

Interactive comment on “Phosphorus recycling in sediments of the Central Baltic Sea” by L. Viktorsson et al.

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This investigation presents an exciting new approach to studying benthic nutrient fluxes in the Baltic Sea, for which I applaud the authors. The topic is an important one, since basin-scale biogeochemical models of the Baltic still struggle to capture many details of this diverse and dynamic coastal system. I fully support the endeavor to refine estimates of burial and regeneration fluxes of phosphorus (P), as a basis for improving these models and better predicting the response of the system to external stresses. However, I feel the need to defend somewhat the criticism of alternative approaches to studying burial and regeneration fluxes in the Baltic, with which I have recently been involved (Jilbert et al., *Biogeosciences* 8, 2011). Furthermore, I would like to highlight for readers of *Biogeosciences* that many aspects of the two studies in fact agree rather

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well. The principal new conclusion, as I see it, is the upwards-revised estimate for Baltic-wide P regeneration from anoxic bottoms, and the resulting downwards-revised estimate of P burial efficiency. If true, this is of course very interesting. However I would like to see a slightly modified approach to the calculation of this value, and if it holds up, a modification of the discussion as to why previous estimates may be insufficient. I wish the authors success with their revisions, which should be considered moderate rather than major.

Comments (Page 15XXX, Line)

469, 18 and 471, 6: It is somewhat misleading to describe (anti)correlations between sediment and porewater profiles in this case. The sediment profiles record not only the diagenetic decay of reactive constituents (e.g. organic C, organic P) but also the changes in the incoming fluxes of these constituents through time. The EGB experienced a major shift in redox conditions and productivity during the eutrophication of the Baltic in the late 20th century, which is partly responsible for the higher P concentrations in the upper sediments and hence the observed anti-correlation with the porewater P profile. This factor will also come back in the discussion of P burial efficiency.

471, 10: From Figure 4 it is difficult to see whether porewater Fe, or porewater Mn, better correlates with phosphate at sites A, B and C. I suggest to show fewer variables per plot. Also, can the authors comment on the possible water-depth dependency of Fe and Mn (and associated P) behavior within this group of sites? I can imagine based on the results of Dellwig et al. (GCA 74, 2010) that the gradients of surface-sediment Fe and Mn oxide concentrations across the redoxcline are offset vertically.

471, 15-20: It is not necessarily valid to use the present-day porewater Fe:P ratio from the deep anoxic sites to predict the potential trapping of P at these sites upon oxygenation. One reason for this is that the Fe currently in the porewaters at these sites would not be the primary source of Fe for the oxyhydroxides which would accumulate

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in the sediments upon reoxygenation. Currently, the Fe profiles of sites D, E and F (Fig. 4) show extremely low concentrations because only a small flux of Fe oxyhydroxides reaches the sediments of the deep basins, most of which is rapidly sulfidized to FeS or FeS₂ with almost no intermediate accumulation of porewater Fe. If the basin was reoxygenated, there would be an increase in the flux of Fe oxyhydroxides to the sediments due to their stability during lateral or vertical transport. Should the basin remain oxygenated, a dynamic surface-sediment cycling of Fe and P would then be established which would control the potential efflux of P (see Reed et al., L&O 56, 2011).

472, 15-25: The authors claim that DIP efflux in the deep basins increases with increasing water depth and thus decreasing oxygen concentrations, and that this is in contrast to previous observations, including those reported in Jilbert et al. (2011). I would like to take this chance to clarify how we (the authors of that study) believe this relationship works, and to show that the current study does not in fact present any relationship which ours did not. In Figure 2 of Jilbert et al (2011), you can see that the diffusive flux of DIP at Group 2 sites (all sites below 90m) seems to increase with depth over the first 4 sites (95, 123, 169, 191 m respectively) but then fall again at the deepest site (238m, I admit that this detail is somewhat lost in the busy figure). Similarly, in the present study (Fig. 7), the sites below 90 m show an apparent upwards trend of the DIP flux with increasing water depth, but this trend is not perfect (e.g., site E, 170 m has a lower flux than site D, 128 m). We suggest that the general trend is related to preferential remineralization of P from organic matter under anoxia (as supported by our N:P and C:P plots, Figure 5 in Jilbert et al., 2011), while the anomalies are related to spatial differences in the organic matter flux to the deep basin sites. This explanation is perfectly applicable to the results of the present study, so I would suggest to modify the text to highlight these similarities. It is also certainly not true that the 'depth range of stations' in the Jilbert et al. (2011) study was 'too limited' to assess these relationships. In fact the depth ranges of the two studies are very comparable, re-emphasizing the point that they have delivered similar results.

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473, 1-20: To issue a further defence of Jilbert et al. (2011) and previous Fick's Law-based estimates of diffusive fluxes, I do not believe that the magnitude of the P fluxes we estimated is very different from the present study. In fact, as the authors state (473, 5) our study showed a range of 0.01-0.8 mmol m⁻²d⁻¹, which resembles the x-axis of Figs. 8-10 rather well. Clearly, a different set of sites will always yield a different set of precise values, regardless of the method used. Considering the anoxic sites only, the present study does indeed record several very high values (>0.4 mmol m⁻²d⁻¹), but our site F80 also showed a P flux of ~0.4 mmol m⁻²d⁻¹ (Fig. 2 in Jilbert et al., 2011). The anoxic sites studied by Hille et al. (2005) were generally less 'reactive' than our F80 (i.e. with less degradation of organic matter, as seen in the lower porewater phosphate concentrations in their Figure 3 with respect to ours from F80 (see also Table A1 in Jilbert et al., 2011). Hence, I believe that the underestimation of Fick's Law-based diffusive fluxes due to sampling resolution is much less significant than the authors suggest. It is unfortunate that the authors did not attempt to perform both methods on their sites in order to quantify the offset. Until this can be demonstrated, I urge the authors to withdraw some of their criticism of the existing methods. What can certainly be ruled out is the possibility of oxidation artifacts in our study, or in that of Mort et al. (2010). Not only did we transfer our cores within minutes to an anoxic glove box for slicing, we also used microelectrodes to measure the oxygen penetration into the overlying water of parallel multicores standing on the lab bench. These remained strongly sulfidic for several tens of minutes, showing that the cores we sliced experienced no oxidation at all.

475, 20-30: I support the authors' comparison of DIC and DIP fluxes to highlight the role of organic matter degradation in controlling P release from the deep basin sediments. I would be very interested to see how the DIC:DIP flux ratio varies with increasing water depth. One of the important conclusions of our study was that the NH₄/PO₄ flux ratio declined with water depth at anoxic sites, showing the clear effect of preferential remineralization of P in the surface sediments (Figure 5 in Jilbert et al., 2011). Can the authors comment on the advantages/disadvantages of studying DIC with respect to

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NH₄ as a tracer for organic matter degradation? For example, what would be the effect of a shallow sulfate-methane transition zone (as present at some deep basin sites) on the flux of DIC?

Sections 4.4. and 4.5: I agree with the authors that their estimates of P burial efficiency are substantially lower than previous estimates. I also agree that this may require a downwards-revision of the Baltic-wide burial efficiency of P, if it can be shown that a large fraction of the deep basins sediments behave similarly to those presented in this study. But I am unsure about the validity of the calculation used here. As stated above, the sites of Hille et al. (2005) were probably a lot less reactive than those of the present study. So, to use the burial rates from these sites to estimate the burial efficiency at the sites of the present study is almost certain to yield an artificially low number. On 478, 26, the authors state that they used a total P concentration 'at depth' to estimate the burial rate of P at their sites, but later choose not to use this number in the calculation of burial efficiency. I would urge the authors not only to use the numbers from their own sites, but moreover to attempt to estimate a 'modern' P burial rate at these sites, using ²¹⁰Pb-derived mass accumulation rates if these are available. As stated above, the P burial rate has probably increased very recently at these sites, so using the background accumulation rate at 10 cm depth (potentially before the onset of modern eutrophication) probably underestimates modern P burial. On the other hand, using the concentration at the core-top would clearly yield an overestimate, due to the presence of a 'fluffy layer' of sediment which will release some P before burial. I agree that it is difficult to select a suitable value, but in our experience most deep basin sites show a two-step profile of organic C and organic P concentrations in the upper decimeter, in which the lower step records the shift to eutrophic conditions in the late 20th century and the upper step represents the base of the fluffy layer. This pattern seems to be visible in sites E and F (Fig. 2), so I would recommend to use the concentrations at ~ 2 cm depth, with locally-derived modern mass accumulation rates, to better constrain the burial rate of P and hence the burial efficiency.

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If, after these adjusted calculations, the P burial efficiency is still much lower than estimated by Hille et al (2005) and other studies, I believe this conclusion should be highlighted as the principal message of the paper. However, the authors will also need to show a valid means of extrapolating their efficiencies to a wider region, if the Hille et al. (2005) estimates are considered to be correct for the sites in that study.

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