Comment on egusphere-2022-579

Anonymous Referee #1

Referee comment on "Anthropogenic climate change drives non-stationary phytoplankton variance" by Geneviève W. Elsworth et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-579-RC1, 2022

Reviewer's comments on the manuscript "Anthropogenic climate change drives non-stationary phytoplankton variance" by Elsworth et al. (2022)

General comments:

The authors investigated future changes in interannual variability of phytoplankton carbon biomass by using the CESM1 large ensemble simulation. Their results seem to indicate highly spatially heterogenous response of interannual variability in the biomass to the global warming by the end of 21st century and relatively important contribution from changes in "top-down" control of the phytoplankton growth.

I totally agreed with the authors' initial point that, especially in the context of ocean biogeochemistry, future changes in variabilities have not been paid much attentions compared to those in the climate mean states, although these are critically important on decisions of mitigation and adaptation policy. I don't think that this study has no potential for being a step to help our understanding about the ocean ecosystem (including from lower to higher trophic levels) response to climate changes. However, I can not recommend the editor to publish the current manuscript from the BioGeosciences, because of the following two concerns: (1) Model validity and (2) authors' conceptual misunderstanding about MLR analysis.

(1) Model validity: The model ability to represent observed variability is critical on judging if projected future changes are valid. The author must show 1) "additional model-observation comparisons" with choosing the variables which are relevant to this study's focus (i.e., phytoplankton biomass) and 2) "evidence" on which results projected from the model with biases can be considered conclusive.

The authors showed the model-observation comparison of variability of annual mean phytoplankton carbon biomass (main target of this study) in Figure 1 and mentioned "Similar spatial patterns (to observation) are apparent (in the model)" in L139. But, for me, obviously, the model special pattern has different spatial characteristics from the observations. In the high latitudes, the observation shows the maximum variance in the most pole-side latitudes in the both hemispheres, while the model shows the variance maximum in somehow equator latitude around 50-60N and 50-60S. In the equator, although there is a strong latitudal maximum along the equator in the model, no such structure can not be seen in the observation, rather higher variabilities are observed in the off-equatorial regions. Moreover, model overestimations of the observed variability can exceed 200% in the equator and the subpolar North Atlantic.

The author also showed the model validity by comparing global internal variance in chlorophyll between the model ensemble and observational ensemble (Figure S2). However, this study's focus is the phytoplankton carbon biomass, not chlorophyll, and these two can have very different spectrums. I think that model-observation comparisons in the biomass are more suitable for the purpose and the author should assess the model in the regional scale (not global), given the spatial heterogenous response of the biomass.

(2) MLR analysis: The methodology is unclear and it seems wrong.

The author tried to reconstruct the contribution of each driver variable to phytoplankton biomass using the MLR coefficients (equation 3 and 4). However, it obviously failed. As shown in Figure S6, the reconstructed "Total Carbon" is not equal to the sum of other terms (i.e., equation 3 and 4 are not correct), maybe because of inaccurate MLR coefficients, neglecting offset term or strong multicollinearity between variables (e.g., MLD and SST, SST and Solar).

Linear decompositions should be applied for "change/anomaly", not for "climatology (10-year mean)".

Given a function F(X,Y,Z), in general, the first order Taylor expansion is robust only for a small change in the F (ΔF),

ΔF = (∂F/∂X)ΔX+(∂F/∂Y)ΔY+(∂F/∂Z)ΔZ + (Residuals from high-order and cross terms).

The author should apply such analysis for "change" (not "climatology") with considering residual terms. As the authors also mentioned, the partial differential coefficients are time-varying. The authors should be able to calculate the coefficients analytically using the model equations of phytoplankton carbon biomass.

Specific comments:

L29−: Any reference? And, does this mean that the CESM1 shows the opposite response of the high-latitude biomass to the global warming? (Figure 3a shows increase in biomass only in the sea-ice biome)

L49−: Please elaborate "*Clarifying how variance in phytoplankton biomass may be changing over long time scales with climate change is important for fisheries management, especially at regional scales.*". What kind of impacts on fisheries by changing in variance in Phytoplankton biomass can one expect?

L82−85: I could not understand clearly. Please clarify with showing equations.

L94−97: The author's description of the experimental setting of CESM1 large ensemble is inaccurate. Please describe it correctly.

L99–101: Show figure as an example.

L118–120: Please provide the map of the aggregated biological provinces used in this study as supplementally figure or superpose the biome boundary on the main figures (e.g., Figure 3).

Figure 1: Please use same colormap and same value range for fair comparison. And, it is better to show the ensemble mean of the σtemporal with a rank analysis (to show whether the observational σ is inside of the ensemble spread at grid by grid).

L179: Figure 2d?

L213–216: Which regions did the author chose? Please show these on map.

Technical corrections:

I don't list any small technical/editorial corrections at this time. Above-mentioned conceptual/major comments should be addressed or fixed by the authors before going into the detail.