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Final reply on RC1

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-AC2, 2021

Note that original text of the reviews is in italics. A good portion of the response to Review 1 was posted during the open discussion period, but planned changes to the manuscript are now noted. We repeat portions of the responses to the major comments that have additional information, but generally we focus here on responses to minor comments. We thank the reviewer for their attention to detail and their efforts to help us improve this manuscript.

Major comment response updates:

Section 2.3, On the computation of L:

Our climate change scenario directly impacts the incoming radiation and the sea-ice coverage of the ocean, changing the light incident at the sea surface. See Fig 1a, which shows changes in the minimum incoming short-wave radiation of 10-25W/m2 over most of the ocean. Then the mixed layer depth changes further impact the light availability through the averaging you noted. The reason for computing L from production rate and Q(N) is that the mixed layer depth, and thus L, can change rapidly. Production rate and N are averaged on-line in our model, and thus computing L from them allows us to have a time-averaged L that would not be possible to compute from the time-averaged mixed layer depth and incoming radiation. Note, however, that sub-monthly covariability of Q and L projects onto L using this approach. This explanation has been added to the text in the first paragraph of Section 2.3. For future development of models, it is a good idea to have as output an online-average of nonlinear functions like L and Q.

Section 3.1, on pattern correlations:

We have added text after equation 6 (now 7) in Section 2.3 on how we will analyze pattern correlations, including the reasoning for the assumed degrees of freedom for the global ocean. We have also added that small pattern correlations, like those of dQdL with d(QL), are insignificant. Finally, we have collected our correlations into a table (Table 2 in the manuscript) as requested in the second reviewer's comment and note those which are significant.

Section 3.2

This section concludes that there are different mechanisms that govern changes in the seasonal cycle of production than that govern changes in the annual average of production. However, I remain unconvinced by this conclusion. The results shown seem to be driven more by regional differences than by seasonality. The largest seasonal cycles are in the high latitudes while the low latitudes have weak seasonality. The results show that changes in the seasonal cycle and the mechanisms driving those changes are similar to the arctic and sub polar North Atlantic, perhaps with more influence from the Southern Ocean in the slow timescale case.

Furthermore, the statement in lines 344-346 is only true for the fast case but not the slow case, again likely due to different responses in the high latitudes with each of the timescales.

The reviewer seems to infer that we are making more sweeping claims than we intend to convey. We agree with the reviewer in that global annual-average production is reduced in the warmer climate mainly due to reduced nutrient availability (LdQ), and this is reflected in the seasonal cycle results as well. Looking at the seasonal cycle, production is reduced mainly in latter half of the growth season, which is shortened. In both the fast and slow cases have different contributions from light availability and light-nutrient covariance during the latter half of the growth season. We rewrote the final paragraph of this section to make these points clearer, including the fact that the noted statement is only in regards to the fast case. The final sentence now reads, "In both our cases, reduced nutrient availability is a major contributor to the shortened growing season, indicating a consistent mechanism for reduced total production in response to the climate perturbation."

Methods: How effectively is the nutrient mixed within the mixed layer? The light is averaged over the mixed layer, however there is a comment about productivity being enhanced below the mixed layer depth (line 469). Does this mean that light is more effectively homogenized than nutrients due to the mixed layer not being an actively mixing layer? How does this affect the results about the mechanisms that drive changes in productivity?

Line 469 should (and has been changed to) read "enhances production in the lower portions of the mixed layer"; the averaged light enhances light availability, and thus production, in the deeper parts of the mixed layer.

We have added a sentence to the text discussing the global spatial patterns of production and nutrient concentration: "We note that the variations in nutrient concentrations within the mixed layer are typically small, less than half the mean concentration and much less than k_N."

Would the global average statistics differ if the Southern Ocean were excluded?

With regards to the Southern Ocean, modeling production here with fidelity to real processes requires the inclusion of iron; we now note this in our section 2.2.2, paragraph 3. While one could get closer to observations with our idealized model, the deep concentration and half-saturation coefficient, at minimum, would need to change. The production in the Southern Ocean is close to half the global annual production (see appendix figure B1). The reductions in global annual production are 8.5-11% without the Southern Ocean, as opposed to 9.5-19.5% when it is included. This has been added to the end of the first paragraph in Section 3.1: "A large portion of this variability in global new production is related to the Southern Ocean, which is the basin with largest production and which our model does not represent well. Without the Southern Ocean the reductions in global annual production are 8.5-11%, which is smaller than the range of export decreases in CMIP5 and within the range of net primary production changes in both CMIP5 and CMIP6." With regards to the correlations we included in our first response on this topic, we have added to the end of the paragraph on the pattern correlations a sentence: "These pattern correlations are qualitatively similar without the Southern Ocean, which we do not represent well with this model."

Minor Comments

Introduction:

--Paragraph at line 30: it is unclear what is meant by "essential properties" here although the phrase is repeated multiple times in this paragraph

We have eliminated this phrase. The goal is to elucidate the sensitivity of the global-warming response of biological productivity to nutrient and light limited productivity dynamics, which we do using a single particularly simple form of joint single-nutrient and light limitation.

--Line 65: add citations to the specific prior work to which you are referring

We have added here references to Kriest et al 2012 and Levy 2015, which were noted earlier in the discussion.

--This study is not the first to use simplified models to discuss biophysical coupling. The introduction should cite more of the relevant theoretical literature such as: Smith, K. M., Hamlington, P. E., & Fox-Kemper, B. (2016). Pasquero, C., Bracco, A., & Provenzale, A. (2005).

We do not wish to imply we are the first to use this method, but we also wish to keep the discussed literature focused on studies similar to ours, which are generally those using global models. These papers are now included for context alongside the OCMIP-2 studies.

Methods:

Line 95: Are the tracers initialized in the model at the beginning of the spin up or only at the beginning of the 10 year timeslice? What is the tracer initial condition?

This clause now reads "we perform a 20-year physics-only spin-up, which is sufficient to reduce interannual drift in the physical state, and then use 10 further years, which include our biogeochemical model, as our early-century timeslice." Around line 150 (section 2.2.1) we have added the initial condition: "The initial conditions for N, which are used at the start of both 10-year timeslices, are a linear interpolation between 20mmol/m3 at 1000m and 1mmol/m3 in the surface gridcell."

Line 111: Figure 1 shows more changes than are outlined briefly in this section. There are some prominent changes like a speeding up of the ACC that are not mentioned.

The changes we present here are those we consider most important for the remainder of the text. We have added your point about the ACC and a few other details. These changes are not exceptional compared to the CESM-LE results and so we felt they did not merit much time.

Equation 1: what is the functional form of the restoring S? Is there a parameter that relates to the nutrient restoring rate?

We do not have a restoring process in the typical sense, but rather a reset process, which adjusts all N values below 1000m to 20mmol/m3 every timestep. The text here has been changed from "restored" to "continually reset" to be more accurate.

Line 154: please explicitly state the advection and mixing methods, which form part of the *results later in the paper.*

We are using default values for this model. We will adjust the relevant sentence to provide the appropriate reference: "The physical transport and mixing are done by the same mechanics as existing passive tracers in CESM-POP, with a third-order upwind scheme for advection and diffusive mixing that is spatially variable due to parameterizations of mixed-layer, submesoscale, and mesoscale isopycnal processes (see Section 2.2 of Danabasoglu et al., 2020, for details)."

Line 165: This is a key point about equilibration, but as it is currently written it is confusing what is being compared.

Thank you for noting this. We have split this sentence into two, with the second now reading "We do not see substantial differences in our climate change results when using year 5 or year 10 of the timeslices in our computations."

Section 2.2.2 is a results section rather than a methods section

We disagree. The question under consideration is the sensitivity of new production projections, not the sensitivity in the current climate. The purpose of 2.2.2 is to determine which parameter choices are worth including in the main analysis, and so we see it as a continuation of the description of the model.

Results:

Use consistent terminology for averages. Sometimes average is used and sometimes mean is used. If these are the same, please use just one term.

Thank you for noting this, we have replaced 'average' with 'mean' whenever referring to our results.

Line 277: The text makes it sound like the patterns don’t change much, but the the correlation seems very low (r = 0.26).

The same is true at line 481-2.

With about 82,500 ocean gridcells, a reasonable decorrelation in both directions of 10 gridcells (6-9 degrees latitude or longitude) allows 825 degrees of freedom. Correlation coefficients above r=0.09 are significant. This has now been noted in the methods section following equation 6 (now eqn 7).

The correlation values are presented for multiple combinations of variables and averages of those variables but it is at times unclear what is being correlated. One example is line 293.

We have made a table (which will be Table 2) of all such correlations to make this clearer.

Line 305: this sentence has confusing wording

Thank you, we have split this into two sentences: From the spatial fields, QL and Delta(QL) show the same spatial patterns as production and its changes, respectively, as expected by definition. However, Delta(QL) looks somewhat different from the percent changes in production shown before (Fig.4).

Line 330: do you mean to repeat qualitative twice in this sentence?

Yes.

Line 403: Is the Arctic region defined using two criteria or are these two criteria equivalent?

These are equivalent, and we have added ‘equivalently’ following ‘or’ to make this clear.

Conclusions:

Line 466: “other processes” is confusing because the next sentence discusses productivity processes that were not included.

This now reads “certain aspects”, in order to keep the focus on production.

An overall note on the grammar is that the sentence structures can be repetitive with a few sentences in a row beginning with the same clauses (e.g. “here”).

We have removed 3 of 11 instances of the use of “here” throughout the manuscript.

Figures:

Figure 5 caption: Expand on what is meant by "Annual and 100m mean nutrient concentration." Is this an integral over the upper 100 meters? Include all information in the caption (what is shown in panels a, b, c, d).

Updated caption: (a,b) Nutrient concentration, mmolN/m3, averaged over a year and the top 100m. (c,d) Percent change of this field from 2000s to 2100s climate. (a,c) slow case, (b,d) fast case.

Figure 6: the color scales appear to be saturated. Could the minimum and maximum values be includes as annotations on the figures?

Yes, they are saturated. The min and max values will be added below each panel here.

Figure 6: What is meant by 100m average? Is that an integral over the upper 100 meters?

It is the average over the top 100m, or the integral over 100m divided by 100m. The first sentence of the caption now reads "QL for 2000s climate (a,f), its change (b,g), and its components, all averaged over 1 year and the top 100m, then normalized by the maximum of QL in the 2000s."

Figure 7: This figure would be easier to interpret if panels c and d were both underneath b.

We will consider this shift when preparing an updated manuscript and will make this change if we find a way to do so without excessively increasing the size of the figure.

Figure 9: Over what depth range are these values averaged?

The vertical fluxes are at 100m depth, not averaged over any range of depths. We have updated the caption to include this point. This is consistent with the flux in Fig 8a.