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Interactive
Comment

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

Anonymous Referee #5

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General comments

The authors present a model study investigating the control on deep ocean N:P ratios by the mechanism originally proposed by Redfield. The authors investigate the potential influence of variability in phytoplankton C:N:P uptake stoichiometry and the possibility that factors other than P availability may be limiting N2-fixation in certain regions of the oceans. The manuscript is well written and thought provoking, concluding that the mechanism proposed by Redfield can still operate in an ocean where N2-fixation may be limited by other factors. They also demonstrate that, in an ocean where the competition between N2-fixers and phytoplankton sets the N:P threshold for diazotrophy, changes in the N:P uptake ratio of the non diazotrophs are the principal factor governing the deep ocean N:P ratio. Overall the manuscript makes an important and novel contribution to understanding of the regulation of the deep ocean N:P ratio. I could find little fault with the logic and results within the constraints of the models investigated and would recommend publication within Biogeosciences. I would, however, agree with the authors own comments that there is clearly scope for further work in the future.

Specific Comments

1) The assumed nature of the competitive dynamics between phytoplankton and diazotrophs is a crucial factor controlling the behaviour of the TT model. Given the results of Klausmeier et al. (2004), it is not necessarily clear to me that N2-fixers would be out competed under P limiting conditions (Page 419, Line 10) in a real ocean where such competition occurs under oligotrophic (i.e. low nutrient) conditions. Klausmeier et al. (2004) suggest that under P limiting conditions (that is, where the bio-available P pool is low, rather than where the supply ratio of N:P to surface waters is 'low', the two not necessarily being equivalent in an ocean where uptake stoichiometry may vary), the optimal N:P ratio may actually be the highest for any growth condition (45:1). Consequently it could actually be speculated that under oligotrophic conditions of low N and P availability, which appears to occur in certain regions (e.g. Wu et al. 2000, Science 259 759-762; Zohary et al. 2005, DSR II 52 3011-3023), diazotrophs, with an essentially unlimited N supply, would actually be able to synthesise more nutrient acquisition proteins and hence would potentially have a competitive advantage? I accept that a greater energetic cost for N2-fixation could potentially offset this hypothetical mechanism. However I also question whether allowing the competitive disadvantage for diaztrophs to be set by a lower maximum growth rate on P (Page 428, Line 10), is actually a valid model for the dynamics which occur in the real ocean as, again, it is likely to be under low nutrient conditions (i.e. where growth rates will inherently not be maximal) that such competition will occur. This would clearly not occur in a situation where excess N remains in the surface following complete depletion of P. However I am not aware of such conditions occurring in the modern ocean? I recognise that investigating

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these possibilities may not be possible within the box models considered and that the authors suggest such future work (Page 440, Line 25), however I invite the authors to comment on how sensitive their conclusions may be to the chosen parameterisation of the competitive dynamics.

2) I am also unsure that restricting the diazotrophs to a decreasing fraction of the ocean (Page 420, Line 20) is necessarily a good model of the effect all the other potential limiting factors may have. The authors conclusions of robust control of deep ocean N:P requires that the biomass of N2-fixers in the permissible areas of the ocean by allowed to increase to compensate for a decrease in this area (i.e. lack of diazotrophy elsewhere) (Page 436, Line 10). This might be a good model if, for example, temperature controls the range of certain diazotrophs, a possibility which the authors incidentally don't discuss (see e.g. Breitbarth et al. Biogeosciences Discussions, 3, 779-801, 2006). However, I am not convinced that limiting the spatial distribution of diazotrophs is necessarily a good model for nutrient limitation by, for example, iron. In the latter case it seems that a better model would limit the total biomass of diazotrophs (yield) which can be maintained in the global ocean? i.e. the authors could ask the question 'what if the iron supply to the ocean is insufficient to maintain diazotrophic biomass at the level which would be achieved if P availability was the only controlling factor?'. A brief investigation of this may be possible with the authors' models?

3) A minor point which is really just further speculation. As recognised by the authors (Page 438, Line 8) the discussion of potential combined effects between changes in weathering and phytoplankton uptake stoichiometry was complicated, although the authors do a reasonable job of explaining this. If I understand correctly, the net effect is an increase in the nutrient inventories of both N and P, with at least the former resulting solely from weathering increases? Again, in a real ocean this may result in more production being carried out under high nutrient regimes? With reference to Klausmeier et al. (2004), this might suggest selection for taxa with lower N:P ratios (8.2) under conditions of exponential growth and potentially provides a mechanism for 'closing the

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loop’ (Section 4.2) and explaining the lower N:P ratio within more recently evolved phyla (Table 3).

4) Again a minor comment, Page 425, Line 4, it isn’t clear to me why the overall feedback on phosphate was negative, presumably this simply reflects choices of initial parameters?

Technical comments

I have few technical comments and thank the authors for producing a well written and clearly well proof read manuscript.

Page 437, Line 2, clarify: ‘i.e. about a factor of 2 either side of deep ocean values in the modern ocean’

Page 441, Line 5, ‘where it is limitedĚ’ somewhat awkward, rephrase if possible.

Fig. 8. It would possibly be useful to either have included in this figure or maybe just in the text a description of how C and P burial are predicted to have changed by the model. Are the predictions consistent with the paleo record?

Interactive comment on Biogeosciences Discuss., 4, 417, 2007.

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