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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 #2

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This paper presents a comprehensive data-modeling study on benthic-pelagic coupling at a seasonally hypoxic, shallow coastal location in the SW Baltic Sea. The paper builds on a previous study by Dale et al. published in *Estuarine Coastal and Shelf Science*. As outlined in the paper, there are only very few detailed data sets documenting changes in pore water composition due to seasonal hypoxia and this makes this data set an important one. The reactive transport modeling of the data and the assumptions made are well-described and the results are clearly presented. Especially the role of episodic bubbling of methane gas and its potential consequences for pore water transport is

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interesting and well-argued.

The major focus of the manuscript lies on the dynamics of phosphorus and the controls on its benthic exchange. In the introduction, the authors summarize various potential sources of pore water phosphate in sediments, and based on their modeling, make the case that the large diffusive fluxes of phosphate at their site are likely due to release from *Beggiatoa* and not from either iron-oxide bound P or organic P. This conclusion is much too speculative given that the authors do not include any data (nor modeling results to match) on sediment P forms in their manuscript. Essential data on the abundance and distribution of *Beggiatoa* in the sediment and the species involved are also missing.

Instead, the main argument appears to be that the pore water phosphate profiles cannot be reproduced when using literature P:Fe ratios of 0.1. However, P:Fe ratios may vary in natural environments. Thus, while the value of 0.1 may indeed hold for several continental shelf environments, P:Fe ratios depend on the conditions of formation of the Fe-P and are known to range up to values of 0.5 (e.g. Gunnars et al., 2002; GCA). While a constant P:Fe ratio is often used in modeling studies for simplicity, ratios also are known to vary with depth in the sediment. This may impact the ability to model the sharp peaks in PO₄ often observed in surface sediments.

Given the large effort that the authors have put into the data collection and modeling, I would suggest that the authors carry out sediment P analyses, ideally for several moments in time to demonstrate whether or not there is indeed seasonality in sediment P and whether Fe-P and organic P can indeed be excluded as the major source of pore water P in autumn.

Other selected issues/Details:

1. The paper is very long and its readability could be improved by shortening the model description section and moving some of the detail to the supplementary information.

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2.P. 11518. Line 2: a 2 m deep channel or 28 m as indicated in the methods section?

3.P. 11518. Line 11. “hydrogen sulfide did not accumulate in the porewater” It would be good to add here at what depth, since figure 4b shows there was sulfide in the porewater.

4.P. 11518. Line 24. Typo in “through”

5.P. 11518. Line 25. “Further experimental studies are needed to strengthen this conclusion and rule out Fe-P and organic P as candidate sources”. It is a relatively small effort to constrain this further by collecting the appropriate data and I urge the authors to do so.

6.P. 11519. Line 1-4. These lines “Our results suggest . . .waters” should be removed. This conclusion is not sufficiently supported by the data shown and is pure speculation.

7. P.11520. “..release of iron-bound P has been proposed as a major source of phosphate to the overlying water column..” Here the term “has been proposed” should be replaced by “has been demonstrated”. There are many papers demonstrating this.

8. P. 11520. In this section, the authors could explain the term “preferential regeneration of P relative to C”. That term is used in the abstract and later in the text but is not adequately explained.

9. P11525. Line 13. Remove “the” before “mass”

10. P11536. I would suggest to remove the sentence “we detail the areas where more research effort is needed to accurately quantify iron and phosphorus cycling”. Instead, the authors should make a better attempt to quantify this themselves.

11.General modeling approach: the authors do not include authigenic apatite formation in their model. It would be appropriate to include why they think this process is not relevant for their study site.

12.P11551. I would suggest to remove “The model has led to new ideas...” That

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release of P from Beggiatoa could potentially contribute to benthic fluxes of P is not a new idea.

13. P11552. Line 15. It would be good to include the data on the distribution of Beggiatoa in this paper and information on their ecology.

14. Fig. 6d and Fig. 8 are pure speculation

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