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Reply on RC2

Genevieve Jay Brett et al.

Author comment on "Sensitivity of 21st-century projected ocean new production changes to idealized biogeochemical model structure" by Genevieve Jay Brett et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-479-AC3, 2021

We thank the reviewer for their thoughtful notes and suggestions to improve this manuscript. Responses to each comment are below and include the intended or in-progress changes to the manuscript. Note that original text of the review is in italics.

(1) *Assumption of "constant phytoplankton": The model implicitly assumes that phytoplankton is constant in space and time, with the concentration being included in mu_0. Therefore, one might expect a strong dependence of surface nutrients, new production, or climate sensitivity on the model parameters and biological time scale. Given this, and the fact that the structure of the model is in contrast to models applied in CMIP6 (as noted briefly in the conclusions), I think this assumption and the consequences, that might arise from it, should be discussed a bit more.*

We are assuming here that the total phytoplankton has an approximately constant effect on the new production rate, such that it may be subsumed into mu_0. This assumption does indeed lead to a strong dependence of near-surface nutrient concentration, new production, and climate sensitivity to the model parameters/biological timescale, as discussed in sections 2.2 and 3.1 (figures 2 and 3). Models which include the phytoplankton concentration generally are interested in simulating the total phytoplankton, not just the new production, and will also include remineralization. Our choice to simulate only new production and new phytoplankton will over-estimate growth when total concentration is low and under-estimate growth when total concentration is high in comparison to a production function including the new phytoplankton concentration. We thereby prevent exponential growth and limit spatial differences in behavior.

The second paragraph of section 2.2.1 has been expanded to include the expected effects of our simplifications and now reads as follows: "In designing the nutrient tracer, we make three simplifying assumptions. First, we assume that the deep nutrient pool has a fixed concentration, not dependent on explicit remineralization, which decouples the nutrient tracer from the export tracer. This assumption will create different vertical nutrient gradients than models with remineralization included. Second, we assume that new production depends on the availability of this nutrient and light alone, not on the water temperature or on the existing plankton population that may be sustained by recycling of nutrients; this omits processes thought to be important in bloom-type events (Behrenfeld and Boss, 2014) but again keeps the nutrient and export tracers decoupled. One may reframe this choice as subsuming an effectively constant total phytoplankton concentration into mu_0, which leads to an over-estimate of growth when and where total concentration would be low and an under-estimate of growth when and where total concentration would be high in comparison to a production function including the phytoplankton concentration. Finally, we assume that the light available for new production in the mixed layer is the mean of the light levels within the mixed layer (as done in McGillicuddy et al. 2003); below the mixed layer, productivity depends on the light at only the depth in question. This choice increases subsurface growth within the mixed layer and decreases near-surface growth, while allowing growth below the mixed layer depth."

This also concerns the normalization of alpha by alpha_0 (Line 195). What could be the (biological) meaning and implicit assumptions of alpha_0? The authors note that tau_bio shows a similar correlation for alpha_0 between 0.1 and 2, but not for a wider range. What range or value would be plausible? As alpha_0 is used to unify the rate constants k_N and alpha for the evaluation of tau_bio, I think this is very much at the heart of the paper, and should be discussed more.

Alpha_0 quantifies the relative effect of changes in nutrients and light on productivity in a single constant. The slopes of the production curves for N and I both contribute to the response of the production rate, which we describe as tau_bio. Alpha_0 is an expression of how we need to stretch the I coordinate so that the slope of Production(I) is equivalent to a Production(N) slope. We could imagine doing this using the ratio of expected values of I and N. Large values of both might be 200W/m2 and 20mmol/m3, leading to alpha_0 of 0.1. Any equivalence like this will likely lead to alpha_0<1. We could also expect slopes to be the same order of magnitude, so alpha_0/alpha would be similar to kn; this suggests alpha_0 similar to kn*alpha, which ranges between 0.0031 and 0.8 for our parameters. Since we are interested in comparing across parameter cases, a constant value of alpha_0 is best. Based on these considerations, our choice of alpha_0=1 is an upper bound.

We have added to the paragraph introducing the biological timescale: "Our α_0 is an expression of how we stretch the light coordinate so that the initial slope of production with respect to I is in the same units as the initial slope of production with respect to N, suggesting α_0 as a ratio of N/I. Given the relative values of I and N, $\alpha_0\leq1mmolN/Wm$ is likely to be the most fruitful.” We have also added to the next paragraph, following “This correlation is best for this and similar values of α_0, e.g. 0.1 or 2 mmol N/WM, but is lower for e.g. 0.01 or 100 mmol N/Wm.” the sentence “This is consistent with our understanding of α_0 as a ratio of N/I.”

(2) *Lack of subsurface remineralization and its potential feedback on surface nutrients: By setting w_s=0, the model assumes no particle sinking and subsurface remineralization below the euphotic zone. But: wouldn't we regard any nutrients remineralized in depths > 100m as new nutrients, which could then be injected back again into the mixed layers or euphotic zone? The effect of sinking is mentioned briefly in line 219: "For instance, with ws of 5m/day, sigma =1/yr has 95% of annual production sink below 100m." I assume that this value of 95% arises for the equilibrium case, i.e. from exp(-100/(5x365)), correct? Then, using these parameters, exp(-900/(5x365))=61% of the export at 100m would sink below 1000m, and 39% would be remineralized in the water column between 100-1000m, adding to the nutrient pool. The effect would be even stronger with the parameter for sigma applied in the study: with sigma = approx. 6/y (1/60days) only exp(-100*6/(5x365))=72% of the particles produced in the surface would leave the upper 100m and the remaining 28% would be remineralized within. While the latter nitrate, by definition, would not add to new production, it might nevertheless affect the gradients and thus supply of nutrients. Further, 95% of the flux leaving the surface would be remineralized above 1000m, thereby increasing the concentration of subsurface nutrients, and their potential re-injection into the surface. This feedback (which is possibly included in all global models run in CMIP6) may have considerable consequences for the model's sensitivity. Especially, since the "slow" case is considered as small phytoplankton (which might even sink more slowly), it may reduce the importance of term Delta Q in the subtropical southern Pacific (Fig 8) quite strongly. I think the specific setup of the model, and its consequences for the different terms should be discussed in much more detail.*

The definitions of new nutrients and new production are indeed flexible and could include nutrients that were remineralized below 100m and above 1000m. Such nutrients will be more important in cases where particles/detritus sink slowly and remineralize quickly. While the equilibrium calculation above is an accurate way to approach this point, we in fact did this work empirically, using our slow case with the above w_s and sigma values.

It is indeed the case that remineralized nutrients affect the vertical gradients of the nutrient field. This feedback mechanism is one that contributes to the challenges of understanding the CMIP class of models’ response to climate change: reduced near-surface nutrient in a warmer climate cannot be solely attributed to reduced physical supply from depth, but may also be affected by changes production, sinking, and remineralization rates that affect the nutrient concentrations from the surface downward. These effects will certainly change the sensitivity of new production to climate change. The advantage of not coupling the nutrient field to the particulate field allows us to more easily and definitively attribute causes to the changes in new production with climate.

We have added a relevant sentence when introducing this assumption, as noted in the previous point, "This assumption will create different vertical nutrient gradients than models with remineralization included." We also added a sentence to the first paragraph of the conclusions, "Second, the lack of nutrient remineralization contributions to the nutrient field and the constant value of the deep nutrient pool remove a mechanism of production feedback which can affect its climate sensitivity."

(3) *Given the differences to other models mentioned in (1) and (2) (and also in the conclusions), I wonder how one could apply or adapt this analysis to CMIP6 models (Lines 501 to 506). Would it be possible to apply this analysis to the BEC model, to which the present model is compared, and which is simulated in the same circulation? This might indeed be a good proof of concept!*

We agree that the first target for applying this analysis to a CMIP model would be the BEC model. The empirical methods discussed in 501-506 would determine tau_bio for the combined production of all phytoplankton, as opposed to the tau_bio for each phytoplankton class which can be determined analytically from the governing equations. Unfortunately, it is outside the scope of this work to include such an effort, as we anticipate it taking several months. Below, we describe in more detail plausible analysis options and the challenges involved.

The option to use a single column with plenty of all but one limiting factor and an injection of a burst of the remaining one at a few levels to determine the production slope is most straightforward but requires implementing BEC into a column ocean. The option to fit the production curve over the nutrients and lights, using the global ocean's variety to provide the range of data, does not require new simulation development but would require a novel analysis. The goal would be to identify the slopes of production at the edge cases where a single nutrient, or light, has a low level and the rest are plentiful. A challenge is that the community composition in different regions varies, such that it is not clear whether a single tau_bio may be an accurate representation. In all cases, combining the production slopes will require choosing normalizing factors, like alpha_0, for each term.

We have added a sentence to the relevant paragraph in the conclusions, "While computing an effective tau_bio is outside the scope of this work, we believe developing a reusable procedure for these intermediate-complexity models to be a useful next step toward interpreting climate change production projections."

Specific comments:

Line 5: "and export via sinking organic particles": as the sinking speed is set to zero, I don't think this is strictly correct, but implies that new production=export production.

This is exactly what we wish to suggest. The choice of zero sinking was for simplicity, as we do not discuss the export in detail in this work. We have experimented with sinking speeds of 1-10m/day, and future work will include a detailed examination of export.

Lines 23-26: The meaning of two sentences is not clear to me: What are the differences between "structural differences" and "a variety of different ocean biogeochemical models".

The first is meant to refer to the differences in the way physics is represented across models, including differences of numerical methods of implementation. The relevant section of the sentence has been expanded to "structural differences in the models' representations of physical processes produce”.

Lines 36-27: "The effects of both biogeochemical model structure and physical circulation–biogeochemical model interactions have been examined in isolation." - What is meant with this? That studies examined either physical or biogeochemical effects? (But see, for example, Romanou et al., 2014, https://doi.org/10.5194/bg-11-1137-2014 or Kriest et al. (2020), https://doi.org/10.5194/bg-17-3057-2020, who both investigated the effects of physical model and biogeochemical setup at the same time.)

Many papers do focus on one aspect or the other of the problem, but we have updated this sentence to no longer imply that there are no papers doing both. Thank you for suggesting these papers in particular; we have included them alongside Loptien and Dietze (2019) in a new paragraph of the introduction focused on examples of papers considering both parts of the problem.

Line 42-44: "(...) differences in mixing that cause small changes in temperature and salinity or global production and biomass, respectively, create large differences in primary and export production (...)" - What is the difference between "global production" and "primary and export production"?

We have split this sentence into two in order to clarify that these are referring to fields in the two different studies.

Line 75: Wrong section number?

Yes, all sections numbers here have been updated, thank you.

Line 95: "to minimize interannual drift" - I assume that with a longer simulation time drift *could be even smaller; perhaps better: "To reduce annual drift"?*

Yes, we have replaced ‘minimize’ with ‘reduce’.

Line 145: POP has not been defined before. I assume that it is particulate organic phosphorus. But, given that the basic unit of the model seems to be nitrogen, shouldn't it rather be PON (particulate organic nitrogen)?

POP is the Parallel Ocean Program, the ocean component of CESM. Both uses are now noted as CESM-POP to avoid confusion and we define POP near the beginning of the methods section.

Eqn. 4: If w_s is set to zero (line 159), why mention this loss term at all?

This term is included so that this model can be used more broadly. As mentioned above, we have varied this parameter in our efforts and future work will include analyses of export. We decided including variations in w_s here would over-extend this piece.

Line 153: "is the specific mortality rate of particles". Particles (as a general term) don't have a mortality rate. I would suggest to choose a different, more general expression, such as "decay rate".

Yes, we have changed this as you suggest.

Line 159: What was the reason for choosing w_s = 0? Please specify the units of w_s.

We have added the units, m/day. W_s=0 gives the best comparison between P and BEC's phytoplankton near the surface; BEC does not have a similar sinking term, but instead a vertical redistribution process.

Line 162: Please specify the units for mu_0, k_N, alpha.

These are noted when the terms are introduced and are now included when giving their values for the sensitivity study.

Line 167-168: It would be interesting to also see the comparison of the model results to those of BEC (as this is also done for particles).

We have added the BEC surface nitrate concentration curve to figure 2a. This model also misses the location of the northern hemisphere peak, suggesting this may be due to the model circulation, but is much closer to observations in the Southern Ocean.

Lines 171-172: "Although our particles represent both living matter and detritus, near the surface this P is most like newly-produced plankton which we expect to have the same spatial patterns as total phytoplankton." - Why not compare directly against BEC's phytoplankton and detritus?

As stated in the quoted sentence, near the surface our P is typically newly-produced and thus more similar to phytoplankton than to detritus. We felt the additional process of the transition in BEC from plankton to detritus would make it a poor comparison.

Line 171: Should be 16/117 mol N/mol C (units), correct?

Yes, we have added the units.

Lines 172-175: But then why compare the model against WOA nitrate? (See my above comment.)

Now that we have added the BEC nitrate to figure 2, we hope it is clear that BEC misses some of the observed patterns. We felt it best to use observations for N and model data for P in part due to the relative sparseness of in-situ phytoplankton concentration observations; the text has been updated here to better express this idea.

Line 195: Eqn. number is missing.

The equation number has been added, thank you.

Fig. 2: Why use units of gN/m3 Is this averaged over the upper 100m? Shouldn't it rather be mmol N/m3?

Yes, these are properly mmolN/m3, this has been fixed. These are the surface fields, not the mean over the top 100m.

Line 240-241: "As μ0, the maximum growth rate, is held constant," - Does this mean "As μ0, the maximum growth rate, does not vary in space or time"?

Yes. We have updated the text as you have written here.

Eqn 6: Shouldn't the sign of the third term on the RHS be negative?

No. This equation has been updated to include the detailed definition of each delta term; a more detailed response was written to reviewer 1's first point.

Fig 3: Again, why gN/m3? Also, I would suggest to label the panels with (a) - (b) -(c) ... in reading direction, as for Fig 4, to avoid confusion.

The panels are labeled in the order they are referred to in the text, to assist in following along. The nutrient units were incorrect, and have been fixed to mmol N/m3.

Line 262-263: "but the reduction percentages highlight that the changes are somewhat insensitive to the varied light- and nutrient-limitation choices." - This is unclear to me: does this refer to panel (d) of Figure 3? They seem to decline considerably (by more than 10%) between tau_bio=4.5 and tau_bio = 160.5. But I might be miistaken. Perhaps using a % scale (as in panel (b)) would be better to compare the two diagnostics?

We show absolute values of the change in mean N concentration rather than percent change because the absolute values do have a notable range that varies with tau_bio while the percentages do not. We have rewritten this sentence: Both initial concentrations and absolute reductions are smaller for shorter tau_bio, but the small range of reduction percentages (15-22%) highlight that the changes are somewhat insensitive to the varied light- and nutrient-limitation choices.

Lines 288-298: Here, and elsewhere, the correlations are mentioned. I would suggest to combine those in one or two tables, and highlight those that are significant.

Yes, we have created table 2 for this purpose.

Lines 314-315: "while the global differences are consistent with the chosen parameters in that the faster cases, which have a higher nutrient utilization, have new production more correlated with the reductions of near-surface nutrient and its vertical supply." - What does "higher nutrient utilization" mean? More production? Could the stronger correlation between new production and nutrient supply and concentrations be caused by the higher k_N of the fast case (with k_N=1 mmol N/m3 and nutrient concentrations in that range for large parts of the tropical and subtropical ocean)?

Your intuition has done well interpreting this point. We have adjusted the middle clause to read "which have higher production and lower near-surface nutrient". The correlation cannot be explained by k_N alone; we think tau_bio is a better explanation.

Line 357: "The"

Yes, thank you.

Line 375: "austral winter and spring"?

Yes, we have added 'austral'.

Lines 383-393: This paragraph introduces (the effects of) KPP, Redi and GM. These should be explained in more detail, as not every reader is familiar with them.

We adjusted this discussion to use more-accessible language rather than the jargon of the names of these parameterizations. The sentence introducing the parameterized mixing now will read: "These fluxes are comprised of downward advective fluxes, upward fluxes from parameterized vertical mixing (Large et al., 1997), and upward fluxes from vertical effects of parameterized along-isopycnal mixing (Gent et al 1995)." These adjustments will also be made to the labels of figure 9ab.

Fig. 8-11: To my opinion, nutrient supply should not be given in gC/m2, which somehow weird. If the unit should be comparable to new production, I'd rather suggest to give both in units of mmol N (which seems to be the basic unit of the model).

We appreciate your insight. We are using gC as the production unit throughout in order to have the rates be easily referenced to other works in the mind of the readers. While this is odd for nutrient supply, we do indeed want the units to match, as you surmised. We have decided against remaking these figures.

Line 403-404: "The Arctic region is defined by being within the Arctic circle (north of 66.5N), or having at least one day per year with no incoming solar radiation." - The logics of this sentence is not clear to me: Does this mean "either-or" (i.e., even including regions south of 66.5, if they have at least one day without insolation), or is the insolation criterion a consequence of phi>66.5?

These criteria are equivalent, and 'equivalently' has been added after 'or'.

Line 408: "Early century" - Do you mean the year 2000?

Yes, this sentence now begins “Seasonal cycles in the 2000s”.

Fig 9: "Profiles of change" - Year 2100-year2000?

Yes, and we have added this to the caption.

Fig 10: Is the magenta line really Delta Q Delta L, and not rather L Delta Q?

Yes, it is deltaQdeltaL.

Fig 7,8,10,11: What are the units of the denominators for normalization (0.055, 0.14, etc.)?

These normalizations are values of QL, which is unitless (as are both Q and L). This is now noted in the text, Section 2.3: “As mu_0, the maximum growth rate, does not vary in space or time, we can examine simply the nutrient availability, Q, and the light availability, L, both of which are nondimensional and have values between zero and one, as does their product... These difference terms are also nondimensional and have values between negative one and one.” We have also noted the nondimensional terms in the captions for figures 6 and 7.