

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

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We recognise the referees concerns about the presentation, but we are concerned that they refer to an earlier version of the paper we published in BGD. In particular, their point 3 cites equation numbers corresponding to the earlier version of the paper. Since that version we have greatly expanded the presentation and explanation of the models.

In response to the specific comments:

1. Table 1 includes and defines all symbols. All equations are presented in the text with some accompanying explanation. For more details the reader is already referred to the original papers of Tyrrell (1999) and Lenton and Watson (2000). We do not see how repeating all equations in a Table will add value or intelligibility to the paper.

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2. The kind of box models we use deal only with globally averaged quantities, and essentially have their initial steady state calibrated to match the current global average data. Consequently there is a good correspondence with the data, of the form of comparing one number with another (e.g. for deep ocean nitrate concentration). Such comparisons are already mentioned in the text presenting the models and will be expanded on.

3. Here there is some confusion as 11-13 are equations of the LW model not the TT model (in the earlier version of the paper 11-13 were the appropriate equations for the TT model). The input and output to each population in the TT model are currently given by equations 24-26. This is visualised in Figure 2 along with the whole of the TT model. Hence we are not clear exactly what the referee is asking for. The populations in the TT model compete indirectly for resources and it is not obvious how to visualise this. All our results presented are for steady states rather than dynamical transient solutions, and we'd like to keep it like that for clarity. We do mention some oscillations that arise when N₂-fixers are restricted to a small fraction of the surface ocean in the TT model. However if we start adding transient model solutions we will rapidly have a much larger paper.

4. Without references from the referee we are not clear what “arguments” are being referred to. We are aware of the work of Falkowski (1997), Anbar and Knoll (2002) and Fennel et al. (2005) regarding the transition from the Archean to the Proterozoic and the lower oxygen world of the early Proterozoic. However, this is not directly relevant to the present paper as we focus on the well oxygenated world of the late Proterozoic and Phanerozoic. We are also aware of the Science paper of Pahlow and Riebesell (2000) and the subsequent Technical Comments in Science. We will add some discussion of these other studies. However, as far as we are aware, no one has previously shown mechanistically how systematic changes in the N:P ratio of the phytoplankton could alter the N:P ratio of the deep ocean. We welcome any references that show otherwise.

5. This is an interesting point. If we assume inefficiency in the biological pump in

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HNLC areas of the ocean, in the form of a mismatch between N and P supplied and that assimilated, it does not greatly affect the model results as long as these areas do not dominate global production. If iron and/or light are the alternative limiting factors (e.g. S. Ocean HNLC) then the region is unlikely to support N-fixers anyway, and the model experiment is like the ones we show where N-fixers are restricted to a fraction of the surface ocean. We will add some discussion of this issue. Note that mismatches between supply and assimilation on an annual average are not critical for our results which consider much longer timescales. The mismatch would have to be present over thousands of years.

6. Our “evidence” for a decrease in N:P of the phytoplankton over the last 1 Gyr is the data of Quigg et al. which shows the older ‘green’ types have higher N:P than the younger ‘red’ types. There is no direct proxy (as yet) of N:P in such ancient sediments to further test this. There is evidence that weathering has become more efficient towards the present, for example based on fossil soil horizons and their geochemistry, and this will be cited in revising the paper.

7. We have revised this paper a number of times before, adding more detail about the models at each stage and before submitting this time we showed the paper to a number of non-modellers who were happy with the level of model description and the referral to the original papers of Lenton and Watson (2000), and Tyrrell (1999) for more detail.

Regarding the originality of the findings, as already stated, we are unaware of anyone having systematically made the link between changes in phytoplankton N:P driving changes in deep ocean N:P. Our other aim was to use the models to clarify the links between non-fixer N:P, the N:P threshold for N₂-fixation, and deep ocean N:P. We are unaware of previous work that has done this in such an analytical way. The other 3 referees note the originality and value of the work. Of course we welcome pointers to any literature we may have missed.

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