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Interactive comment on “C, N and P stoichiometric mismatch between resources and consumers influence the dynamics of a marine microbial food web model and its response to atmospheric N and P inputs” by P. Pondaven et al.

Anonymous Referee #1

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The manuscript describes the application of a simple stoichiometric model to examine the balance between autotrophy and heterotrophy in oligotrophic systems and how it is related to consumer-driven nutrient recycling (CNR). I found the work thoroughly unconvincing. The model is not well conceived and the parameterisation looks woeful, I cannot understand the need to take a statistical approach with 1000 simulations, there is no comparison with data and the inferences and conclusions drawn from the results are weak, particularly with respect to the debate on net heterotrophy. As such, I am unable to recommend publication. I am not prepared to write a point by point review of

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the whole manuscript, but will restrict myself to my main concerns.

First, the stoichiometric theory that the authors apply is weak and does not appropriately build on the existing literature. My job of reviewing was made doubly hard because the equations were not numbered. At the bottom of p. 2940, the authors present the very simple balancing equation: $\theta_{C:P_f} a_C / a_P = \theta_{C:P_k}$ where $\theta_{C:P_f}$ and $\theta_{C:P_k}$ are the C:P ratios in food and consumer, and a_C and a_P are the gross growth efficiencies (GGEs) for C and P. There is nothing new about this equation and yet there is no referencing of the previous literature. The authors then present the application of this equation in a multielement framework (C,N,P). In fact, the application of multiple elements has previously (and more elegantly) been presented elsewhere (e.g., Anderson and Pond, 2000: L&O 45, 1167) but, again, there is no citation. But there is worse in the way this equation is applied. A major aspect of using this approach is that the limiting element will always be used with maximum GGE, while the use of other elements will be below their maxima. But the maxima will be different for C, N and P. One might expect high maximum GGE for N and P because of nutrient sparing. Carbon cannot be spared and so would be expected to have a lower maximum GGE. But the text shows no appreciation of this important aspect and the maximum GGE appears to be set to 0.75 for all three elements (line 10, p. 2942). The authors tamely cite Sterner (1990) and Straile (1997) but this will not do. Throughout, there is a purely mathematical treatment with little reference to assumptions in the underlying physiology.

The example above is just one of many I could give for really poor justification for both the modelling approach and associated parameterisation. The model is poorly described and trying to understand it by looking at Table 1 is a nightmare. There are other inconsistencies. For example, GGE suddenly becomes parameter omega on p. 2943. The equations are opaque. And looking at Table 2, none of the parameter values are justified. They are all ad hoc. Another parameter that caught my eye, for example, is the last parameter in Table 2, λ_e , the fraction of postabsorptive

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excretion released as dissolved inorganic nutrients of CO₂. It is given a fixed value of 0.6 without any apparent justification. I would expect this parameter to differ for C, N and P and probably also with food quantity. But there is no text to give the reader any appreciation of the issues involved. Surely this parameter is important for CNR? I could go on and on with the other parameters also. The whole model description and parameterisation is opaque and unconvincing.

I find the whole approach of using 1000 simulations to get at weak and strong stoichiometric interactions unconvincing. This is after all a deterministic model. So why not just pick a few well parameterised and characteristic simulations to do the job? It would be so much easier (and more interesting) for the reader and proper sensitivity analysis could then be conducted on key parameters.

There is no comparison with data despite mention of the DUNE experiments in both Introduction and Discussion. At the very least, surely the DUNE experiments could have been used to help parameterise the model, and preferably also to provide data to compare with. As it is, the work represents a purely theoretical study with little empirical basis. Even if the modelling work could be brought up to scratch (a mighty task), I suggest it should be published in a modelling journal (e.g. Ecological Modelling) and not Biogeosciences.

I find the inferences and conclusions drawn from the modelling work hard to understand and weak. The authors define net heterotrophy in terms of GPP vs CR. This GPP should include all photosynthetic input, including exudation of DOC, but I am not convinced this is the case in the analysis. Without including the DOC then of course one predicts net heterotrophy, but it is not real. At steady state, surely the model can be neither net heterotrophic or autotrophic? Of course, with the dust additions, it goes net heterotrophic and will not balance again until steady state is reached. But this is all rather obvious and I do not see what the modelling is achieving in terms of new insights. I was looking for something exciting, e.g. in terms of CNR. But I did not get it. The weakness of the conclusions is typical on p. 2952: "Defining an index of C: P

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and C:N stoichiometric mismatch, the model suggested that C:N: P imbalance in food compare with consumer's requirement contributed to drive the balance between net autotrophy and net heterotrophy." So what? I want details of underlying mechanisms if something is to be learned.

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