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Interactive comment on "A Baltic Sea estuary as phosphorus source and sink" *by* Jakob Walve et al.

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Authors' response (AR) to comments by referee 1 (RC)

RC: "Walve et al. (bg-2017-496) present an interesting study where phosphorus dynamics in an estuary in the Baltic Sea are quantified using a box-model approach. The study period spans from 1968 to 2015, and has a seasonal temporal resolution. Box model budgets for water and salt are calculated from measured freshwater inflow and salinity from two stations. These water budgets are further used to infer phosphorus fluxes to and from the study estuary. In general, the study comprises of a straightforward model exercise where abundant monitoring data has been used to build the model. One of the main findings is the identification of the sediment as a sink of phos-

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phorus in spring and as a source during summer and autumn. Overall, the manuscript is well written and easy to follow." AR: We are pleased that our study was overall found interesting and easy to follow, and are thankful for the constructive comments. Although the finding of the sediment as seasonal source and sink is important, we consider the finding that seasonal P release is larger than legacy P release, and that legacy P release was clearly demonstrable only for a few initial years, as the main results. Moreover, seasonal P release nearly equals the sink meaning that annual net retention is very low.

RC: "The manuscript would benefit from a clear formulation of scientifically relevant research question and/or hypotheses, which would potentially be relevant to other systems as well." AR: We agree. As is also suggested in a comment below, we will add the requested text in the final paragraph of the introduction. Research questions include: 1. In an estuary that has long been subject to a very high P loading, is there a long-term influence of legacy P once the load has been drastically reduced? 2. What is the current annual P retention of the area? 3. To what extent does oxygen control seasonal P release and annual P retention? Can we expect improvements in oxygen conditions to increase long-term P retention? 4. What is the effect of the temperature increase from global warming on P release?

RC: "What are the main discrepancies compared to other studies, and why adding one more model study of this system is justified?" AR: This is the first temporally highly resolved dynamic box model study of the Stockholm IA that follows long-term P balance, including the period of change from high to low P load, and the first with seasonal resolution. Moreover, we link P retention to oxygen concentrations and oxygen consumption (from an oxygen budget). We find lower P retention than previous studies, even periods of net P export. We find that oxygen consumption from degradation of organic matter is predominantly directly linked to P release. We also find that low oxygen can promote an additional release of P. We will clarify this justification of our study in the final paragraph of the introduction. Discrepancies from other studies (e.g. in mean

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annual P retention) are presently dealt with in the discussion, but will be clarified also in the introduction.

RC: “The title is very concise, maybe even too much so. The seasonality behind the source and sink actions could be introduced in the title already” AR: We suggest expanding the title to “A Baltic Sea estuary as phosphorus source and sink after drastic load reduction: seasonal and long-term mass-balances for the Stockholm inner archipelago 1968-2015”. This emphasizes the long-term results and gives the exact location of the study.

RC: “Abstract P1, L10: In effect, two box models are used in the study, and the box models are vertically layered. This should be made clear here as well.” AR: Agree, we will make it clear already here that we use two models, and that both are vertically layered.

RC: “P1, L10: Here and elsewhere: there is inconsistent use of terms “box-model” and “box model”. Authors should choose either one and use the same term throughout.” AR: We now consistently write “box model”.

RC: “P1, L20: Replace stores with pools or storages.” AR: Corrected.

RC: “P1, L23: Sentence is incomplete, contributed to what?” AR: Contributed to improved oxygen conditions. We have removed lines 21-24 that deal with reasons for oxygen increases in order to shorten and focus the abstract (as also suggested by reviewer 2). We replace them with a short sentence about temperature effect on oxygen consumption.

RC: “Introduction P1, L31-32: Sentence is vague and does not read well, please rephrase.” AR: We agree, and will rephrase to “The occurrence of hypoxia is also influenced by variations in deep-water ventilation, which depend on basin morphology and meteorological forcing (Zhang et al., 2010).”

RC: “P3, L1: Remove “coastal”, as estuaries are by definition coastal. If estuaries are

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to be emphasized here, then the sentence should be rephrased." AR: Corrected and rephrased.

RC: "In general, estuaries have short residence time in the mixed layer, but is that the case in the Stockholm Archipelago? I would assume that the stratification and the relatively deep basins lead to high water residence times in the water masses close to the bottom, compared to the fast water exchange in the surface layer." AR: The water mass in the upper 10 meters is relatively large, so the difference is not substantial. The fresh-water residence time is in the order of weeks to months. We add a reference to Engqvist and Andrejev (2003), who give estimates of water retention times. This reference is cited elsewhere in the manuscript and is in the reference list.

RC: "P3, L6: "deteriorated badly" does not read well, rephrase." AR: Changed to: ... deteriorated seriously ...

RC: "P3, L32: Is this not the case with all box models, that they are dynamic? Or is this a special case of a "dynamic" box model? If so, it should be elaborated here." AR: We are referring to the relatively high temporal resolution and the imbalanced instantaneous salt inflow and outflow, i.e. it is not just based on annual means assuming steady-state. We rephrase to clarify this.

RC: "P4, L1-4: See above; here I would expect to see a set of research questions, objectives and/or hypotheses which would clarify the aim of the study and also make it easy to follow throughout the manuscript if the goals are met in the study." AR: We agree. We will add the requested text. See response to second comment above.

RC: "Methods P4, L7: should be "comprises of"." AR: We do not agree, and neither does our dictionary.

RC: "P4, L20: Does this mean that temperature was measured from inside the sampling vessels? Please clarify in text." AR: Thermometer inside water sampler, then in-situ with CTD. Will be clarified.

RC: “P5, L8-13: Why four layers? What is the justification of this number of layers and the depth ranges of those layers? Please elaborate.” AR: We used a box model with fixed depth layers and need three layers to separate deep water (primarily influenced by inflowing salt water) from a pycnocline depth layer and surface water. We further divided the surface layer since at high fresh water flows there was occasionally a shallower halocline. This is elaborated on page 6, line 20-24. We move these lines to the first paragraph in section 2.3 and add explanation of other layers.

RC: “P6, L20: Replace at with during.” AR: Corrected.

RC: “Discussion P13, L21: Replace scale with resolution.” AR: Changed.

RC: “P13, L21: The reader should be reminded at this point what is IA.” AR: Agree. Corrected.

RC: “P13, L22: “. . .and has poor long-term P retention..”does not read well, rephrase.” AR: Rephrased. “Poor” is changed to “low”.

RC: “P13, L26-27: When both models yield similar results in P budgets, what is the motivation to use both? If the poorer prediction of water exchange in the mean model is not important (indicating decoupling between surface and bottom water masses), then why include the boundary model at all?” AR: The boundary model should be more correct if looking at the P balance of the inner area. However, the mean model is more representative regarding the internal dynamics, based on central stations data. We think we have to present all the results to be clear. The similarity of the results of both models is in itself an interesting result that adds credence to the analysis.

RC: “P14, L22: Replace yearly with annual.” AR: Corrected.

RC: “P14, L18-19: This is an important factor causing bias in sediment studies; in practice, most sediment cores are taken from sedimentation areas (local bathymetric depressions) where sedimentation rates are higher than in their surrounding area in general. This matter should be elaborated here.” AR: We elaborate in lines 20-25, but

will add also this aspect. We rephrase this part and the rest of this paragraph to make it clearer (in response also to comment by reviewer 2).

RC: “P15, L10-11: What is the significant difference between the inner and outer part here? Why do you think that the polychates do not have an effect there? This should be elaborated here.” AR: Other factors than the polychaetes can explain the observations in the cited paper. The inner part of the IA has a complex water circulation. We conclude we cannot see an effect, but lack a solid basis for further speculation.

RC: “P15, L13-21: So where does the TP in the upper layer go? It seems like sedimentation is suggested, but it is vaguely formulated and should be made clearer.” AR: Agree, we modify the second sentence.

RC: “P16, L14: Iron obviously plays a major role in the P cycling in the sediments, therefore the iron fluxes and concentrations presented on page 17 should be brought up already here. This would better justify the lengthy discussion about the role of Fe in the system.” AR: We moved the sentence on Fe loading to the area to page 16.

RC: “P17, L15: Is this mean annual concentration? Should be made clear.” AR: Corrected.

RC: “P17, L19-20: Due to salinity differences between the surface and the bottom layers in the system, this is more like a rule than an exception, is it not? Therefore the assumption of water masses being well-mixed seems not justified.” AR: There is hardly any horizontal salinity gradient in the deep water, so this is likely not a problem. The vertical salinity differences are handled by the model.

RC: “P17, L32: Replace yearly with annual.” AR: Corrected.

RC: “P18, L10: Here, P is presumed to be deposited as organic P. Why iron-bound (inorganic) P is not considered here, as it clearly is a significant pathway for P sedimentation as presented before.” AR: The intended point here is that the organic P load was very likely higher in the 1970s than in later years, which should contribute to high

oxygen consumption, and mostly low O2 concentrations, unless O2 inputs are particularly large. (There is probably significant inorganic P sedimentation too, but this will not contribute to oxygen consumption.) We rephrase to clarify.

RC: “Conclusions Overall, this section is too long and would benefit from shortening. Ideally, conclusions are a concise presentation of the key findings, which relate to the research questions and/or hypotheses presented earlier. For instance, the seasonality aspects could be the main concluding remarks.” AR: Yes, we suggest moving most of the last two paragraphs to new section 4.6.

RC: “Tables Table 1: Replace yearly with annual in the caption Table 2: Replace yearly with annual in the caption” AR: Corrected.

RC: “Figure 1: Sampling points can not be distinguished well enough from their background, so they should be made larger and with better contrast.” AR: Corrected.

RC: “Figure 4: These time series seem to have an increasing trend over the study period. This could be emphasized by plotting a regression line of some kind to the plots. This would also allow the quantification of the annual increase in observed values.” AR: In principle yes. But then we should for consistency add such lines in many of the following graphs too. Many changes are not steady trends but rather short term fluctuations and thus a regression line could be a bit misleading. We prefer mentioning it in the text.

RC: “Figure 5: In panel (c), the net values are hard to read because of the scale issue. For that reason, the net values should be shown only in the separate panel (d). The motivation to show also the values from the mean model in panel (d) is unclear.” AR: We agree net values in panel (c) can be removed. We think results from the mean model are important for comparative reasons. However, we have results of the mean model in Table 3 and can remove it in fig. 5d.

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