Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-323-RC3, 2020

Geoscientific Model Development Discussions

Interactive comment on "Optimality-Based Non-Redfield Plankton-Ecosystem Model (OPEMv1.0) in the UVic-ESCM 2.9. Part I: Implementation and ModelBehaviour" *by* Markus Pahlow et al.

Anonymous Referee #3

Received and published: 16 April 2020

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"

In this manuscript, the authors take as a reference an existing global biogechemical model, which they improve in several ways (e.g. better parametrizations, different phytoplankton temperature response curves...). One of the main focus is to move from fixed to flexible phytoplankton stoichiometry (C:N:P), as well as the implementation of optimal phytoplankton nutrient uptake and zooplankton grazing. The manuscript is de-

voted to comparing versions of the global model, with special emphasis on reproducing key patterns for N and P, including patterns of nitrogen fixation.

The manuscript is overall well written, and most of the different components are understandable. Although I have some experience with global models, most of my expertise focuses on more localized microbial models and, in spite of this, I think I could understand most of the model explanation and results. Still, I think my comments below can help improve the accessibility of the manuscript to a broader modeler audience.

1) In general, I got the feeling that the authors tried so many different versions of the UVic model (e.g. several parametrizations) that it is difficult to trace back why the improved OPEM models show the behavior they show. Also, the authors emphasize the move from fixed to flexible stoichiometry as the main selling point of their improvement, but they do alter and discuss other many aspects and for the same reasons it is difficult to understand what part of the observed behavior results from that improvement versus just a more suitable parametrization. The authors somehow touch on this same issue by the end of the manuscript, but I do not think they suggest any way to fix it. In models with so many moving pieces, I would have suggested choosing one single "best" UVic version/parametrization, and change one aspect at a time. I understand that given the rigidity of the model there won't be a single good parametrization that works globally, but then it may make sense to focus on the comparison of specific regions using the best version for each region. That would mean move from global to semi-regional maps, but at least it would be easier to identify which details of the OPEM models make a difference with the UVic model.

2) I found Fig1, which is supposed to schematically show how the OPEM model works, quite uninformative. It describes the links between the different components of an improved NPZD model, but I don't see any detail that makes it specific of the optimality model (other than the caption stating that some of those components are described with optimality functions). I think some additional panels describing how optimality works for those components would go a long way in convincing the reader that this is

a significantly different version of the model of reference.

Actually, I think the authors could improve the justification as to why the optimality assumption is needed or is expected to describe the system more closely. Would other forms of variability play the same role? Would a non-optimal description of plasticity for uptake and grazing play the same role? Given the expected variability for planktonic organisms, why would them all follow an optimal strategy? And why would nutrient uptake follow an optimal strategy and not, e.g. temperature acclimation?

And regarding the optimality description, why does the N-related maximum uptake go to zero when Q->Q0? Isn't that behavior exactly opposite to what has been reported experimentally (see e.g. S.Dyhrman's work or, from a theoretical point of view, F. Morel's work)? Why is there no flexible P-related maximum uptake (even though it's been shown experimentally that regulation of P transporters occurs)? And why is r_DIC multiplicative? All these are modeling choices and therefore need to be well justified and put in context.

Finally, can the authors explain whether this (instantaneous) optimal acclimation entails any type of metabolic cost in the model?

3) Although I understand this is a quite standard way to present the information, I find Figs4-12 not very helpful when it comes to assessing which model does a better job where. Unless there is a very obvious divergence with observations, it's difficult to see clearly which model works better at each region/feature. The authors mentioned a cost function to compare models (which I guess acts as indicators such as the AIC, and hopefully also takes into account the number of parameters). I think that maps that show instead the difference in that or another way to quantify closeness to the specific pattern they want to show would help hugely the discussion, because it'd be much easier to spot which model diverges less from observations and where.

4) I would also suggest for the authors to state more clearly/emphasize what assumptions/parametrizations are based on published experimental observations, which ones

in existing model results that have been validated, and which ones are just the result of observing that including them brings the model closer to general observations.

Also, I think it'd be also reassuring if the authors commented on whether some of the "moving pieces" introduced here (e.g. Vmax for nitrogen, gmax) remain within realistic ranges. I can envision several compensating factors leading to e.g. realistic overall uptake through highly unrealistic Vmax values. For example, gmax in the OPEM model is 4x the one reached with UVic, and the authors don't seem bothered about it because the overall total grazing remains under acceptable levels, but it would be reassuring if the authors commented on whether such high gmax values are still within reasonable levels themselves.

5) Did the authors track how close each version of the model is to observations at particular times of the year (e.g. around blooms, winter...)? That exercise may help narrow down when and why one version works better than another for a particular feature.

6) I strongly recommend that the authors structure the subsections by key findings, i.e. introduce sub-subsections with titles that summarize the main finding. This would help/guide the reader to discern better what the main messages from each studied feature is. Although the individual subsections read well, the fact that a model does well in a particular region for a particular feature but not another, etc makes the flow a bit lost/erratic, and thus it is difficult to know what the take-home message for each section.

7) Finally, given how large the potential for grazing gets, I think it'd be very interesting for the authors to comment on how other sources of top-down regulation that are not present (e.g. viruses, or even fish targeting grazers) would affect their results. After all, one of the main goals of the manuscript is to identify the deficiencies of this and similar models, and the lack of a realistic representation for such a key player in the microbial loop is one of the main shortcomings of current global models.

L50: Plasticity has a very specific meaning for these organisms, and is not necessarily the same as variability (the latter can come from other sources and not only plasticity). L86-99: Please be explicit as to whether all these improvements are also implemented in the UVic reference version. L119: Has FTC been defined before in the text? L122: Just for PON and POP, right? Page 6: I think "balance equation" is easier to understand (and more standard) than "sources-minus-sinks terms". 130-133: Why is leakage not a nutrient-specific parameter/process? L137: Replace "phy" and "dia" for their complete word. L152: A figure similar to Fig2 explaining how the optimal uptake/grazing terms differ from the ones used for the UVic model would be very illustrative. Table 2 (page 9): Does the lack of values for the original model mean that the OPEM versions are incorporating 13 new parameters to describe zooplankton? If so, it should be noted in the main text (the same way it is discussed the fact that the phytoplankton improved component does not increase the number of parameters). L197: I think N* should have been defined like this much earlier (the definition in the abstract is not as clear as this one). Eq.8: It'd be good to translate each term into its ecological meaning as it's done with other equations, so the reader understands how NPP is exactly defined here.

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