

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

T. M. Lenton and C. A. Klausmeier

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We thank the referee for their supportive comments and agree that there is indeed scope for further work in the future. We propose to add a short sub-section to the end of the Discussion addressing this.

In response to their specific comments:

1. We agree that it is not necessarily clear that N₂-fixers would be out competed under P limiting conditions and find the referee’s scenario where they are able to synthesise more nutrient acquisition proteins reasonable. However, following the scenario through, the N₂-fixers would then produce more available N and thus benefit the non-fixers - making the situation somewhat self-regulating. There are a few regions of the world ocean where excess N remains in the surface following complete depletion of P

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and perhaps these are regions where something like the scenario has occurred. However, what is clear is that N₂-fixers do not dominate the global phytoplankton, so the energetic costs of N₂-fixation must act to lower their growth rate somehow. The TT model actually gives them lower maximum growth rate on both N and P to account for this. In revising the paper we will comment on how sensitive our conclusions are to the parameterisation of competitive dynamics.

2. Our approach of restricting the diazotrophs to a decreasing fraction of the surface ocean was just intended to be the simplest possible implementation of their being limited by factors that are not explicitly represented in the model (e.g. light, Fe, temperature). We agree that temperature is a potential control and will add reference to the paper suggested. Our simple approach may be reasonable for both temperature and light controls which vary inversely with latitude. We agree that it is not necessarily a good model for e.g. iron limitation, and that it would be interesting to limit the total biomass of diazotrophs. This could be implemented in the code for the TT model in the form of an ‘if, then’ cap on total diazotroph biomass but that would destroy the analytic tractability of the model and I would expect it to disrupt the dynamics of the model, which often have the form of pulses of N₂-fixation followed by quiescence. A better way to implement it would be to include an Fe cycle and Fe uptake control of productivity, but that would make a rather different and more complex model. We will note the scope for these approaches in revising the paper.

3. The referee is correct that the net result of an increase in weathering and a decrease in the average N:P of the phytoplankton is an increase in both N and P inventories, with the increase in N due solely to weathering if the phytoplankton C:N ratio is constant. This may indeed result in more production being carried out under higher nutrient regimes, and therefore select for lower N:P phytoplankton. We thank the referee for this neat suggestion for why there may have been a general decline in phytoplankton N:P which could ‘close the loop’. We will note it in revising the paper with credit to an anonymous referee (unless they would like to reveal their identity to

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t.lenton@uea.ac.uk).

4. The overall feedback on phosphate is negative because feedbacks on Org-P and Ca-P burial are both negative, they comprise circa 83% of the present P burial flux, and the functional dependence of their burial on productivity is quadratic (i.e. non-linear). In contrast, the positive feedback on Fe-P burial comprises circa 17% of P burial and has a simple inverse dependence on productivity. If one includes a positive feedback due to an increasing (C/P)organic burial ratio under anoxic conditions then the balance shifts somewhat toward positive feedback, but positive feedback only comes to dominate if one also makes anoxia more sensitive to changes in productivity (see Handoh and Lenton, 2003). We will clarify this in revising the paper.

In response to the Technical comments:

We will clarify and rephrase the suggested parts.

We will add some text describing how C and P burial are predicted to have changed under an increase in weathering. Basically both total P and Org-C burial are predicted to have increased in order to maintain steady state for the P and O₂ cycles respectively. An estimate of P burial only extends as far back as the oldest ocean floor sediments that haven't been subducted; circa 160 Myr (see Föllmi, 1995). Ideally we want a much longer record to test our prediction. The delta-13C records of carbonates and organic carbon extend much further back and can be used (with various ancillary assumptions) to infer the fraction of total carbon burial in organic form. A significant increase in this fraction is inferred in the Neoproterozoic, which is also a time of an inferred rise in atmospheric O₂, both of which are consistent with the LW model (see Lenton and Watson, 2004). During the Phanerozoic, there is a further inferred increase in Org-C burial and atmospheric O₂ associated with a 'hump' in delta-13C and a predominance of rock types that are rich in organic carbon, centered in the Carboniferous and associated with rooted vascular plants colonizing the land surface. This is also consistent with the LW model. In revising the paper we will note the scope for further work testing

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predictions of our hypothesis against the paleo record, probably best conducted using the COPSE model, which embeds the LW model (see Bergman et al., 2004).

References

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