

Interactive
Comment

Interactive comment on “The global marine phosphorus cycle: sensitivity to oceanic circulation” by C. P. Slomp and P. Van Cappellen

G. Filippelli (Referee)

gfilippe@iupui.edu

Received and published: 23 October 2006

Slomp and Van Cappellen have performed an important next step in developing this more sophisticated P cycle model. Phosphorus is an important limiting nutrient on geologic timescales, and as we have learned more about P recycling in sedimentary environments, we have become equally aware that the nature of P cycling in the ocean may have played a critical role in whole-ocean biogeochemical cycling in the past. In summary, this is an excellently constructed, excellently argued, and well-timed discussion submission.

In particular, this four-box model evolved from an earlier more simplistic P-Fe-O₂ feedback model developed by Van Cappellen and Ingall. While an intriguing way to develop hypotheses testable by the geological record of Oceanic Anoxic Events, this

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

earlier model was significantly hampered by oversimplification. In contrast, this newer model includes one of the most important discoveries that we have made over the past decade—namely that continental margins act much differently than do open-ocean systems in terms of P deposition and recycling, as observed by more sophisticated methods for determining early diagenesis of P-bearing phases.

I am intrigued by the potential of this new model, and because of my interest, have several issues that the authors might want to consider in a revision, and in future work.

First, some items that remain unclear to me from the current manuscript.

1). Does the model take into account differential P recycling in the water column as a function of water depth? Does the model take into account differential f-ratio for marginal higher productivity settings versus deep sea settings? I suppose that the first issue is taken into account by P burial rates in proximal+distal versus deep sea settings in this model, but it would be nice to articulate that factor more clearly in the text. For the second factor, again it seems that the model accounts for this by sedimentation, but again it would be nice to have this discussed/thought about. Why? Because this model could also be used to develop hypotheses of the effects of varying shelf area on the marine P cycle. As you vary the actual area of shelf burial (a phenomenon clearly exemplified by the geologic record), you would also vary the locus of deposition of P-bearing phases, and thus potentially the internal P recycling efficiency of the ocean.

2) The results as articulated on the bottom of p. 1601 are intriguing in light of our (Filippelli et al., 2003, Paleogeography, Paleoclimatology, and Paleoecology, 190: 335-348) results for P recycling in latest Miocene sapropel sequences from Spain. We found that precessionally-controlled variation in circulation of the Med. caused a rhythmic variation in P recycling and ecosystem evolution. As lower ventilation rates stratified the Med., sapropels were deposited due to both moderate export of organic matter and lack of benthic oxygen. However, the anoxic conditions resulted in the regeneration of the Fe-bound P from the sapropel sequences, and the build-up of SRP in deeper

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

waters. Upon precession-driven overturn, the SRP flux to the surface water increased substantially, resulting in first marl and ultimately diatomite production, until the excess P was depleted. As the system shifted back to slower ventilation, sapropels were again deposited, with the cycle repeated continuously. We called this the Sediment-Nutrient-Oxygen (SNO) feedback model. It fits in well with these model observations, with the difference being that the 20 kyr precession cycles were too short to have the upwelling SRP flux increase during stagnation (i.e., higher deep SRP concentrations overcoming slower water ventilation rates).

3) Not sure if the claim around line 10 of p. 1606 is accurate—that P accumulation transfer from deep sea to margin sinks may have driven OAE-related phosphorites. Phosphorite formation on margins is complicated—you have to have an environment favorable for phosphogenesis and phosphatic sediment accumulation (high productivity centered over a terrigenous-sediment-starved basin or shelf) followed by an interval of sediment reworking and concentration of disseminated CFA into a phosphorite. This has been shown effectively the similar P accumulation rates in modern high productivity settings like the Peru margin versus rates in ancient phosphorite depositional settings (e.g., Filippelli and Delaney, 1992, *Geology* and others). Phosphorites are the result of dynamic sedimentologic processes, and not just high P accumulation rates (remember, P accumulation rates in even normal marginal sediments are quite high).

Second, an item that the authors might want to consider in future work. I have been working quite a bit on variations in the global phosphorus cycle on glacial timescales. Setting aside efforts to understand terrestrial P cycling, the largest ocean system change that occurs on glacial timescales is the periodic loss and gain of the continental margin sink for P. Sea level variations should have a profound impact on the continental margin sink for the P on glacial timescales. New estimates for a shorter response time of P in the ocean suggest that variations in the P cycle may play a role on marine biological productivity, and thus the carbon cycle, during glacial/interglacial intervals. Although this glacial transfer of nutrients from shelves to the deep sea was postulated

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

over two decades ago, little quantitative data have been produced to support or refute this hypothesis.

We are using some ocean records of P burial to examine the role of sea level change on the marine P cycle. What we find is predictable, but a bit odd, with implications that remain unclear to us. In a nutshell, we (Filippelli, Latimer, Flores, and Murray, DSR II, submitted) see that our P-based records of export production from the Southern and equatorial Pacific Oceans indicate that this redistribution of P may have resulted in higher oceanic dissolved P concentrations and increased oceanic productivity toward the end of glacials and the beginning of subsequent interglacials. We produced a composite P/Ti record (a proxy for excess P burial in marine sediments) using binned averages (10 kyr) of equatorial Pacific and southeast Atlantic sites (ODP Site 1089 in the southeastern Atlantic sector of the Southern Ocean and JGOFS Cores PC 72 and PC 114 in the equatorial Pacific), and compared this to the modeled response of deep sea nutrient budgets to changes in marginal sink using two residence time parameters: 10 kyr and 20 kyr. P/Ti values were normalized to enrichment factor over baseline at each site, and then averaged across all three sites (the normalization to enrichment factor controlled for varying baseline and maximum values at each site). The deep sea nutrient model is simply the sea level record interpretation of the oxygen isotopic record for ODP Site 846; considered to be a relatively robust and continuous whole-ocean record. The quantitative transfer of nutrients from continental margins recorded by the P/Ti record is best matched by the nutrient model with a 20 kyr lag; thus the net impact of redistribution of marginal nutrients to the deep sea matches with the predictions of the P response time in the ocean. A 10-20 kyr lag between P-based export production records and sea level both supports the newer estimates of the marine P response time and indicates that this “nutrient lag” may be a heretofore unrecognized driver of other biological paleoproductivity records. (Note that all of the above is best seen in a figure, but a figure does not seem to be uploadable in the reviewer comments...)

I am left wondering how the model by Slomp and Van Cappellen might be adjusted to

BGD

3, S630–S634, 2006

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

turn on and off the marine sink (maybe not completely, but by some fraction of total) to see how the whole-ocean model compares to the data and simple marginal-sink model presented above. In particular, how would deep ocean P concentration vary when the model is “pulsed” on glacial timescales, instead of simply running the model forward after a single change as done in the current paper?

Gabriel Filippelli Indiana University - Purdue University Indianapolis (IUPUI)

Interactive comment on Biogeosciences Discuss., 3, 1587, 2006.

BGD

3, S630–S634, 2006

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper