

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

**Anonymous Referee #1**

Received and published: 16 November 2012

The investigators present an impressive number of in situ flux measurements and pore-water profiles to quantify the fluxes of DIP to/from the sediments in the Eastern Gotland Basin. These fluxes are then upscaled to the Baltic Proper based on the distribution of bottom water oxygen levels, and reveal that the internal loading of DIP in this area has been grossly underestimated in the past and is much greater than point source P inputs. This is the main result of the paper and is of great significance if it turns out to be correct. In situ fluxes in the Baltic Sea, and the marine environment in general, are scarce and deserve publication. The authors also look at the C:P ratios of the inorganic fluxes and attempt to quantify P burial efficiencies. Overall, it is a solid paper based on sound measurements. The extent of the interpretation is somewhat limited by the relatively few variables that have been reported. For example, it would have been nice to see how ferrous iron and oxygen fluxes fit into the story, and the conclusions would

C5701

be significantly strengthened with organic C and P data. Are these data unavailable? Other comments follow.

Comments (Page 15XXX, Line)

463, 11: The stated aim to investigate the effect of bottom types on benthic DIP fluxes has not been achieved. Instead the focus on oxygen levels on P fluxes is much more strongly put forward. This is a shame, since I really would have liked to have seen the role of sandy versus muddy bottoms much more clearly differentiated since this is very poorly understood area. It seems that, perhaps by coincidence, the accumulating (muddy) bottoms are all found in the anoxic sections, meaning that the role of sediment type cannot be distinguished from that of oxygen concentrations. This makes the definition of erosional versus accumulating sediments somewhat redundant based on the data presented. The authors need to comment on this, particularly since this could have big implications for their upscaling based on oxygen levels only. Can it be demonstrated that the main control is indeed oxygen and not sediment type based on the data presented? For example, sandy sediments may be more efficient at flushing DIP from the sediment. Following on from this, and given the large amount of data gathered, it would be nice to explicitly see in the conclusions where the major uncertainties in the controls on P fluxes are to be found, i.e. bottom type versus oxygen, and hence in what type of setting where more measurements are needed.

The data all originate from the August-October period. This opens the question regarding seasonal variability in their data which is not at all mentioned. In the conclusions (p480,L16) the authors state that the rate of deposition and degradation of organic matter is the main control on DIP fluxes. I realise that nothing can be done about the seasonal bias, but some commentary is warranted since POC fluxes are strongly seasonal. Can published regional models provide insight into the seasonality of POC fluxes to the seafloor and thus the natural variability in DIP fluxes?

464, 11: I don't understand the term 'transport bottoms'. How do these differ from

C5702

erosional bottoms?

464, 26: Except for the permanently anoxic sites, it is not clear in Table 1 which stations have oscillating oxygen concentrations. I expect that all stations will show some degree of oxygen oscillation due to the combined effects of circulation changes and biogeochemical processes. Do the authors mean oscillating between oxic-hypoxic? Furthermore, the definition of permanently oxic, hypoxic etc is somewhat arbitrary since it is only valid for the time of sampling. Again, how could these potential oscillating oxygen concentrations affect DIP fluxes? Could this be important? Please comment.

468, 21. No explanation is given as to why TP concentrations increase again with depth. Is this apatite formation, Fe-P precipitation or non-steady state diagenesis?

471, 20: I don't see how the authors arrived at 5-10 %. Please clarify. Also, the authors use data from the deep anoxic stations to illustrate the potential re-oxidation of authigenic Fe-P, but these deep stations will not likely be exposed to oxidation. Have I missed something here?

475, 30: Suggest to rephrase: Previously published C:P ratios include 30 (Ingall), 39 ....

476, 4-9: I believe the authors are referring to enhanced organic P regeneration relative to organic C in sediments underlying anoxic bottom waters, but the argument is presented in an awkward way and the sentence beginning line 3 does not support the argument. As mentioned above, organic C and P data would really bolster their argument and help to rule out P release from inorganic fractions. Please include these data if available.

I recommend switching section 4.4 and 4.5, since the new estimate of internal loading should be the main and final result of the paper.

Table 1. Mass percent is an unusual way to report water content. Why not volume percent (i.e. porosity)? Table 1. BW not defined in header.

C5703

Fig. 6. The Big landers have 9 syringes each for sampling, but in 2 of the panels less than 9 data points are included. Why is this?

Fig. 7: ...sorted by bottom... Fig. 7: ...x-axis are (from top to bottom) bottom water...

Fig. 7: ...water depth in m

Linguistic comment: on several occasions in the paper, "on anoxic bottoms/stations" is mentioned. It is more correct to write "at the anoxic bottoms stations" or "for anoxic bottom waters" etc.

---

Interactive comment on Biogeosciences Discuss., 9, 15459, 2012.

C5704