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Biogeosciences Discussions

Interactive comment on "Influence of consumer-driven nutrient recycling on primary production and the distribution of N and P in the ocean" *by* A. Nugraha et al.

Anonymous Referee #2

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I enjoyed reading this article and, with some clarification of some issues, I think it will make a good contribution to the scientific literature. The extension of Tyrrell's box modelling approach to include the stoichiometry of grazers is interesting and topical given current interest in the role of multi-nutrients in structuring ecosystems and the impacts on associated biogeochemistry. I very much liked the main conclusion: Levels of global primary production were higher particularly when herbivores had higher N:P ratios than phytoplankton. This higher primary production was triggered by a low N:P resupply ratio from herbivores, which in turn favoured the P-limited N2-fixation.

I did however find reading the ms hard going at times. My main criticism, which I think

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the authors must address, is that it the stoichiometric model of herbivores is poorly described in terms of text accompanying the equations. It is based on a relatively old model of Sterner's. The authors need to do far more in terms of describing, in plain text accessible to modellers and non-modellers alike, the basic assumptions and parameterization of this model, and say why they are justified for their application of it. Let me give a few examples: 1) Page 117, line 19: What is an "accumulation efficiency"? How do accumulation efficiencies affect the balance of N and P cycling, and how are they parameterized (given that they appear to be calculated, not fixed)? 2) State the conditions under which N and P will be limiting. Looking at Fig 4 (c) and (d), it appears that the threshold elemental ratio is at an algal N:P ratio of about 24. So how has this come about (which parameters is it determined from)? How sensitive are the overall results and conclusions to this TER? Likewise, GGEs wore 52% and 33% for N and P (page 131, line 11). Make it far easier for the reader to understand how this has come about. What is actually predicted to be limiting the herbivores, and what were the consequences for nutrient excretion. 3) Some statements confused me. E.g. (p. 122, line 14): "In the model, a total of 56.5% of gross intake was released as either NH+4 15 or DON." But surely the release should be variable, according to the N:P ratios of predator and prey? Indeed, on p. 131 (line 10) there is: "In the model, herbivores' excretion depended on the N and P assimilation and accumulation efficiencies. I've only given a few examples. But in general I found the model impenetrable, which was a shame. The model description does, I suggest, require a major overhaul.

The only other major criticism I have relates to iron. Tyrrell himself received criticism for not including iron in his model, given that it likely mediates the competition between nitrogen fixers and other phytoplankton. Given that iron cycling in the ocean is a topical issue, I am surprised that I could find no mention of it in this ms. The authors do suggest, for example, that N2-fixers are P-limited. Interesting, but surely this should be set in context of current views of this group being limited by Fe.?

Interactive comment on Biogeosciences Discuss., 7, 111, 2010.