

Authors' Response

We thank the editor and the reviewers for their careful reading of the manuscript and for their constructive comments. We have revised the manuscript accordingly and addressed all points raised by the reviewers.

Below we respond to each comment in detail. The specific comments are written in *italics* and are followed by our response.

Response to the Editor

Report #2's comment on deep chlorophyll / production maxima is one where I am more ambivalent. In ultra-oligotrophic regions where surface nutrients approach zero and throttle NPP, the greatest rates of NPP may necessarily occur away from the surface where nutrient conditions are less unfavourable. So I would tend to disagree with Report #2. Nonetheless, the prevalence of such locations, in space and time, may be an issue here, since I note that your model seems to significantly overestimate this (cf. Figure 11).

Our understanding of the comment is as follows. We agree that a deep chlorophyll maximum should occur, particularly in oligotrophic regions. The model does resolve a deep chlorophyll maximum (at least in the water column with sufficient vertical resolution). We do agree, however, that the correspondence with observed NPP could be improved.

In any case, I would encourage you to revisit these portions of your manuscript to see if additional clarity or detail could assist. Report #2 makes a number of other points that, again, I would appreciate seeing addressed where appropriate.

We have addressed all comments, in particular the quite constructive comments in Report #2.

Response to Report #1

Line 365: the sentence beginning with "(specify)" needs to be completed.

The text has been revised accordingly.

Line 381: Shouldn't "underestimate" be "overestimate" here? Accounting for DOC increases NPP, compared to not accounting for it.

Clarified in the revised manuscript.

Lines 367-368: "is a good proxy for". I do not agree. If the authors have evidence for this claim, that evidence should be cited and at least briefly explained. Phytoplankton do alter their C:chl ratios substantially by acclimation, and the C:chl ratio of different species (or strains, in the case of photosynthetic bacteria) do differ substantially. In the absence of such evidence, at least the word "good" should be deleted and this should be stated as a mere assumption. See for example: Kruskopf, M., & Flynn, K. J. (2006). Chlorophyll content and fluorescence responses cannot be used to gauge reliably phytoplankton biomass, nutrient status or growth rate. New Phytologist, 169(3), 525-536.

Good point. We have softened the statement and clarified the assumption.

Line 416: “the equatorial upwelling is less pronounced”. The figure shows only biomass, not any metric of upwelling per se. Did the authors mean something like the following? “biomass was not elevated [to the extent expected] in the equatorial upwelling region.”

Indeed. The text has been clarified.

Line 528: Shouldn’t “non-sinking” be “sinking”: The model includes silicate (mineral) in diatom biomass, which is assumed to remain neutrally buoyant while living, but does NOT include silicate in sinking POM. Or am I mis-interpreting?

It should indeed be “sinking”.

Lines 530-534: Could the failure to capture diatom biomass in equatorial upwelling regions not also result from failure to capture key trait values of the diatoms that dominate in equatorial regions?

We believe that the trait we are missing is that of active buoyancy control by diatoms. We develop this argument in the last part of the paragraph.

Line 537: Perhaps this is too nit-picking, but “variable stoichiometry” reads like an oxymoron, given that stoichiometry refers to fixed composition. How about “variable composition”? Changed to “variable stoichiometric composition”

Line 543: Given the modest (at best) agreement and the lack of any quantitative comparisons in the manuscript, replace “with some accuracy” with “in broad, qualitative terms”, or similar.

Revised accordingly.

p. 30, 2nd Paragraph: It is worth mentioning an alternative possible reason for the model’s overestimation of the contrast in NPP between oligotrophic vs. more nutrient-rich regions. This could also be because the model does not capture the systematic variation in the carbon-to-nutrient ratios of biomass, which are higher in oligotrophic waters.

This is a valid point. However, we do not fully understand why a higher C:N ratio would necessarily lead to higher NPP in oligotrophic waters, and therefore we did not include this explanation.

Line 573: “We believe that the pattern in passive/active copepods were ...” Does the pattern refer to that observed and modelled by Prowe et al. or to another (e.g., observed) pattern? In any case revise for clarity. For example, if the authors here refer to the pattern observed and modeled by Prowe et al., the sentences could be combined as follows: “... observed and modelled by Prowe et al. (2019), which we believe was due to ...”

We refer to the observed pattern. The sentence has been clarified now.

Some figures could be made easier to see and more informative by making the plots within each panel larger. This is particularly true for: the small global projections (Figs. 10, 12, 13, 14, etc) and for the line graphs in Figure 16. For example, making Figure 16 a full page figure, and enlarging the size of each panel (reducing the empty space between panels) would allow the

reader to better see the modelled patterns over the seasonal cycle, among the many lines, which are very close together in the small panels of the current version.
We have enlarged the global projections wherever possible.

Figures 16 and 17: Captions should specify which model configuration / location was used to generate the results shown.

The figure captions now specify the model configuration.

Response to Report #2

We thank the reviewer for the comments and acknowledge that the previous version contained several unclear formulations. These have now been corrected in the revised manuscript.

I. 57 and elsewhere: I would prefer primary trait over dominant trait

Corrected in the revised manuscript.

I. 84: from single-celled group -> from a single-celled group

Corrected in the revised manuscript.

I. 85: particular -> particulate

Corrected in the revised manuscript.

II. 160–165: unclear why two sets of indices (k,l) and (i,j). what is the difference?

Clarified that k and l refer to groups, while i and j refer to sizes

I. 167: delete the first maximum

Corrected in the revised manuscript.

II. 182–184: Density-dependent mortality is indeed commonly referred to as quadratic and what is termed constant in Eq. (5) is actually a linear mortality. It would be better simply to stick to the established nomenclature to avoid confusion

Corrected.

II. 184–189: This does not make any sense. If the predator:prey biomass ratio is constant then predator biomass must increase proportionally with prey biomass. So, unless the mortality coefficient ($\mu_{HTL,0}$) scales inversely with prey or predator biomass, this results directly in a quadratic loss term, since the encounter is the product of prey and predator biomass. The statement that "As the mortality is also a function of the predator:prey biomass ratio, it follows that the mortality is independent of biomass" is simply wrong, at least if, as stated, the predator:prey biomass ratio is constant. This becomes clear also when you insert the predation terms in Eq. (12)

We have removed the discussion about quadratic mortality, as it falls outside the scope of the present manuscript.

I. 191: formation should be formulation

Corrected in the revised manuscript.

II. 230–233: Although much better, this sentence is still very difficult to read, in particular the part "it regulates ...". I would suggest to split the sentence in two and explain the growth maximisation as the result of applying the law of the minimum.

The sentence has been revised and split into two for clarity.

I. 845: What are l , k here? L and k do not appear in (D1) to (D4) and are not mentioned elsewhere in Appendix D. Eq. (1) does not help here, as I cannot see the link between the variables in (1) and those in (D1) to (D4)

Clarified that k and l are group indices.

II. 244–246: Should this not also apply to predation? Applying the biomass correction only to viral lysis appears inconsistent. Also, why are viruses not considered for multi-cellular organisms (viruses do infect us humans)?

The multi-cellular organisms are already regulated by a quadratic mortality from higher trophic levels (HTLs).

II. 290–304: The TMM is a method for quickly spinning-up a (global) model into steady state and not suitable for short-term or real-time simulations. The acceleration of the spin-up is achieved by parallel execution and hence requires a high-performance computing environment. These should be emphasised to avoid undue expectations of what can be done with it.

We disagree that the TMM is unsuitable for time-resolved simulations. We have clarified that we used monthly resolved TMs and not annual.

I. 295: reporduces -> reproduces

Corrected in the revised manuscript.

I. 356: metrics -> metric

Corrected in the revised manuscript.

I. 365: (specify): something seems (again) missing

We have corrected the missing text and carefully checked the manuscript for similar issues.

Fig. 4: The explanation of j_{lieb} is unclear, please provide an equation for this (the previous ms did have one, but the equations changed a lot)

Updated to be fully consistent with the equations in appendix.

Please add some explanations for your equations, and check them thoroughly. I still have the strong impression that they are mostly based on pragmatism and simplicity, rather than first principles. This would actually be fine, were it not for the authors' claims regarding first

principles.

Eq. (B1.4): I see 2 major problems here. (1) The factor $1 - v$ ($= 1 - \text{active cell volume} = \text{cell membrane fraction}$, according to Eq. 7) is very small for larger cells and much greater for small cells, and I think the opposite would be closer to the truth. (2) This is multiplied with light intensity in (B1.10), which makes light absorption a linear function of light intensity. This is obviously wrong since photosynthetic pigments have a limited capacity to absorb light, leading to a saturating function of light intensity (or even declining at high intensity due to photo-damage). One might argue that light is not limiting at high intensity and the use of a minimum function in (B1.13) then could make the linear function acceptable. However, this is clearly not acceptable in light of the authors' claim that they tried to base the parameters of the cellular model on first principles.

We clarified the wording. v represents the membrane fraction, not the biomass fraction.

Eq. (B1.6): This make the max. synthesis rate proportional to the membrane fraction, which makes no sense. This seems to contradict the definition of the membrane fraction as $1 - (\text{cell mass available for active metabolism})$, Eq. 7).

As above, this has been corrected in the revised manuscript.

Eq. (B1.9): Why are all the $pC:X$ multiplied with N ? This sounds rather illogical and causes inconsistent units in (B1.12, B1.13). Why did you not use Eq. (8), which appears more logical? Corrected by replacing "N" with "X".

Eq. (B1.12): This equation seems rather nonsensical. It could make sense to add the carbon from photosynthesis and DOC, but certainly not the carbon associated with potential N and Si uptake.

Fixed. Carbon uptake now includes only L, DOC, and F.

Eq. (B1.13): The use of a minimum function for nutrient and light limitation (this is also unclear since X does not include L in B1.9, but otherwise light limitation could have no effect on g) is counter to current knowledge about light and nutrient limitation. So, again, this puts into question the authors' claim regarding the use of first principles.

Corrected as described above.

Eq. (B1.15): None of the three terms in (B1.15) is used for (10), so it remains unclear how (10) was derived, although (10) seems to make more sense than the third case in (B1.15).

Fixed. The last equation missed a term.

Eq. (B1.18) and (B1.19) appear cryptic to me and need some explanation.

Added an explanation in a footnote in the table.

Response to Report #3

We thank the reviewer for the positive assessment of the revised manuscript and for the helpful comments in the previous review round. We are pleased that the revisions and clarifications addressed the reviewer's concerns.

We hope that the revisions satisfactorily address the remaining comments and we thank the editor and reviewers again for their helpful suggestions.