Review #3

First of all, we would like to thank the referee for the thorough review of our manuscript. We provide our response on how to consider the referee's suggestions in a revised version of our manuscript.

In the following, we respond to the referee's #3 remarks. Remarks are shown in black and our response in red, and text modifications in blue.

Some of the reviewer's concerns stem from the perceived brevity of the model description. However, all components of our model system have been thoroughly introduced in previous publications, to which we have provided appropriate references. Rather than replicating extensive details from these sources, we have opted to provide concise explanations of each model component's purpose. Including full descriptions would unnecessarily expand the manuscript. That said, we will enhance certain aspects of the model description in the revised version to improve clarity where needed.

This manuscript used a high-resolution coastal model to study the retention capcity of the Oder Lagoon. The topic is interesting, however, the manuscript need a major revision before publication for the following major concerns.

First, as a model study, the manuscript lacks sufficient details regarding model description and needs clarification: 1) How are model parameters determined? Please provide justification or references for parameter values.

In line 81 of our manuscript, we refer to Neumann et al. (2022) for a detailed explanation and validation of the model. The ERGOM model employs over 130 parameters, which—consistent with standard practice in ecosystem modeling—were determined during the calibration phase to optimize model performance. Since the complete model description is provided in the cited reference, we consider it unnecessary to repeat these details here. Furthermore, we contend that, within a reasonable range, an ecosystem model's performance depends more critically on its structural design than on the specific parameter values selected.

2) In oxygen-rich environment, phosphate becomes bound to iron oxide. How does the model simulate iron oxide? Are this process and phosphate release simulated prognostically or through parameterization?

Iron oxide complexes are represented as a prognostic state variable in the model. As with all ecosystem models, not every process is described deterministically; instead, certain aspects are parameterized. For a detailed treatment of these processes, we refer the interested reader to our comprehensive model description in Neumann et al. (2022).

3) In this model, the light attenuation is determined by chlorophyll and CDOM. Please explain how to simulate CDOM and how well is the simulation of CDOM?

In line 94 of our manuscript, we refer to Neumann et al. (2021) for a detailed description and validation of the optical model, including colored dissolved organic matter (CDOM). As CDOM observations for the Oder Lagoon are unavailable, we used Secchi depth measurements as an alternative validation approach.

4) For riverine input, only fresh water and nutrients were described. How about other state variables, including CDOM, each functional group of phytoplankton, and etc? Since the liminic phytoplankton thrive in fresh and turbid water, I assume that the fresh water is turbid with high chlorophyll or CDOM concentration. Then, the question is, how are the riverine inputs of chlorophyll (each groups of phytoplankton) and CDOM specified, by observations or some assumptions?

As described by Neumann et al. (2021), CDOM is introduced into the model via riverine runoff based on observational data. All phytoplankton groups in the model are capable of growth when environmental conditions are favorable. Consequently, there is no need to include marine phytoplankton groups in the runoff. For limnic phytoplankton, however, we employ a different approach: a portion of the total nutrient input is allocated specifically to limnic phytoplankton. This distinction should be clarified in a revised version of the manuscript.

Additional text:

A minor fraction of the total nutrient loads enters the lagoon through the limnic phytoplankton state variable, which ensures seed concentrations near the river mouth. Riverine CDOM concentrations are prescribed using a monthly climatology, as described in detail by Neumann2021.

5) Does meterological forcing include data of nutrient deposition? How is the nutrient deposition simulated in the model?

Atmospheric deposition is realized as a boundary condition (air-sea fluxes) based on data provided by HELCOM which are originated from EMEP (https://www.eea.europa.eu/data-and-maps/data/external/emep-n-atmospheric-deposition). We will add this information in a revised version of the manuscript.

Additional text:

Atmospheric deposition is realized as a boundary condition (air-sea fluxes) based on data provided by HELCOM assessments (e.g., HELCOM, 2018) which are originated from EMEP (https://www.eea.europa.eu/data-and-maps/data/external/emep-n-atmospheric-deposition).

6) The authors state in Line 104: “the extracellular excretion of dissolved organic matter by phytoplankton results in non-Redfield carbon uptake”. This sentence is unclear to me and needs clarification. Also, I would like to ask how does the model deal with nutrient stoichiometry? Is it fixed or variable in the model?

The non-Redfieldian carbon uptake and resulting stoichiometric relationships are described in Neumann et al. (2022). In the revised manuscript, we will include essential information to ensure readers understand the fundamental mechanisms of this process.

Text changed:

Removed: Furthermore, the extracellular excretion of dissolved organic matter by phytoplankton results in non-Redfield carbon uptake.

Added: Furthermore, phytoplankton excrete extracellular dissolved organic matter with non-Redfield stoichiometry, resulting in non-Redfield carbon uptake, while maintaining canonical Redfield ratios within their cellular composition.

7) Some model components are missing in the model description, including the ice model, the two-layer sediment model, and the definition of residence time. The authors answered in their response letter that they had added description of the ice model in the revised manuscript, however, it remained absent. For the residence time, please give the equation used to calculate the residence time in the manuscript.

In response to Referee #2's request regarding the model's sea ice capabilities, we confirm that our manuscript specifies both the sea-ice model employed and its foundational reference. Our implementation utilizes a regional configuration of the well-established MOM (Modular Ocean Model) framework, which includes a fully coupled ocean-sea-ice component.

As this sea ice component is based on a widely-used, thoroughly documented framework with numerous existing publications detailing its formulations and capabilities, we have chosen to reference the foundational publications rather than replicate these descriptions. The key reference providing the basic formulations of the sea-ice model is Winton (2000), which offers comprehensive details on its physical and thermodynamic representations.

We believe this approach provides sufficient information for readers to understand our implementation while avoiding unnecessary duplication of well-documented methods. However, we are happy to add a brief summary of the sea ice model's key features in the revised manuscript if the referee finds this would be helpful for clarity.

Text added:

The sea ice component implements:

a) A three-layer vertical thermodynamic scheme
b) Multiple ice thickness categories with dynamic redistribution
c) Category transition mechanisms responding to thermodynamic and mechanical forcing
d) Full ice dynamics incorporating internal stresses via an elastic-viscous-plastic rheology

The equation and relevant references for residence time estimation are provided in the caption of Figure 8 (Fig. 6 of the revised manuscript).

Second, the model validation is not sufficient enough to support their conclusions.

1) The model failed to reproduce the observed season cycle of surface chlorophyll (one peak versus two peaks). Since phytoplankton growth is the primary driver of organic matter deposition and therefore is a key in N/P retention, the authors should discuss how this model bias might affect their key conclusions. Attributing the model bias to the fixed carbon-to-chlorophyll ratio is insufficient.

We disagree with the characterization that our model fails in representing chlorophyll concentration. Organic matter production is also reflected in the nutrient dynamics, which likely provides a more accurate representation of net production than chlorophyll concentration. Furthermore, we maintain that the chlorophyll-to-carbon ratio has significant implications on the chlorophyll concentration estimate, contrary to the suggestion of minimal impact. We also acknowledge that water column opacity represents an additional influencing factor, which we will incorporate into the discussion of our revised manuscript.

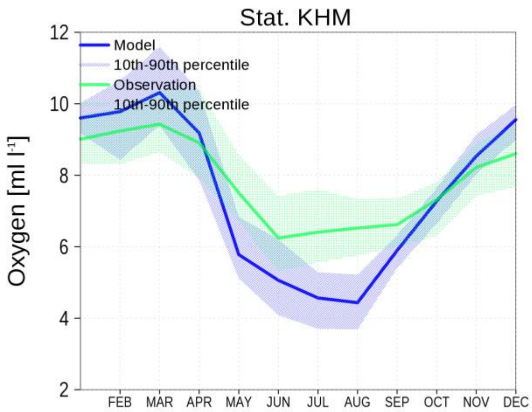
Text added:

Another limitation of our simulations is the systematic underestimation of winter opacity (Fig. A4), which may potentially advance the timing of vernal blooms. This discrepancy likely arises from our model's current implementation, which accounts for resuspension of organic matter only, while neglecting the resuspension of mineral sediments. The omission of mineral sediment dynamics probably contributes to the simulated overestimation of winter water clarity.

2) The model significantly underestimated bottom oxygen concentration. However, the authors' justifications are unsatisfactory: (i) mismatch in depth between model and observations – the validation should use the closest model grid cells to measurement depths for comparisons (ii) ship-induced mixing – The authors claim in appendix that the model is more realistic and ship-induced mixing as a shortcoming of observations. I disagree with this statement. While ship disturbances occure in reality, neglecting this process is the a weakness of the model, not shortcoming of observations. Since the oxygen controls phosphate release and denitrification, the model's failure in simulating bottom oxygen cast doubt on conclusions of this manuscript. Please improve the model performance or provide discussions on whether this bias affect conclusions.

(i) Unfortunately, the available observations from the Oder Lagoon are limited to surface and near-bottom measurements only, with no exact depth indication. With regard to the observing staff, near-bottom observations are estimated to represent conditions approximately 1 meter above the seafloor.

While we could include an additional figure comparing observed near-bottom oxygen concentrations (1 m above bottom) with model data at the corresponding depth, we believe the current figure showing simulated bottom oxygen remains the most appropriate representation for our analysis.



Near bottom oxygen observations and model simulation 1 meter above bottom at station KHM.

The figure presented above compares observed near-bottom oxygen concentrations with model simulations at approximately equivalent depths corresponding to the measurement locations. We emphasize that within the 10th to 90th percentile range, anoxic conditions never occur, thereby maintaining stable redox conditions. While anoxia represents a rare and transient phenomenon in our system, these episodic events nonetheless exert significant influence on phosphorus cycling and benthic community dynamics. This conclusion remains valid even when considering the closest near-bottom simulation values (20 cm above the sediment-water interface).

Text added:

Figure A3 illustrates the elevated oxygen concentrations simulated at 1 m above the seafloor (approximating the measurement depth) compared to our simulated near-bottom oxygen concentrations (Fig. A1f). We emphasize that within the 10th to 90th percentile range, anoxic conditions never occur, thereby maintaining stable redox conditions. While anoxia represents a rare and transient phenomenon in our system, these episodic events nonetheless exert significant influence on phosphorus cycling and benthic community dynamics. This conclusion remains valid even when considering the closest near-bottom simulation values (20 cm above the sediment-water interface).

(ii) We acknowledge the referee's valid point regarding unresolved ship traffic as a limitation of our model. To clarify, we never intended to suggest that the model provides enhanced realism in not representing ship traffic effects—this appears to be a misunderstanding that we will correct by revising the relevant text for greater precision. Furthermore, we explicitly identify this limitation as a model weakness and highlight it as an area for future improvement in the conclusions section of our manuscript.

The observed discrepancies between measured and simulated near-bottom oxygen concentrations are primarily confined to the navigation channel, where they result from the model's inability to account for ship traffic effects. However, given that the navigation channel constitutes only a small fraction of the

Oder Lagoon's total area, these localized oxygen dynamics have minimal impact on the overall phosphate release and binding processes in the lagoon system.

Modified Text in the discussion section:

Oxygen deficiency in the near-bottom water is widespread in the Oder Lagoon. The most affected area is the artificial navigation channel (see section 3.2). In the recent model implementation, the model does not account for ship traffic, which causes regular vertical mixing down to the bottom. Thus, in contrast to the model, observations do rarely show hypoxia in the navigation channel. Given that the navigation channel constitutes only a small fraction of the Oder Lagoon's total area, these localized oxygen dynamics have minimal impact on the overall phosphate release and binding processes in the lagoon system.

3) There is no validation of bottom nutrients, which is an important indicator of phosphate release and nutrient retention.

An additional figure will be provided in a revised version of the manuscript.

Text added:

For completeness, we have included Figure A5, which compares simulated and observed near-bottom nutrient concentrations at stations KHM. Notably, these near-bottom values exhibit minimal divergence from surface concentrations (Fig A1), suggesting frequent vertical homogenization of the water column. This interpretation is further supported by Figure A9, which demonstrates that stratification events in the Oder Lagoon are typically short-lived and frequently disrupted by meteorological forcing.

4) For model validation of stratification, Figure A7 is hard to read. Please provide some metrics (e.g. RMSE, R-square) between observations and modelled results.

We question the added scientific value of including this data, given that observational records are predominantly unavailable during the winter season, which would limit meaningful comparison and interpretation.

The primary objective of this figure is to demonstrate that stratification patterns are consistently observed in both field measurements and model simulations. Given that stratification constitutes a necessary precondition for anoxia development, this correspondence demonstrates that both the natural system and our model have the potential to develop anoxic conditions under appropriate circumstances. We will strengthen interpretations in the revised manuscript version.

Text added:

The comparative analysis presented in this figure reveals consistent stratification patterns between empirical observations and model simulations. Given that stratification constitutes a necessary precondition for anoxia development, this correspondence demonstrates that both the natural system and our model have the potential to develop anoxic conditions under appropriate circumstances.

Third, the current version of discussion section should go to results, and some discussions looks not highly relevant. The subsection 4.2 should be shortened.

We find this comment insufficiently specific regarding which particular elements require relocation. Nevertheless, we will carefully evaluate the organization of our Results and Discussion sections in the revised manuscript. Additionally, we will review Section 4.2 to eliminate any potential redundancy and improve overall clarity.

In response to referee's comments, we have restructured the manuscript by relocating the majority of interpretative content from the Discussion to the Results section. The former Section 4.2 has been condensed and streamlined for improved clarity and conciseness. All specific textual modifications are indicated in the marked-up version of the revised manuscript.

In addition, some key issues are not discussed in the current mansucript. For instance,

1) as I mentioned above, How might model bias in simulating surface chlorophyll and bottom oxygen affect the nutrient retention conclusions? What sensitivity analysis can be done to test the robustness of conclusions?

As outlined in our previous response, we do not consider the observed differences in chlorophyll time series to significantly impact net primary production estimates. This interpretation is supported by the consistent nutrient dynamics across observations and our simulations, which remain the primary driver of production.

In the conclusion section of our revised manuscript, we will propose potential avenues for future sensitivity analyses.

Paragraph added:

To complement our hindcast simulations, targeted scenario analyses could provide valuable insights into the system's sensitivity to specific anthropogenic interventions. Particularly informative scenarios might

a) Assessments of varying ship traffic intensities and their impacts
b) Evaluations of potential navigation channel deepening effects

Such scenario simulations would enable a more comprehensive understanding of the lagoon's response to management measures and environmental modifications.

2) Differences in N- and P-retention response to nutrient loads deserve explanation, including but not limited to model bias in simulating bottom oxygen, the N:P ratio of riverine input (limited nutrients), and the inherent differences between N and P cycles. Answers to this question may help clarify whether this conclusion in Odor Lagoon can be applied to other coastal systems.

We would like to clarify that the differences in retention capacity due to load reduction arising from nitrogen and phosphorus pathways are explained in Sections 3.3 and 4.2 of our manuscript. Based on our current understanding of the system, we maintain that no significant correlation between riverine N:P ratios and retention exists, as the Oder Lagoon is not typically nutrient-limited. This lack of limitation suggests that retention processes are not primarily controlled by nutrient stoichiometry in this particular ecosystem.

(3) What is the implications for larger-scale models? In the introduction part, the authors mentioned that some baltic model accounted for the filter function of nutrients by assuming bioavaliability or reduction factors. The authors should compare their finds to previous empirical approaches, and discuss the validity of the previous assumptions.

It is important to note that most model descriptions in the literature do not explicitly quantify bioavailability parameters. These parameters are results of model calibration. By contrast, our reduction/retention factors are derived from a mechanistic approach. We believe this methodological distinction is evident in our manuscript.

Finally, some comments and concerns raised in the initial review were not well addressed and some revisions promised in the response round were not made in the revision round. These will be listed in the detailed comments. To facilitate the evaluation of revisions, I would also suggest the authors to 1) povide locations for each revision in their response letter and 2) include revised text in the response letter.

Detailed comments:

L87: please provide the number of model grid cells or the length/width of the lagoon. This will help readers who are not familiar with this region to better understand the model resolution.

The model's spatial resolution is specified in lines 87-88 of the manuscript. While the spatial extent can be approximated from Figure 1, we are happy to provide the exact domain dimensions in the revised version of the manuscript.

Text added:

(altogether 330x191 grid points)

L104: Why this lead to non-Redfield ratio of nutrient uptake?

We note that this comment appears to be identical to the referee's major concern #6. Therefore, we refer the referee to our response to that concern, where this issue has already been addressed.

L128: How to quantify/calculate mas transport?

Mass transport in our model is calculated using the standard oceanographic approach of integrating the product of density and velocity across the cross-section. As this is a fundamental fluid dynamics principle that is well-established in the literature, we have not included a detailed explanation in the manuscript.

Section 3.1: wrong reference to Fig1

The reference to Figure 1 is intended to direct readers to the locations of stations KHM and C. To prevent any potential misunderstanding, we will either: (1) remove the reference if it proves confusing, or (2) clarify it by adding a parenthetical note such as '(for station locations, see Figure 1)' in the revised manuscript.

Text added:

(for station locations, see Figure 1)

L217-228: The N-retention rate (40%) in this study is much higher than previous studies (<30%). Why? The authors attribute this difference to the fact that previous studies only account for denitrification while neglecting other source/sink terms; however, this explanation appears insufficient. Based on Figure 6, the non-denitrification processes contribute minimally to the overall nitrogen budget.

Given the known uncertainties in experimental denitrification estimates—compounded by their sparse spatial and temporal coverage—we consider the discrepancy between 40% and 30% to be within an acceptable range of variation. Our modeling approach does not aim to precisely replicate observations (which themselves contain inherent uncertainties), but rather to provide complementary insights through an alternative methodological framework.

We acknowledge the referee's valid point that sediment burial alone cannot fully account for the observed differences, and we will remove this statement in the revised manuscript.

Text added:

However, burial and pelagic denitrification are only minor contributions to the nitrogen retention.

Regarding nutrient retention, Pastuszak et al. (2005) reported values of 85% for total nitrogen (TN) and 72% for total phosphorus (TP). However, it is important to note that their study area included inland portions of the Oder Lagoon that fall only partially within our model domain. We will incorporate this reference in the revised version to provide additional context for our results.

Text added:

Pastuszak et al. (2005) report substantially higher retention rates of 85% for nitrogen and 72% for phosphorus. However, these elevated values must be interpreted with consideration of methodological differences: their study encompassed inland regions of the Oder Lagoon that extend beyond our defined model domain, potentially influencing the observed retention metrics.

Response to reviewer #1 Comment on L199-205: the promised discussion of hypoxia were not made in the revised manuscript.

To the best of our current knowledge, no direct observations of anoxic conditions in the Oder Lagoon have been documented. This absence of empirical data can primarily be attributed to suboptimal temporal and spatial sampling strategies. Nevertheless, several proxy indicators suggest episodic anoxia occurrence, including documented fish and mussel mortality events as well as summer phosphate peaks.

These findings have been comprehensively reported in Schernewski et al. (2025). We will synthesize and incorporate these proxy-based observations in the revised version of our manuscript.

Schernewski G, Neumann T, Piehl S and von Weber M (2025) New approaches to unveil the unknown: oxygen depletion and internal eutrophication in a Baltic lagoon over decades. Front. Environ. Sci. 13:1620191. doi: 10.3389/fenvs.2025.1620191

Paragraph added:

To the best of our current knowledge, no direct observations of anoxic conditions in the Oder Lagoon have been documented. This absence of empirical data can primarily be attributed to suboptimal temporal and spatial sampling strategies. Nevertheless, several proxy indicators suggest episodic anoxia occurrence, including documented fish and mussel mortality events as well as summer phosphate peaks. These findings have been comprehensively reported in Schernewski et al. (2025].

Comment on L222: Regarding “what could be transfered to other regions”, this was not well addressed. The authors present several assertions yet without providing any justification or supporting evidence in the manuscript. Please also see my major comments above. In addition, the promissed discussion on “which coastal type requires additional studies” were not made.

In lines 253 et seq., we present our conclusions regarding the transferability of our findings to other regions, including an assessment of which regions may require further investigation.

Response to reviewer #2

Comment 5: no description of ice model in revised manuscript

We refer the referee to our detailed response to Major Comment 7 under Referee #3's review, where this specific issue has been thoroughly addressed.

Comment 9: maybe I missed it, but I can’t find it in the revised manuscript

We direct the referee to lines 128, 180, and 186 of our manuscript, where this specific aspect is addressed.

Comment 10: please add this to manuscript

We have already addressed this question in our response to the review. Regarding inclusion in the manuscript, we respectfully maintain that this information constitutes established textbook knowledge and therefore does not require repetition in our publication.

Comment 12: This didn’t answer the reviewer’s concerns.

We are confident that our response to the review effectively addresses the referee's concern.

Comment 18: No statistical evaluation in the manuscript. The reviewer was requesting a statistic test to examine differences between the control and halved-load experiments, rather than the trends from combined experiments.

Upon reflection, we acknowledge that our initial response to this comment could have been more precise. We maintain that statistical proof of the retention difference between the two experiments is not scientifically justified in this context, as it was not the primary research question. Rather, our investigation focuses on determining whether different nutrient loads elicit distinct relative retention capacities—a question that can be effectively addressed through the trend analysis we have conducted and presented in our manuscript.

Comment 19: see major comments above

Regarding Comment 19, we have already provided a definition of retention in our response to Referee #2.

Comment 33: The response is unclear

We would like to clarify that Referee #2's comment pertains to the **water surface**, whereas our discussion specifically addresses the **sediment-water interface**, as clearly indicated in line 237 of the manuscript.

Comment 36: see major comments above

We kindly refer the referee to our response to Major Comment 7, where this issue is addressed.

Comment 38: It confused me. The authors kept mentioning that changes in river discharge reflect changes in nutrient load because nutrient concentrations were stable, yet at the same time argued that this relationship did not apply for long-term analyses.

We would like to clarify that our statement does not suggest this relationship fails to hold in long-term **analyses**. Rather, we emphasize that under a long-term **perspective**—particularly when considering potential changes in land-use or agricultural practices, as noted in the manuscript—additional factors may influence the observed dynamics.