

Interactive comment on “Modeling benthic-pelagic nutrient exchange processes and porewater distributions in a seasonally-hypoxic sediment: evidence for massive phosphate release by *Beggiatoa*?” by A. W. Dale et al.

Anonymous Referee #1

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This well-written, albeit somewhat long manuscript that presents modeling results of monthly varying benthic exchange processes at the sediment surface of the western Baltic (Boknis Eck). The original model was published by Dale et al. (2011) in *Estuarine and Coastal Shelf Science*. The current version is expanded and treats the integrated carbon, nitrogen, phosphorus, iron, sulfur, and oxygen diagenesis at the sediment surface using a one-dimensional non-steady state diffusion-advection-reaction model treating bioturbation and bioirrigation and transport through bubble irrigation. The dynamic model is designed so that annual closure is achieved ignoring potential

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debts and excesses extending into the following years. Except for one short section, I didn't see enough discussion on the advantages/disadvantages of this model aspect. Generally, this is a state-of-the-art model that produces well-constrained results, allows detailed scenario testing and provides the opportunity for lots of new hypotheses for testing physical and biological processes regulating benthic porewater and gas exchange. It was particularly welcoming to see that bioirrigation coefficients were independently constrained with bromide tracer experiments and that bubble irrigation was included.

The site is a well-investigated location in Eckernförde Bay in the western Baltic, for which, curiously, recent data such as the ones published here have not been available. This makes both the data and the model a possibly valuable contribution for Baltic Sea scientists and the broader scientific community. The authors have picked a timely topic and focus on the dynamics of phosphorus exchange at the sediment surface. They come to the conclusion that Fe-dependent sorption/desorption of phosphorus is insufficient to explain the late summer-fall fluxes across the sediment surface. Instead, they evoke an additional mechanism initially suggested by Schulz and Schulz (2005) in a *Science* manuscript for anoxic Namibian shelf sediments, but so far not demonstrated for organic-rich coastal marine environments in the Baltic. This mechanism involves phosphate-storing large sulfur bacteria such as *Thiomargarita* and *Beggiatoa* that take up phosphorus under mildly oxidizing conditions and release it during periods of anoxia to the porewater from where it diffuses or is irrigated back to the bottom water. It is important to note that the exact mechanism and regulation of this bacterial storage and release remain insufficiently understood. Furthermore, there is still insufficient evidence whether all large sulfur bacteria are capable of intermediate polyphosphate storage. For *Beggiatoa* this is currently only known for one marine strain and it is also not known whether *Beggiatoa* in the Baltic are capable of intermediate polyphosphate storage. In the present manuscript the authors suggest that seasonal blooms of large sulfur bacteria scavenge a significant amount of phosphorus during the early and summer season and release this phosphorus in the hypoxic period at the end of the year.

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The short-term release would lead to drastic increases in bottom-water phosphorus, which, according to the authors needs to be taken into account for understanding nutrient budgets in the overlying water column.

I have few troubles with the technical aspects of the model. The accompanying pore-water data are straightforward, solid, and of good quality. These data include routine porewater analyses of nutrients that were both taken with rhizons and by squeezing core sections. The former technique has become the common go-to method for its ease of use. However, great caution is advised when using rhizons on large porewater volumes such as probably used here. Then, smearing of porewater volumes across overlapping depth intervals can occur, if the porewater volumes are too large. This makes profiles look very good but changes gradients.

An important missing component of the present work is the fact that solid phase analyses are not presented. In addition, there are no data on abundance of sulfur bacteria or even rough constraints whether they (a) store nitrate and (b) store polyphosphate. Given the number and qualification of the co-authors on the manuscript to obtain such data, it is disappointing that they were not included, even more so since there is a real disconnect between the quality of the model-based results and discussion and these microbiological constraints. This is really the only major weakness of the manuscript, but given the centrality of the Fe-oxyhydroxide-P sorption mechanism as opposed to bacterial P storage I consider this a significant deficit. I would like to see that a revised manuscript provides such data in support. With these data the manuscript can go a long way further and can be a really significant contribution and not merely a hypothesis.

The manuscript is well structured and all sections are clearly written. For the review it would help if line numbers were included to refer to specific passages in the text. The model description is very long and the authors may consider shortening some aspects of it and moving them to the appendix, for example the detailed description of the setup of the bromide experiments and the model design to obtain bioirrigation coefficients.

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This would improve the readability and flow of the manuscript.

Text comment: p.15 first paragraph: "Although bottom water nutrient concentrations also fluctuate throughout the year in the bottom water (Dale et al., 2011), the trends are not obvious and ignored in the model. Thus, all bottom water solute concentrations except O₂, Br⁻ and SO₄²⁻ were fixed."

I was bit surprised by that statement. If late-summer/fall release of phosphate significantly contributes to the benthic phosphorus inventory and the proposed mechanism is significant, should one not at least have tried to compare the time series bottom water data with the model-generated fluxes? And then, if necessary, included them? In addition, could varying bottom water concentrations have affected the the different benthic nitrogen cycling processes? I would appreciate if the authors could comment on this aspect.

Interactive comment on Biogeosciences Discuss., 9, 11517, 2012.

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