Biogeosciences Discuss., 3, S378–S380, 2006
www.biogeosciences-discuss.net/3/S378/2006/

Biogeosciences Discussions

Interactive comment on "Co-evolution of phytoplankton C:N:P stoichiometry and the deep ocean N:P ratio" *by* T. M. Lenton and C. A. Klausmeier

Anonymous Referee #2

Received and published: 18 August 2006

Review of "Co-evolution of phytoplankton C:N:P stoichiometry and the deep ocean N:P ratio" by Lenton and Klausmeier

General evaluation:

The relationship between N-fixing and non N-fixing phytoplankton N:P ratio and deep ocean N:P ratio is re-examined using simple models of global N and P cycles. The authors find that their model deep ocean N:P ratio is mostly controlled by the N:P threshold for N fixation in the surface ocean, which in turn is set by the N:P uptake ratio of non N-fixing phytoplankton. The authors conclude that (1) deep ocean N:P is not related to the N:P ratio of sinking material, (2) the Redfield C:N:P ratio is not optimal,

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and (3) phytoplankton composition drives deep ocean N:P ratio.

The manuscript is well written but very difficult to read. A list of symbols would have made it much easier to follow the equations. While the model calculations appear more or less logical, albeit highly oversimplified, the conclusions are not justified in any way by this study. Therefore, I do not recommend publication of the manuscript.

Specific points.

1. The authors' claim that the N:P threshold for N fixation sets deep ocean N:P appears to be a consequence of varying density of N fixers compensating for changes in non N-fixer N:P as well as available area for N fixers (p. 1037, last para before Discussion). I would consider this an artifact resulting from the simplicity of the model: if the P cannot go into non N-fixers, it is forced into N-fixers because all other fluxes are fixed. This observation should have told the authors that their model(s) can not be used for this analysis. Although I like the idea that the threshold for N fixation sets deep ocean N:P, it does not follow from the present study.

2. Even if one overlooks above model deficiency, the three main conclusions of the authors still do not appear justified. The authors state that the N:P threshold for N fixation sets deep ocean N:P (p.1033, second para) and that this is in turn set by the N:P ratio of non N-fixers (p. 1034, bottom). The authors go on to conclude that deep ocean N:P is not related to the N:P of sinking matter. Now non N-fixers dominate the sinking flux, not N fixers, so the logical conclusion seems to be that deep ocean is controlled by the N:P ratio of sinking material, which is the opposite of the authors' conclusion.

3. Whether the Redfield C:N:P ratio is optimal depends very much on the optimality criteria (optimal for what?). Indeed, one could easily construct an optimality criterion where the N:P threshold for N fixation is defined by the point of equal growth for N fixers and non N-fixers, such that the threshold represents the optimal utilization of available P. While I agree that there is no strong experimental support for optimality of Redfield

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C:N:P, the authors do not present evidence against the concept. Thus the conclusion that Redfield C:N:P is not optimal is also not justified.

4. The conclusion that phytoplankton composition controls deep ocean N:P and not vice versa (p. 1039, last para) appears to be pure speculation. The only argument presented here for the authors' conclusion is the observed change in phytoplankton composition, but the authors admit they have no answer as to what would drive such a change (p. 1040, top). In the preceding paras the authors rather appear to make the case for the opposite conclusion, which is that rising oxygen concentration reduced denitrification and thereby increased marine N:P ratio. Thus, if phytoplankton evolved to adapt to this rising N:P, we would have a logical chain of arguments.

Interactive comment on Biogeosciences Discuss., 3, 1023, 2006.