Review for «Transformation Processes in the Oder Lagoon as seen from a Model Perspective» by Neumann et al.

Overall evaluation

In general, the manuscript is well written, well presented and clear, including mathematical expressions and equations. It reaches innovative and robust conclusions that can greatly improve model studies in the area. Especially as little is known about coastal retention in the Baltic Sea.
It is also relevant for policy measures, because official nutrient reductions do not account for nutrient retention in coastal waters. However, the conclusions could be more specific and, with additional discussions, could be more relevant for the entire Baltic Sea. While their methodology is valid, clear and relevant, it is difficult to be applied by any other institute studying the Baltic Sea's biogeochemistry, as setting a coastal model for one single region is costly (manpower, budget, time) and not all may have that possibility. However, by expanding their analysis further and adding more discussions on how the Order Lagoon compare to other coastal areas in the Baltic Sea, approximations for N and P retention could be estimated for other areas as well. Published results for the Baltic Sea mentioned in the paper have large discrepancies with specific coastal studies, and therefore new suggestions will be relevant to address including approximations for other coastal areas, especially for those similar to the Oder Lagoon. Their conclusions do mention that the method can be adapted for other regions beyond the Baltic Sea, but as written now is too vague. If this means adapting their model to a new area, it could translate as a costly task (time and computational) and not necessarily straightforward. In this sens, a better clarification on what is possible to extrapolate to other areas with this method is required. In other words, the main missing discussion in this manuscript is: Can the relation found in this study between loads and nutrient retention be used to estimate %retention in other basins? What can be used from these results to improve retention estimates in other coastal areas in the Baltic Sea and other Seas?

The Scientific significance of this study is good and after some revisions, will contribute significantly to our understanding of nutrient retention in coastal waters. The latter is poorly known and therefore, the content of this manuscript is very valuable. The methodology is clear and results relevant to biogeochemistry.

The scientific quality of the study is fair, the methods and approach are valid and innovative. However, it misses additional discussions, described in the specific comments below.

The presentation quality is good, the structure and figures are clear and well explained. However, it misses some additional figures, for example, in the validation section time series of model and observed oxygen should be shown, as well as more on the water column dynamics in the model. The oxygen section can also be expanded, as it is a major parameter affecting both N and P. Section 4.4 should be expanded or moved to the results section. For details, see specific comment below.

In short, I would strongly recommend this manuscript for publication after minor/medium revision.

Specific comments

Introduction
line 21: What is meant by extended residence time? Long? If so, replace by "long"
line 26: Please expand and add references. Why are there only a few coastal waters that can reduce nutrient loads in the Baltic Sea? Why not all coastal areas have that capacity? The Baltic Sea has many different types of coastal waters, please mention them as well and why would they be less

effective in nutrient retention.
Line 28: Why is the Order Lagoon the most critical?
Line 48: Are the MAI here referred per basin in the Baltic Sea or to the entire Baltic Sea?

Line 55: Models can account for nutrient retention in the Baltic Sea taking values from literature and making assumptions for unstudied coastal areas. They can give good enough nutrient concentration near coastal areas, but can certainly be improved. The problem is not that they do not adequately account for it, but that there is not enough information to adequately account for all different types of coastal areas of the Baltic Sea. In this regard, this study can really improve such model results. Here, I would suggest to focus more on the fact that there is missing information on this for the Baltic Sea (and other Seas) and discuss this further. Importantly, this has been already mentioned in previous modeling studies, for example in:

Eiola et al., 2011, they mention that "*the major differences between the nutrient supplies to the different models are due to the different assumptions on the bioavailability of phosphorus loads*", referring to the retention of P in coastal waters. They also have a dedicated chapter on bioavailable nutrient loads in their discussions, which should be mentioned in this study.

Ruvalcaba-Baroni et al., 2024, they refer to the little information available and what can be currently used: "*As the response to nutrient removal of different coastal types is poorly quantified... these factors are taken from previous studies in the Baltic Sea (Eilola et al., 2011; Edman and Anderson, 2014; Asmala et al., 2017).*" In their discussion, they also mention that "*one factor affecting detritus is the fraction of the organic matter coming from rivers that is actually bio-available and not directly retained in coastal waters.*" They assume a constant fraction for the entire Baltic Sea and say that "*the input of organic matter from rivers, especially nitrogen, could be improved by better accounting for river-specific organic matter retention in coastal waters*".

Adding this discussion, will make the point of this study stronger, as it "partly" answers a problem that has been existing since many years.

Results
line 112: The authors mention that "the model fails to reproduce the low salinities observed between 2004 and 2008, possibly due to an underestimation of runoff in the forcing", but do not show the forcing used. What evidence do you have for this? It could also be due to the atmospheric forcing (precipitation vs evaporation). Please add more explanation for this, a supporting figure with, for example the runoff used in the high resolution model and how it compares to observations could be helpful here (or in supplementary). Also, why not show salinity and temperatures profiles or some water column performance?
Line 114: The authors mention that "However, this discrepancy has yet to be confirmed". This statement falls a bit short. How would you confirm this and why not doing it in this manuscript? Please rephrase or add information.
Line 123: The authors say "the model successfully captures the decreasing trend in winter phosphate". Plotting the trend directly in the figure for both model and observations would make this point more clear. It will also be relevant to plot it for the other biogeochemical parameters. The significance of the trend can then be estimated, which, for example, it is not entirely clear for chlorophyll-a at KHM (is the decreasing trend significant or not?). This information would be valuable for the discussions later on and for line 129, where the model and observed chla are described. From Fig 4, it looks like the model has also a more pronounced decreasing trend than the observations at KHM, but very good match at C.
Line 131-132: The authors mention that "phytoplankton response to changing ambient light conditions and (???) may vary between 23 and 60". I think the authors missed writing "Chl:C ratios". Please add the missing part. If so, would the Chl:C ratio be that different between C and

KHM? Would this alone explain the discrepancies in model performance between C and KHM?

Oxygen dynamics (section 3.2)
This section is very relevant to the manuscript. While the model shows oxygen depletion, there is missing evidence on how depleted the oxygen is in the area. Why not add oxygen time series in bottom waters from both observations and model? Is there any oxygen measurements in this area? This requires further discussions either to show more evidence for oxygen depletion or mention the lack of observations and what are the alternatives to "validate" the model in such case. What are the references for oxygen depletion in this area? This is also relevant for the statement that the authors write in line 126 "..., the amount of phosphate released is insufficient to significantly increase the surface concentration." This is linked to oxygen, but also to mixing. Neither are shown. Water column dynamics can be shown in different ways (e.g., salinity and temperature profiles, mixing water depths, etc). It is difficult to judge how well the model represents the oxygen concentration in bottom waters or phosphorous retention in the sediments. It may as well be that the release of phosphorus is not insufficient in the model, but that the lower values in the surface are due to a biased runoff forcing that stratifies the water column too much in the model. Please add water column dynamic information (it can be in the appendix) and evidence for low oxygen concentrations in bottom waters from observations (e.g., time series for C and KHM of bottom oxygen). In figure 5, an additional map showing the actual model oxygen concentrations in bottom waters for the months with oxygen depletion and available observations will be valuable.

Line 164: Phosphorus retention in the sediments is also affected by bottom oxygen concentrations. It will be interesting to plot retention rates vs oxygen concentrations in bottom waters, especially for P. Could this (partly) explain the lack of relationship between rates of retention and P loads?

Line 175: How can the higher resolution model overestimate nitrogen retention?

Discussion
Line 185: The discussion relates to runoff forcing and would benefit from a runoff figure (as mentioned in comments above for line 112).
Line 195: See comment above on Oxygen dynamics (section 3.2). This discussion should be expanded.
198: There is much more literature available for P recycling in the Baltic Sea that can help tuning the model. Please add more references.

Lines 199-205: Relates to comment above on Oxygen Dynamics. This paragraph does not have any reference. How do we know the Order Lagoon experiences anoxia? Oxygen is a relevant topic for this type of study. Please expand the discussions on this and evidences for oxygen depletion available for the Order lagoon.
Line 205: So there are observations? Why not show them?

Line 220: Pleas add "As done in Eiola et al., 2011 and Ruvalcaba-Baroni et al., 2024, where they use a factor to account for nutrient retention" or similar (see comment above for line 55).

Line 221-222: Please expand. This has more potential and general suggestions on how to improve model estimates in the Baltic Sea can be given based on the relevant results in this manuscript. The big picture would be to say something on how these very nice results (based on chapter 4.2, 4.3 and 4.4) can be used by the larger scientific community and perhaps even policy. This could be discussed in a chapter on its own (4.5?).

Line 222: As mentioned before, not all coastal areas can be modeled the way it was done in this manuscript (it will be nice, but it will take time). Please expand on how your results could differ in other areas, based on bathymetry, residence time, oxygen content, etc and how they relate to published values (some of which are already mentioned in the introduction, but there are a few more, see discussions in Eiola et al., 2014). Or is it that every single coastal area in the Baltic Sea needs to be modeled separately to be able to estimate their coastal nutrient retention? Perhaps, but some relationships must be similar.

Line 240: There is no statistics shown in the paper, how do you know it is “not statistically significant”?

Line 258: This is an important statement that could be repeated in the conclusion.

Line 291: Please replace “.” by “,”

Interannual variability of discharge and loads and its consequences
This entire section is structured more as results than as discussions. Please expand (add discussion and references) or move to results.

Line 308: Please replace “emissions” by “inputs”

Line 310: How do you know that interannual variability is likely controlled by extreme events? Is there a reference or some evidence? It may seem obvious, but the relative or quantitative impact remains quite unknown for the Baltic Sea, as far as I know. Also what do you mean by “extreme events”? Storms, heatwaves and/or what else?

Line 315: How easy/difficult would it be to expand it to other regions in and outside the Baltic Sea? Please clarify.

320: I am not sure that the word “realistically impossible” can be used in this study, as it does not assess economical impacts and costs. Maybe rephrase to something less strong.

Figures
Figure 1. It would be good to mention already in the caption the navigation channel. It is quite obvious that there is a channel, but is not redundant to mention it. It would be also interesting to know when was this channel made (before the 60s?) to have an idea on how far back in time we should go to get “pristine conditions” in this area and to better interpret the observational data sets in this area as it seems to have a large impact for nutrient retention.

Figure 5. Please add the period mean in the caption.