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Biogeosciences Discussions

Interactive comment on "Biotic stoichiometric controls on the deep ocean N:P ratio" *by* T. M. Lenton and C. A. Klausmeier

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We thank the referee for their supportive remarks.

Regarding the strength of our conclusions, the main issue seems to be a semantic one regarding the use of the word "controls". Nowhere do we claim that biological properties "alone" control Rd. We completely agree with the referee that inorganic factors also exert control on Rd and indeed we examine the effect of weathering (which is both an inorganic and organic process in today's world). The model systems we examine are at least partly analogous to a thermostat in that they exhibit negative feedback: if one perturbs the models they tend to damp the perturbation, if one changes the forcing (e.g. weathering) they counteract this (although not perfectly). It is really a matter of degree. The Daisyworld model is perhaps more like a thermostat, in being a stronger regulator against changing external forcing. But both Daisyworld and the thermostat

fail at some point. Note also that a thermostat is not a single control mechanism - to be really effective it must include different mechanisms to counteract both warming and cooling.

Regarding Ocean Anoxic Events, the example given of forcing the TT model with a 10-fold increase in dentrificaiton/anammox and observing an almost 2-fold decrease in Rd is an example of strong negative feedback! If this system did not include regulatory negative feedback (increased nitrogen fixation) then one would expect more than a 2-fold response to a 10-fold change in forcing! I agree with the referee's interpretation of what happened in the OAEs and have simulated these in the LW model (see Handoh and Lenton, 2003). The LW model shows qualitatively the same response described for the TT model, except that with certain modifications it can generate OAEs as part of a self-sustaining oscillation generated by increasing weathering forcing past a critical threshold. As I have already examined this in detail I chose not to dwell on it here or indulge in self-citation. However, in revising the paper I will add some discussion of OAEs and the prediction of lower N:P during them. I will also cite the suggested paper by Kuypers et al.

I agree with the referee regarding the steady state solution to the TT model.

I also agree that our analysis does not rule out the possibility of strong abiotic influences on Rd. In no way did I mean to imply that our single negative model result should be taken as ruling this out. I will revise the text accordingly.

Regarding the use of "controls" in the title and elsewhere, in my understanding of the English language this does not imply exclusivity i.e. that the biotic controls are the only controls. One could equally write a paper on the "Abiotic controls on the deep ocean N:P ratio" without it contradicting the present paper. I personally think "effects", "influences" or "impacts" are too weak to describe the striking results, but if the editor insists I will change "controls" to "effects". I will rephrase the penultimate sentence of the abstract. I used "since the ocean became well oxygenated" deliberately to focus on

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times of deep ocean oxygenation, i.e. to exclude the OAEs, but I can see this needs rewording. I will change “set” to “influences” where suggested. The conclusions will be revised, but note that the result the referee cites for an OAE is actually consistent with what I have written: In an extreme OAE scenario they find Rd is “almost halved” (i.e. not more than halved).

In response to the Specific Comments:

p.418, line 11: This will be clarified in the text with reference to the Cullen comment.

p.420: The recent Deutsch et al. Nature (not Science) paper will be cited (but note we wrote our paper long before this appeared)

p.428: We will clarify that N2 is taken up from seawater N2 (although this is still effectively “outside the system” represented by the models).

p.434, line 19: Our use of language was sloppy here and we will rephrase it. For the default parameter settings and fixed weathering flux of P to the ocean deep ocean PO4 is constant in the TT model (but it is clearly a dynamic state variable).

p.439, line 26-28: We will rephrase and shorten this section and clarify the point highlighted, which is that the stoichiometric composition of the oldest phytoplankton types appears somewhat at odds with the composition of the ocean one would predict under the decreased weathering that is thought to have prevailed at the time. The section draws on more than one simulation run - it uses the steady state solutions - but the referee is not the first to take a withering view of our efforts to extend the analysis to deep time. To support our scenario we will make more references to data indicating a reduction in weathering and Org-C burial 1 Gyr ago (see response to referee 5).

We will clarify the caption to figure 5. The assumption $r_{N:P} = r_{N:P,Fix}$ only applies directly to the LW model, it is an implicit assumption in the TT model.

Reference

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Handoh, I. C. and T. M. Lenton (2003). "Periodic mid-Cretaceous Oceanic Anoxic Events linked by oscillations of the phosphorus and oxygen biogeochemical cycles." Global Biogeochemical Cycles 17(4): 1092, doi:10.1029/2003GB002039.

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