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Comment on bg-2020-479

Anonymous Referee #2

Referee 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-RC2, 2021

The paper "Sensitivity of 21st-century projected ocean new production changes to idealized biogeochemical model structure" by Brett et al. applies a simple global biogeochemical ocean model to investigate the sensitivity of new production to three phytoplankton growth parameters. This is done by first comparing the output of 27 model experiments with different combinations of these three parameters under present-day and future forcing/circulation. By combining the three parameters to a functional form representing the time scale of (phytoplankton)plankton growth the authors can relate this time scale to surface nutrient concentration. Two parameter sets of slow and fast turnover are then chosen to represent small and large phytoplankton. These two model simulations are further analyzed with respect to their sensitivity to climate-induced changes in nutrients and new production, with focus on the global seasonal cycle and three different regions. The simple form of the biogeochemical model allows the decomposition of climate-induced changes due to nutrient and light limitation. It is shown that for this model shorter timescales are associated with larger global annual new production, but also greater declines of this diagnostic in a warmer climate, with different contribution of nutrient and light terms in different regions.

The analysis of different process contributions (nutrient supply, light availability) to projected decline of global production and its decomposition into different parts can be a useful contribution to our understanding of how production in global models is affected by climate change. The simplicity of the applied model enables the derivation of useful analytic tools. However, I am not sure to what extent the results and methods of this paper are applicable to the more complex models. In particular, I think that two aspects of the model's structure require more consideration and discussion:

(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. 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.

(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.

(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!

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.

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".

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.)

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"?

Line 75: Wrong section number?

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"?

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)?

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

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".

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

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

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).

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?

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

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

Line 195: Eqn. number is missing.

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

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"?

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

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.

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?

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.

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)?

Line 357: "The"

Line 375: "austral winter and spring"?

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.

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).

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?

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

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

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

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