Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2020-396

Anonymous Referee #1

Referee comment on "FABM-NflexPD 1.0: assessing an instantaneous acclimation approach for modeling phytoplankton growth" by Onur Kerimoglu et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2020-396-RC1, 2021

Dear authors,

General comments

The manuscript describes three variants (FS, IA, and DA) in a biogeochemical model (FABM-NflexPD) coupled with a hydrodynamical model (GOTM). It clearly describes the differences between a fixed stoichiometry (FS) to more complex variants considering a dynamic acclimation (DA) and an instantaneous acclimation (IA).

Based on an idealized set up, the response of each model variant to irradiance and temperature is studied. Results show that adding higher complexity to the model creates differences in the model output that cannot be negligible. It is an interesting manuscript related to the scope of the journal and model development.

Overall, this manuscript presents an interesting approach to understand the dynamics of adding a flexible stoichiometry. However, the authors pose two objectives for this

manuscript that are not fully addressed, so it is suggested that more discussion is added to fully address the objectives specified. It is also necessary that the authors specify why they used an NPD model instead of the NPZD model available from the FABM library. Moreover, I think the manuscript is lacking a more extensive discussion (or conclusions) about the novelty of this work, which could be done by, for example, extending the discussion (or conclusions) to explain why this work is relevant, given the model development is interesting but there is not a comparison to observations. Under the idealized set up of the different model variants created in this work and without comparison to observations, the authors should clearly state the applicability of their work and why it is important to have an IA variant considered in further biogeochemical modeling research.

This manuscript is within the standards of excellence of the journal, but the authors should address the comments suggested here.

Specific comments

Introduction

L29 It is mentioned that acclimation models are now commonly used, but only Geider's model is included. Could it be added a couple more examples apart from Geider's paper?

L34-35 In the sentence 'Models that account for variations in cellular composition are indeed more likely to provide more realistic estimates of phytoplankton biomass and biogeochemical fluxes'. Can it be briefly explained how are estimates provided more realistic based on the variable cellular ratios?

L38 clones/types – what is the meaning of this? Could it be specified what are clones/types?

L42 ... 'response to changes in resource environment' ... - please describe what those resources are.

L48-54 Please add the units of Q, fv, fA, and the Chl:C.

L59-60 What is short-term in this case? Please specify.

L84-85 'Compared to the FS variant, do the results differ sufficiently to justify the additional complexities introduced by the IA variant?' - I think this is an interesting question to set as an objective of this study. However, it is not fully addressed in this work (see further comments in the conclusion section). If the FS model provides significantly different results than the IA model, is that a justification to add further complexity to models? Under an idealised setup and with no comparison to observations, how can you tell that the differences found are answering to your question? Because of the obvious differences between FS and IA stated in section 1.4, it is expect that the results of the simulation will differ between both approaches. I suggest this question to be rephrased, as it is hard to justify IA over FS when there's no comparison to observations, which would help the manuscript better ground the differences found. Also, add more about the answer to your objective (ii) in the discussion and conclusion section (see below comments for those sections).

Model description

L128 Which mechanisms mentioned in the Introduction is this referring to? I am not sure to what section of the introduction to look. Could it be specified which section? Or mention those mechanisms in L128.

L142, eq. 10 – Please describe what Qo is and reference where it is defined (Table 3).

L177 – Please describe what nutrient affinity is in this section.

L203 – Add reference at the end of the sentence (Table 3).

L248 – The sinking rate value was based on observations? Please say where was that value obtained from.

L254 – Mention the other meteorological variables assumed as constant.

Results

L287 – can the authors extend the explanation about the effect of DIN depletion? Are the DIN depletion differences the only reason for the differences between Ln (FS) and fc (IA + DA)?

L291 – In the contour plot for the net growth rate, it looks as if FS has higher values during the spring bloom in comparison to IA and DA.

L300 – Which sensitivity experiment?

L304-305 – saying 'before the onset of winter mixing' is vague. Do the authors mean one day before, the week before? I suggest it is specified which day/date this is. Moreover, it is confusing to see how 'substantially' higher are the values of DIN for the FS variant in comparison to IA and DA variants. To avoid confusion, please also specify what the DIN values are for the FS, IA, and DA variants before (insert date here) the onset of winter mixing. I can see there are differences during the summer period for DIN, but is that a substantial difference? Beware of the adjectives used if there is not a quantitative comparison.

L312-313 – the differences between Ln (FS) and fc (IA, DA) are large to be only explained by small differences in the DIN during the summer period. Could the authors give a thought to that difference and explain it in a short sentence?

L316 – Please describe more the differences between FS and IA, DA during the spring bloom. FS reaches a higher PhyC in March than IA and DA.

Discussion

L344-352 – the discussion in this paragraph mainly focuses in 3-D models, but this work considers a 1-D approach. For L349-350, the authors mention 'Recent applications of these models in 3D setups with realistic forcings (Kerimogluet al., 2017; Pahlow et al., 2020) have indicated that accounting for acclimation enhances the ability of models to reproduce field observations.' I suggest to also giving examples of 1-D models that have been developed with flexible stoichiometry (photo-acclimation) and have been compared to simpler variants and have been a better fit for field observations.

L365 – If computational costs were nearly negligible for the different variants in this work, adding a further state variable (e.g. zooplankton, phosphorus, etc) would have made the model more realistic and it would have been interesting to look at further biogeochemical processes in each of the variants and how they respond to the complexities added. I would suggest adding some thoughts in this paragraph about the importance of a 1-D model in terms of time- and space-effectiveness when referring to computational costs and to mention why not more state variables were added or if that is part of future work.

L387-388 'However, improvements in these specific aspects typically result in greater

discrepancies in other aspects, such as the timing of the spring bloom, or winter concentrations of nutrients and phytoplankton.' - Was a different tuning of parameters tried? In Table 3, from the papers the authors used, how was it decided, which value to use? Was it a mean of all the estimates or different phytoplankton species? Could it have been a better tuning for the FS variant without facing higher discrepancies in the output? I suggest that in the caption of Table 3 it is specified about how that data was obtained from those two papers and mention in this section of the discussion if different parameterisations were tried and what issues were encountered. Currently, it sounds as if the higher discrepancies due to tuning are only an idea and not something proven.

L416-419 – Comparing to N:P ratios it is hard in this manuscript as the model does not include P. I suggest including more discussion about how the different variants would affect the N:P ratio even if P is not a state variable in the model. This could be phrased as future work if the authors have interest in a follow-up for the N:P ratio.

L450-454 – Why was the NPZD model not used for this manuscript? Zooplankton is relevant for phytoplankton growth, especially in a location such as the one chosen in this manuscript, where seasonal changes are relevant. I think that there should be a strong justification as to why the NPZD model available from the FABM library was not used. How would the results be expected to change with an explicit zooplankton in each variant?

Overall, I think there should be a paragraph added with more discussion about the objectives set in this manuscript. Where the objectives fully addressed in this work?

Conclusions

L472-479 – It is not necessary to mention again that information as it is clearly stated in the introduction and model description sections.

For the conclusion, can the authors conclude more in terms of the objectives of the manuscript (section 1.4)? How do the authors justify the IA model without comparing it to observations and under an idealised set up? It is better or just different? If this is discussed in more detail in the discussion section as suggested, then adding a couple of sentences about this in the conclusions would create a good closure for the manuscript.

Code availability – Please add the Zenodo link in this section.

Technical corrections

Table 1 – in the Expansion/Value column, first row, do you mean $Phyto^2_N$ or $Phyto_N$?

L127-132 – please be consistent with the quotation marks when you mention each model variant.

Table 2 – What are the blank spaces representing?

Table 2 – For dimensionless variables, please write ‘dimensionless’ instead of a hyphen

Table 2 – for the units with (**), it is inconsistent to put gChl mol C-1. Table 2 caption states that they are in gChl gC-1.

Table 2 – Why are there Equations being named in the middle of the columns? Such as Eq. 14, Eq. 18, Eq. 26, Eq. 11 and the last NA? If they correspond to both IA and DA, please specify it in each column. Otherwise, it is fairly confusing.

L237 – The authors mention Eq. 23 and Eq. 17, is this correct? I think instead of Eq. 23, it should be Eq. 28. Please correct me if I am wrong.

L243 – n2 = 17m. Missing the n (greek letter).

L274 – Fig 4 h-i instead of Fig 3 h-i.

L282 – Fig 5 e-f, h-i instead of Fig 5 e-f, h-j

L283 – Is it Fig 5 j-l instead of Fig 5 n-o? Fig 5 n-o is for light limitation.

Figure 5 caption – for dimensionless variables/parameters, write ‘dimensionless’ instead of a hyphen.

Figure 6 – subplot (4,3,7) – change (h) for (g).

Appendix A, L501 – subscripts for growth and Q should be a multiplication (last term in Eq. A1).