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

Anonymous Referee #3

Received and published: 3 April 2007

Biotic stoichiometric controls on the deep ocean N:P ratio. TM Lenton & CA Klausmeier.

This paper is a theoretical analysis of controls on the deep ocean nitrate to phosphate ratio. Variants of two previously-published, independent models are used: (Lenton & Watson 2000 = LW) and (Tyrrell 1999 = TT). Through various model runs and reasoning, it is concluded that the elemental stoichiometry of organisms (biotic factor) controls the deep ocean N:P ratio (hereafter = Rd).

In my view this paper is an interesting attempt at a question of fundamental importance. The paper is well-written and clearly argued. The use of the two models seems generally reasonable, although I doubt that such a strong conclusion is justified.

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My main comment concerns the strength of the conclusions that can be derived. It would be correct to say that the temperature dial (or the agent who turns it) on a thermostat, controls the temperature of a room. In this case there can be external perturbations to the system (cold weather, warm weather outside) but, because of the thermostat control over the heating, the room temperature will be little affected and will continue to track the setting of the dial temperature. It is not clear to me that the analyses presented, sophisticated though they are, demonstrate that biological properties alone control Rd. There are probably multiple controls over Rd, both organic and inorganic, and I don't see any evidence that just one factor controls Rd in the same way that a thermostat controls temperature of a room.

As one example of this, when we ran the TT model to simulate nutrient cycling through an Ocean Anoxic Event, we found that a forced ten-fold increase in rates of denitrification/anammox during the anoxic conditions led to a new equilibrium state in which the intense DIN removal almost halved Rd. Nitrogen-fixation increased in order to keep the nitrogen cycle in overall balance. The spatial separation of the two competing fluxes (denitrification/anammox occurs sub-surface whereas nitrogen-fixation is restricted to surface waters) allowed a large deviation in Rd. Deep water upwells with low Rd, nitrate runs out well before phosphate, the excess surface phosphate stimulates large amounts of nitrogen fixation. Larger imposed changes in rates of deepwater DIN destruction would reduce equilibrium Rd further. The LW model is not able to reproduce this behaviour because surface and deep waters are not modelled separately. While there is no data to confirm or deny most aspects of this scenario, there is some supporting evidence: a dramatic increase in cyanobacteria-specific biomarkers is observed in sediments laid down during OAE1a and OAE2 (Kuypers et al, 2004). Kuypers MMM et al (2004) N2-fixing cyanobacteria supplied nutrient N for Cretaceous oceanic anoxic events, Geology, 32(10): 853-856. The point here is that some unknown cause, most likely physical in nature (e.g. reduced physical circulation leading to ocean stagnation) most likely forced Rd to low values during OAEs.

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Equation 14 of (Tyrrell 1999) gives the steady-state analytical solution (for that model) for Rd. Both inorganic (K=mixing, RP=river input of phosphorus) and organic (for instance, Rorg = elemental stoichiometry of organic matter, NH and PH are half-saturation constants for growth on nitrate and phosphate) factors appear in the formula, arguing again against purely biological control over Rd.

The paper reviewed here considers only one scenario under which abiotic factors might control Rd: that of variations in phosphate weathering in the LW model. In this case it was found that two factors coincidentally almost cancel out, leading to a net small impact. This single negative model result (most likely rather sensitive to the parameterisation of uncertain details of the LW model) does not by itself rule out the possibility of strong abiotic influences on Rd.

That caveat aside, overall the model results are interesting in my opinion, in particular the discussion of how the regulation of Rd responds to shrinkage of the fraction of ocean surface conducive to nitrogen-fixation. However, I do not believe they prove their main conclusion as embodied in their title; the following changes are required: 1. In the title the word 'controls' needs changing to 'influences' or 'effects' or 'impacts', i.e. something which does not imply that biotic stoichiometry is the sole control. 2. Penultimate sentence of the abstract needs deleting/amending since Cretaceous OAEs have not been modelled here. (other extreme events such as K/T boundary, PETM etc have not been considered either; what happened to Rd then?) 3. Top of pg 439: 'sets' should be changed to 'influences' or some such weaker term. 4. The Conclusions section needs to be revised, in particular the second sentence since I don't think the claim that Rd has not varied by more than a factor of two over the last 1 By can be substantiated.

Specific comments:

Pg 418, line 11 (abstract), and elsewhere throughout the MS, e.g. also (pg 434, line 13) and (pg 438 last line): "if competitive dynamics set the N:P threshold for N2-fixation

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... it remains close to the N:P requirement of non-fixers." I am not convinced that this is necessarily true. It is only correct if the ratio NH:PH is approximately equal to the N:P of biomass of non-fixers, which is not a given. See Cullen comment and Tyrrell reply: Nature 402, 372 (25 Nov 1999)

pg 420, para 2: the recent Science paper by Deutsch et al should also be cited.

Pg 428, last para: The N2 is actually taken up from seawater N2 which exchanges with the atm N2.

Pg 434, line 19: deep ocean PO4 is not fixed in the TT model. On the contrary it is one of the dynamic state variables.

pg 439, line 26-28: Since we have little clue as to the actual value of Rd this far back in the past (there is no proxy), this claim cannot be made (that organisms did not adapt to prevailing Rd). In fact section 4.1 as a whole seems very weak to me. It is based on one simulation run with no sensititivty analyses, and assuming that this represents what really happened over 1 By, even though there is data to say one way or the other. I suggest this whole section be deleted.

The caption to figure 5 needs to provide more detail about how the line for the TT model was calculated. How was the assumption rN:P = rN:P,Fix implemented?

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