Review for “Optimality-Based Non-Redfield Plankton-Ecosystem Model (OPEM v1.0) in the UVic-ESCM 2.9. Part I: Implementation and Model Behaviour” March 2020

Summary

The authors have embedded an ecosystem model which resolve lower trophic dynamics within the University of Victoria Earth System model. They report on the methodology which was used, with explicit focus on pertinent aspects of their chosen integration scheme, and a summary of ecological / physiological parameterizations of phytoplankton and zooplankton dynamics, which are largely based on prior work by these authors and others. They report first-order properties of the calibrated model solutions, such as NPP, NCP, C:N:P, etc. They discuss and interpret their model solutions, highlighting areas for improvement which are opportunities for learning about the global ecosystem.

Main points

Overall, the manuscript is extremely carefully prepared and quite straightforward to interpret. I haven’t downloaded and used the code but they have provided access to online repositories and instructions for reproduction of the output. The model assumptions are firmly rooted in prior works. I anticipate this will be a useful tool for future investigations of marine ecosystem properties and the coupling with climate. I have a few queries regarding the solutions. Since this journal is focused on model development rather than specific modeling outcomes, I don’t necessarily regard possible shortcomings as a barrier to publication. It might be nice, however, for the authors to respond to these major issues, clarifying whether they intend to investigate these issues here or in subsequent publications:

1. Phytoplankton biomass in the gyres seems a little high. This is most evident in Fig 9 comparing MODIS inferred Chl with model output. There are a few conspicuous patches especially in the South Pacific, which are clearly absent in the MODIS data. The patches in the south pacific look to me like they might be numerical artefacts. Can the authors comment on this? It sort of gets brushed over. There is more focus on the comparison of model vs. CbPM NPP (Fig 10). I’m not an expert on the CbPM but my understanding is that there is relatively low uncertainty on chl relative to carbon when inferred from satellites. Given the rather high estimates of global NPP in this study, it might be nice to be extremely clear about situations when the model over-estimates satellite inferred Chl, before moving on to other comparisons.
2. I may have missed this, but I don’t quite understand what aspect of the non-N fixing diazotrophs sets them apart from regular algae, from a trait perspective? Is it their high N:P ratio? Given that the high N:P of these groups appears to introduce artefacts in N*, is it really necessary to include this, instead of a functional representing, say, haptophytes? Apologies if I missed something very obvious here.
3. Regarding the rather high C:N of detritus. I usually try to avoid doing this, but I wrote a paper on exactly this topic back in 2016 (Talmy *et al.*, 2016). It looks like the mismatch in phyto and zoo C:N is largely being excreted directly into the detrital pool. Our conclusion with a model of microzooplankton respiration, was that much

of the C may in fact be respired. This is a simple explanation for the overestimation of carbon in detrital pools.

Specific comments

Line 135 and Fig 2: I got a bit confused here. The figure shows three temp responses but there are only two models. I get that the defining characteristic of OPEM-H is the contrasting temp response for N2 fixation. I just wonder if the fig. can be changed to more clearly group OPEM-H temp responses, e.g. with dashed lines, and by grouping them with an OPEM-H flag in the legend?

Line 146: Take 'B18' out of parentheses?

Line 165: surely grazing is a form of mortality. Can you say 'background' mortality, or 'closure', or similar..? Also, might be nice to add a word or two on the quadratic closure

Line 178: "C:N = 6.625 molC molN−1, as 1.45−2/6.625 = 1.15 mol O2 mol C−1." Apologies but I'm missing the reasoning for the 1.45-2/6.625. Can you add a word or two to explain?

Line 179: "Increases with depth" Why does sinking speed of detritus increase with depth? I understand this was reported elsewhere. Just might help to add a sentence or two about what underlies this physically / biologically.

Line 187-188: "400 parameter sets" I understand that the calibration was reported in the companion paper. Might help with the flow to give a little explanation. At least that the Latin Hypercube scheme was used. I had to look this up, but many readers will not.

Line 196-197: "excess nitrate with respect to phosphate, termed N*" I thought the point of N* was to subtract out Redfield N:P, so that surpluses and deficits in N are evident. This wording feels a little off, perhaps rephrase?

Line 238-239: "require the combination of decoupled C, N, and P with a suitable parameter set" what is it about certain parameter sets that decouples C, N, and P? – this seems important

Line 248: why is your NPP so high? Apologies if I missed this. But perhaps it could be clarified more directly?

Fig 13 and accompanying argument, specifically line 305 "inverse relation between inorganic N:P and P". First, I find it really hard to grasp what is intended with Fig 13. There are a lot of data in the different panels and I find it hard to focus in on what's intended. Moreover, can the inverse relationship between N:P and P also be explained by preferential P remineralization? Given that the conclusion of this paragraph appears to be that "more investigation is warranted", and the main findings are somewhat obscure and difficult to grasp, I suggest either removing this figure or editing it / the accompanying text to make it clearer.

Line 318: “a higher values” check grammar

Line 320: “Phytoplankton is much more evenly distributed” in line with my comments above, the high phyto biomass in the gyres feels inconsistent with satellite estimates, and also frankly with our basic understanding of plankton biogeography.

Line 334: “non-N2 fixing species adapted to low light and long periods of darkness” As per my main points above, surely this could apply to many phytos, why do they need to be diazotrophs?

Line 375-378: “The relatively low assimilation efficiencies…”. I can’t make sense of this sentence. Consider clarifying.