cartensen and many others. Some of them are referred in the introduction, but none in the discussions and therefore, your conclusions, although relevant, are not very robust as presented at the moment.

Response: Thanyou for the comment. We agree that this finding should be better supported by previous studies.The revised manuscript now includes the relevant literature support in the Discussion section, particularly regarding the sediment biogeochemistry, phosphorus retention under oxic conditions, and phosphorus transport influencing the Bothnian Bay. The Summary and Conclusions have also been revised to clarify this finding.

Changes to manuscript: In the summary and conclusion the text has been replace from "The Bothnian Bay is more sensitive to changes in coastal processes than other basins." to " The Bothnian Bay is more sensitive to changes in coastal processes than other basins. In particular, phosphate responds differently to changes confined to the coastal zone than to changes applied across the Baltic Sea. This contrasting response is related to the basin's distinctive sed-iment biogeochemistry, phosphorus retention under oxic conditions, and its connection to phosphorus transport from the more anoxic Baltic basins (Savchuk, 2018; Rolff and Elfwing, 2015; Liu et al., 2017). "

The Discussion section has been revised to include relevant references. For more details, please refer to our responses to the comments on Lines 552–556 and Lines 552–567.