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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

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General comments

The referee says "the conclusions are not justified in any way by this study", which is a strong accusation. However, they have misunderstood some of them, which is partly our fault for not presenting them clearly. Hence we have clarified our conclusions as follows: (1) We conclude that deep ocean N:P is *only indirectly* related to the N:P ratio of sinking material (rather than "not related"). (2) The statement that "the Redfield C:N:P ratio is not optimal" is *not* a conclusion from this study, rather it is a conclusion of previous work that we discuss – best stated as follows: that there is no evidence *as yet* that C:N:P=106:16:1 is optimal. (3) We do argue that changes in phytoplankton composition can drive the deep ocean N:P ratio, but we also acknowledge that other factors, especially changes in weathering forcing, can alter the ocean N:P ratio.

We are baffled by the referee's apparently self-contradictory remark that "The manuscript is well written but very difficult to read". Nevertheless, we have worked hard to make it more readable by adding explanation and 5 figures illustrating the key model results. A table summarising the key parameters has been added.

The referee states that "the model calculations appear more or less logical, albeit highly oversimplified". The art of heuristic modelling is to find the appropriate compromise between keeping it simple enough to aid understanding without making it so simple that the essence of the mechanisms in the real system are lost. Clearly we differ with the referee in thinking that the models chosen, although simple, are still useful in aiding understanding of the real system. We chose to study two models with quite different structures, to see whether the models agreed, and we think the fact that they give qualitatively the same results adds strength to our argument. Provoked by the referee, we have now achieved a complete analytical solution of the LW model, which means that our calculations from it are completely (not "more or less") logical.

Let us emphasize that we are putting forward a *hypothesis* – that phytoplankton composition can control the deep ocean N:P ratio – not a statement of fact. Our inferences are open to testing, and that is the scientific way to approach them.

Specific comments

1. The referee's statement that "the idea that the threshold for N fixation sets deep ocean N:P... does not follow from the present study", is wrong unless one completely rejects *both* models we use. In the LW model, the N:P threshold for N-fixation is a parameter, and we show analytically that the deep ocean N:P ratio depends strongly on this. We have now extended the analytical solution and added some figures illustrating this, and showing, furthermore, that the deep ocean N:P is insensitive to changes in

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the Redfield ratios when they are uncoupled from the N:P threshold for N-fixation. In the TT model, the N:P threshold for N-fixation is not directly prescribed, rather it is set by the N:P requirement of non-fixing phytoplankton. We now show in a figure that the deep ocean N:P depends linearly on the N:P requirement of non-fixers. The referee says "if the P cannot go into non-fixers, it is forced into N-fixers because all other fluxes are fixed". We presume that here they must be referring to the TT model (as it's the only one distinguishing the two types of plankton). Their interpretation is incorrect – the P is not *forced* into non-fixers, rather both types of plankton can take up N and P, and the fluxes are not *fixed*, rather both nutrients can also be lost in a mixing flux with the deep ocean that varies with nutrient concentration. Furthermore, the LW model gives the same result with no explicit representation of N-fixers and non-fixers.

2. We conclude that deep ocean N:P is not *directly* related to the N:P of sinking matter. We acknowledge that because the N:P uptake ratio of non-fixers sets the N:P threshold for N-fixation as well as determining the N:P ratio of the sinking flux, then of course there is an *indirect* connection between the N:P ratio of the sinking flux and that of the deep ocean. But our point is to show what is the key control – namely the N:P threshold for N-fixation – and clear up the old misunderstanding that remineralisation can alter the ratio of N and P in the deep ocean. Furthermore, if there were some decoupling in the system (e.g. other factors limiting N-fixation) then it would be the N:P threshold that triggers N-fixation that sets the deep ocean N:P. We trust that other readers find this a useful insight, and we believe that it is a strength of the simple models that this result can be demonstrated clearly and analytically.

3. It is not *our* conclusion that the classical Redfield ratios C:N:P=106:16:1 are not optimal. This point is incidental to our argument, it just provides useful background context for the article. Indeed the referee agrees with us that there is (as yet) no experimental support for the optimality of the original Redfield ratios. Here they are raising a complete red herring.

4. We now conclude that phytoplankton composition *can* control deep ocean N:P.

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Interactive Comment

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This is described as “pure speculation”, yet we have two structurally quite different models with analytical solutions in which it is clearly the case. Implicitly or explicitly, the referee is rejecting both models but offering no alternative model. Whether changes in phytoplankton composition *have* altered ocean composition also depends on whether one accepts that there have been changes in phytoplankton composition. Given the data of Quigg et al. we think this is a reasonable *hypothesis*. The referee is right that we do not have an answer as to what would drive such a change in phytoplankton composition. As we now try to make crystal clear, there is no evidence to support the referee’s suggestion that “phytoplankton evolved to adapt to...rising N:P” (due to increasing weathering), because the older phytoplankton groups have *higher* rather than *lower* N:P. Thus the referee certainly doesn’t have “a logical chain of arguments” to explain the available evidence. We have now altered our discussion to emphasize that both changes in weathering and changes in phytoplankton composition can alter deep ocean N:P, but they do so primarily via different nutrients. We also acknowledge that (although we have not modeled it), phytoplankton probably have some capacity to adapt to prevailing conditions. Including that process in more detailed models is a topic for future work.

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