

Interactive comment on “Benthic phosphorus cycling in the Peruvian oxygen minimum zone” by U. Lomnitz et al.

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General comments

This is indeed a very interesting study which attempts to close a budget for phosphorus cycling in Peruvian OMZ sediments. Its major strong point is the coupling of suspended particulate analysis with sediment, porewater and flux incubation data. The authors should be commended for this comprehensive approach which, as they state, has not been attempted often enough in previous studies of P cycling. In general I am satisfied that the study will be worthy of publication in Biogeosciences after minor to moderate revision according to the comments below.

Specific comments

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Major:

- One of the main challenges associated with the approach employed here is that the fluxes being measured are generally not in steady state, and a single sampling moment (i.e. one cruise) will struggle to tell the full story. The authors have acknowledged this and indeed conclude that their budget cannot be closed for this reason, but I agree with Peter Kraal's comment (re. P80 L18-26) that more specific contextual information about the variable redox regime of the study site should be included earlier in the paper. The authors refer to the studies of Guitierrez et al., 2008 and Schunk et al., 2013 in section 2 but perhaps it would be good to show in graphical form (e.g. incorporated into Fig. 1) how much the redox conditions can actually change at these sites, and how frequently. For example, can potential loading-unloading cycles for the bacterial P 'capacitor' the authors postulate be identified from past data?

- I recommend that the authors present not only the various C/P ratios (e.g. Fig. 2) but also the raw C and P data in graphical form (these are currently given in table form and only in the supplement). This is important because in both the water column and sediments, trends in C and P concentrations may help to indicate transformations (e.g. breakdown of organic matter, precipitation of CFA), which are relevant for the interpretation of the data.

- In the case of the water column data, I miss a conclusive statement from the authors about what they believe the PIP phase on their filters actually is. The authors refer to the paper of Sanudo-Wilhelmy et al. which describes P adsorbed on the surface of living *Trichodesmium* tissue, probably associated with hydrous Mn oxides. There are several questions which follow from this: How representative are the conclusions of that study in a high-productivity upwelling system such as this one, considering the different primary producer assemblages? And if there is indeed a major fraction of surface-adsorbed P associated with surface-water POC, is it realistic that P will be preserved in such a form as this material sinks through the water column?

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The water column POC data from the offshore sites (Table S1) suggest export of ~5% of surface-water POC to the deep waters at Station VI (see POC at 10m vs. 240 m) and ~20% at Station VIII. What can the authors say about the quality of the exported POC, and what does this imply for the fate of any surface-adsorbed P? Furthermore, how would hydrous Mn-oxide bound P be expected to behave while sinking through the anoxic water column?

It is indeed remarkable that POC/TPP is so close to Redfield throughout the water column. Could there be a role for conversion of POP to polyphosphates as suggested by Diaz et al. (Global Biogeochem. Cycles 26, 2012) or to CFA (Jilbert and Slomp, GCA 107, 2013), during the decay of organic matter in the water column? If P-bearing molecules are preferentially broken down, and P is then stored in these forms, POC/TPP could stay quite constant as observed here. In summary, I think a discussion of these various options for the PIP phase is essential, including statements about the expected extraction behavior of the candidate phases (e.g. are polyphosphates expected to count as POP or PIP?).

- In the case of the sediment data, I would also like to see the raw plots of TPP, POP and PIP to see if there is evidence for transformations with depth, such as the precipitation of CFA as observed in many open ocean studies (Ruttenberg and Berner, GCA 57, 1993). As stated by Peter Kraal it is important to distinguish between different mechanisms of CFA formation and with all the data available here it is possible to say something more about this.

A further concern I have about the sediment data is the way in which the burial fluxes of P have been calculated. This has been done using average P concentrations in the top 10 cm and mass accumulation rates (Page 66). In the supplemental data it can be seen that TPP concentration decreases from the sediment surface downwards, so the ultimate burial flux is probably better calculated from the value at 10 cm rather than an average including the surface sediments. In fact the authors state that a previous related study (Dale et al., 2015) employed exactly this approach to determine carbon

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budgets. How would the P budget change if the same was done here?

Minor/technical:

Page 61, Lines 16-20. What was the pH during analysis? The way this is phrased, it sounds like you carried out the analyses at neutral pH but normally this is not done. Please clarify.

Page 62, Line 25 onwards. Related to my earlier comment, it would be good to state somewhere which P phases are expected to dissolve in the 1M HCl (pre-combustion) extraction. Especially in this study where intracellular polyphosphate pools, and surface-adsorbed P, may be quantitatively important, the expected extraction behavior of different phases should be clarified.

Page 71, Line 24: 'where' should be 'were'.

Page 72, Line 28. 'or takes also place' should be 'or also takes place'. Also remove comma after 'Since'.

Page 72, Line 29. 'comparable' should be 'comparatively'.

Page 73, Lines 10-20. Here I would state more clearly the role of Fe in sedimentary P cycling in oxic conditions. i.e. the observation that POC/TPP in oxic sediments is typically 10-50 is primarily due to the association of P with Fe (oxy)hydroxides in surface sediments after release from decaying organic matter. Also check the reference given as Baturin (2007). In the reference list the only article with this first author is Baturin and Savenko, *Oceanologia* 37, 1997.

Page 77, Lines 1-10. Again, add some more contextual statements about the expected role of Fe in P cycling in the OMZ environment. Iron availability in the surface sediments should vary greatly with water depth through the OMZ but this is not expressly discussed here.

Page 77, Line 23. 'similar like' should be 'similarly to'.

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Page 77, Line 29. 'supports' should be 'support'.

Page 80, Lines 1-3. Once again I agree with Peter's comment, that the mechanism of the proposed foraminiferal P uptake should be discussed in more detail. If true this could be an exciting new process in sedimentary P cycling so it deserves more attention.

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