Review for GMD: Pahlow et al. "Optimality-Based Non-Redfield Plankton-Ecosystem Model (OPEM v1.0) in the UVic-ESCM 2.9. Part I: Implementation and Model Behaviour"

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Summary:

Pahlow et al. incorporate previously published mechanistic parameterizations for plankton into a global ecosystem model. Specifically, they incorporate the variable elemental stoichiometry model for phytoplankton from Pahlow et al 2013, and an optimal current feeding model for zooplankton from Prowe and Pahlow 2010. Two different temperature functions are also explored for N fixation. The global configuration improves much of UVic's simulation of observations.

General comments:

1. Temperature function

Since this is a model development journal, the temperature implementation could be clarified in section 2.1 and in the abstract. In Fig. 2, the y axis label and the first sentence of the caption indicates that the plot shows the temperature function only for N2 fixation, but the rest of the caption and the in-text discussion (lines 135-139) suggests a different configuration. My take-away understanding is:

a. Original UVic: All diazotrophic rates (uptake, growth, and N2 fixation) are multiplied by a factor of 0 at 15C and a factor of 2 at 30C.

b. OPEM: same

c. OPEM-H: The Eppley curve is used for uptake and growth for diazotrophs as well as ordinary phytoplankton. The Houlton curve is used for N2 fixation alone.

Is this correct? If so, what is the temperature function for ordinary phytoplankton in UVic and OPEM? Did that also change between OPEM and OPEM-H, so that ordinary phytoplankton metabolic rates are also higher at lower temperatures and lower at higher temperatures?

2. Denitrification and cost function

Could the global water column denitrification rates for the three models be summarized somewhere? They are referred to multiple times. A realistic denitrification rate effectively served as a second cost function for assessing the simulations, in addition to the cost function itself (l. 190). Since denitrification rates are stated to be lower in OPEM and OPEM-H (l. 231), this implies that the cost function was also different. How did denitrification weigh against the actual cost function? With effectively two cost functions to minimize in this way, how does this result in an objective determination of one parameter set? Since the same optimized parameter set emerged for both OPEM and OPEM-H, does that mean that they have the same denitrification

rate? Did the geography of denitrification change (the OMZs themselves or the anoxic portions of them), or was it just lower everywhere? It would be helpful for the interpretation of the results to have a bit more information about denitrification.

3. Discussion of the new grazing model?

The results and discussion are nearly exclusively focused on the variable stoichiometry of the phytoplankton and its effects. Yet the model also includes a new grazing parameterization: the optimal current feeding model. As a suggestion (within the authors' discretion), it would be more comprehensive to at least include a few sentences evaluating the impacts of this portion of the implementation on the simulations. Perhaps the discussion of the coexistence of ordinary phytoplankton with the non-N2 fixing diazotrophs (l. 335-338) or the presentation of the more evenly distributed phytoplankton biomass would be good segues for this.

Specific comments:

l. 94-95: To what degree is the tracer not conserved as a result of these schemes?

l. 76-99: These paragraphs include quite technical detail about how to deal with negative concentrations in the model. For readability purposes, it would be more engaging to have the model descriptions first (starting with section 2.1), and move these two paragraphs either to after 2.3, with their own section heading, or (better yet) even moving them into Appendix A. In either case, it would also be helpful to address why it is that negative concentrations are "one of the main problems for implementing variable stoichiometry" (l. 76).

l. 136: "the same temperature dependence (Eppley, 1972)" -- does this mean the same as in OPEM? Or just that it is the same for both ordinary phytoplankton and diazotrophic uptake and growth?

l. 157-158: "mostly in dissolved form (as inorganic nutrients)". This is consistent with Chi in Table 1 described as "dissolved N, P loss". However, Chi then shows up in Eqn. 6 as a source for sinking detritus. Could the fate of Chi be clarified? It would also be helpful to describe Chi in words after it is introduced in Eqn. 6.

l. 238: "reproducing the deep DIN:DIP distribution appears to require ... a suitable parameter set". Could you qualify what is suitable? Any information about what parameter space works better than another?

l. 231: Since C export is the same, N export must be lower. Could the lower O2 consumption from lower rates of nitrification partially explain the lower denitrification?

l. 235-240: The fact that only OPEM-H is able to capture the Pac vs. Atl basin differences in N* seems key, if this is separate from the gradient within the Atl. Could this be emphasized and explained?

l. 248 and on: If NPP is ~2x as high, and the export is the same, why is the export efficiency so much lower?

l. 256: Perhaps somehow the much higher NPP is simply evidence that the optimized growth is indeed optimizing the pp growth in the model, and so the well-matched UVic estimate might be close to the observations for the wrong reason?

l. 290: "a wider geographical range" in OPEM-H. Does the fact that the temperature function is lower at higher temperatures have any impact?

l. 317-318: Does the higher kFe for diazotrophy impact its resulting biogeography?

l. 335-338: Could top-down control also play a role in supporting the non-N2 fixing "diazotrophs", suppressing the ordinary phytoplankton?

l. 350 and Table 3 caption: Do you mean the average of the log-transformed values? Then write as the "log-average" or the geometric mean (not "log-normally averaged"). Also, by particulate, do you mean both the biomass and the sinking detritus?

l. 364-370: Is it appropriate to have the matching of the model with data as a goal when preferential remineralization is not included? (I.e. Letscher and Moore 2015 as you've already cited). Perhaps discussion could be tweaked to acknowledge that only part of the story is included. Also, Talmy et al 2016 GBC showed that zooplankon respiring the extra C, rather than returning it in organic form, might be more mechanistic and would have the effect of dampening the non-living surface ratios.

Fig. 15: The two captions should be one caption that is the same for both plots.