

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

P. Kraal (Referee)

p.kraal@uu.nl

Received and published: 16 November 2015

General comments

With great interest I have read this manuscript, in which the authors investigate phosphorus cycling in sediments along a depth transect in the Peruvian oxygen minimum zone. The study is of great interest, in particular because of the combined characterization of water column particulates and sediments as well as measurements of benthic P fluxes. This approach allows the authors to compose benthic P budgets in Peruvian margin sediments, and to show that there must be an additional sedimentary P source (seasonal release from bacterial mats) to explain the high benthic P fluxes. The experimental approach is sound, the manuscript is well-written and the findings provide

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valuable insight into the functioning of the biogeochemical P cycle in oxygen-depleted marine systems. I recommend publication of this manuscript in Biogeosciences after minor revision. Below you will find some suggestions that might be of use to the authors.

Specific comments

It would be nice if the porewater PO₄ profiles are referred to before the freeze-thaw experiments as background to the resulting calculated PO₄ fluxes.

P56, L21. A few more words could be spent to clarify the statements regarding phosphorite formation.

P57, L21-22. Perhaps some nuance is needed here. Under certain conditions (such as nutrient-rich upwelling regions off Peru and Namibia), extensive (microbially-mediated) CFA authigenesis has been found. And during burial below the oxygen penetration depth, reductive release of P from metal (oxyhydr)oxides triggers apatite formation, often in disperse form at low concentrations (sensu Ruttenger, Slomp). These are two very different, anoxic mechanisms, where the latter may be more representative for non-upwelling oceanic settings.

P68, L7. “except slightly increasing ratios close to the seafloor” Clearly state that this refers to the difference between the deepest water column sample and the sediment surface (as there is a general decrease between the deepest water column sample and the one immediately above it).

P68, L9. Description of the trend at Stat VIII deserved a little more detail. Truncated profile, cyclicity?

P68, L23-24. Unclear what is meant here; I do not see any consistent strong changes in the deepest water column sample.

P70, L6. “that could be triggered by the TPP”, rephrase and mention mechanism/process

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P73, L21. Here, phrasing could be more clear: enhanced POC preservation and/or enhanced P release could lead to higher C/P in anoxic sediments compared to oxic. Current phrasing is convoluted while an answer is not (and cannot) be provided regarding the main driver.

P75, L21-27. It seems from this that the authors assume that terrestrial P, i.e. P associated with riverine suspended particles, would not show up at all in the PIP fraction on the filters from the water column samples? If riverine particulate matter sinks through the water column (or do the authors assume all riverine input is lateral?), and consists at least in part of Ca-P and oxide-associated P, why would it not (partially) be part of the measured PIP on the filters? Even though it is a minor P source, the authors might want to elaborate slightly on why riverine P is treated as a completely separate P input.

P76, L12-18. Could sinking of fish debris through anoxic waters not provide a rapidly-sinking source of both apatite and organic matter to the seafloor (biological fish remains, collagen etc. in fish bones)? Then, would fish debris necessarily leave a signature of very low C/P ratios? If these fish debris are indeed missed during water column sampling (or perhaps have a strong seasonal character), the associated OC would also be missed in the budgets, which would have an impact on the argument of the theoretically required low C/P ratio of incoming particles. Perhaps C/P ratios alone are insufficient to dismiss a potential role for fish debris as (additional) PO₄ source? The authors show nice links between PO₄ fluxes and sedimentary bacterial (biological) activity, which can be used to hypothesize on the likely source(s) of PO₄. Perhaps it would be better to focus on the evidence for the contribution of bacteria, to the point where other P sources are not necessarily required to close the budgets.

P78, L13-14. In fact, highest freeze-thaw P release was found in sediments with abundant foraminifera rather than microbial mats (Stat VIII). This deserves a bit more attention here, as it might put the potential role of bacteria in more context?

P80, L1-3. If foraminifera represent such an efficient sink of BW PO₄, it would be good

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if the authors provide their preferred mechanism by which the internally-stored PO₄ makes it into authigenic apatite. Would this be a matter of release after death fueling apatite formation rather than uptake by living forams? What type of redox-dependent PO₄ uptake/release behavior do forams display?

P80, L18-26. This is crucial information regarding potential PO₄ release mechanisms and should be included in the discussion. The conclusions should not contain such crucial new information, but rather should concisely synthesize the findings and interpretations already presented in the results and discussion.

Technical corrections

Starting a sentence with an abbreviation (P) does not have my preference, nor that of style guides. Table 2 is very hard to read and could do with careful attention to layout.

P57, L7. Associated with

P57, L13-14. The phrasing implies that P is associated with CFA through adsorption. Also, it is unclear where these P phases form: water column, sediment, or both (I would say both)?

P58, L13. Roth 2014 citation not in reference list

P58, L21-22. I would end with a highlight/main finding instead of this very general description.

P59, L1. lenses

P61, L16-17. What is meant by the standard and standard solution? A PO₄ stock solution?

P61, L24. Please explain the factor 0.02 in the equation (did I miss it in the text?)

P62, L21. Frozen or refrigerated, sounds a bit random. . .

P62, L23-24. Decalcification before TOC analysis?

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P63, L8. input or flux into

P63, L8-9. How does total digestion give you the P (burial) flux? Maybe leave that until section 3.6 (or refer to the section: For calculation of, see. . .).

P63, L23. "there was only one. . ."

P66, L22. sediment slices

P69, L25. "The calculated diffusive fluxes" (for emphasis)

P71, L8. insight into

P72, L19-21. Here, the trends should be described clearly: there is a consistent increase in POC/POP between the deepest water column sample and the sediment surface. "close to the seabed" is too vague. And "an increase in the first sediment sample" is impossible as it is only one sample.

P73, L19. Hence, the fact that the POC/TPP ratio is close to Redfield is likely due. . .

P75, L3. Suggesting the likely presence of an additional (inorganic) source of dissolved PO₄.

P80, L17. release from

Regards,

Peter Kraal Department of Earth Sciences - Geochemistry Utrecht University
p.kraal@uu.nl

Interactive comment on Biogeosciences Discuss., 12, 16755, 2015.

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