Review 3 of Elsworth et al. (2022) “Anthropogenic climate change drives non-stationary phytoplankton variance”, submitted to Biogeosciences.

In this manuscript, the authors use the Community Earth System Model I Large Ensemble to evaluate the impacts of anthropogenic climate change on long-term variability in phytoplankton distributions within the global ocean. The authors additionally use a multiple linear regression to evaluate the ecological drivers of this change, reporting zooplankton grazing as being a major factor in reducing variability in phytoplankton biomass.

The analysis of earth systems models is well outside my area of expertise. So while the authors’ main finding that variance in phytoplankton biomass is anticipated to decrease in the future ocean seems informative from my perspective, I defer to the first reviewer’s comments regarding best practices in model interpretation. I was interested to see the multiple linear regression results, which seem to highlight a particularly strong coupling between phytoplankton biomass and grazing in model results. However, by the authors’ admission on L265, it does not seem possible to establish cause and effect regarding the nature of this interaction. With this, it seems like an overstatement to suggest (as in the abstract and elsewhere) that these results provide evidence for grazing-driven declines in phytoplankton biomass.

More importantly, insufficient documentation is provided for the reader to interpret the MLR results. Critically, it is not immediately clear from the text how contributions to phytoplankton/diatom variance were calculated. Equations should be provided, and associated details on the MLR analysis should be moved to the methods section to make this information easier to locate in the manuscript. Moreover, the MLR results themselves seem insufficiently documented. No details are provided on the overall model fit nor on uncertainties associated with the MLR coefficients. The relationship between the parameters □quationns 3 and 4 and the larger set of parameters included in figure 5 is unclear as well.

The discussion should also be expanded to provide more context on the authors' interpretation of these results. Altogether, even after reading the manuscript several times, I’m not sure why the results shouldn’t be interpreted as a weakening of top-down control in the future ocean (with the decrease in contributions to phytoplankton biomass variance due to grazing in Figure 5 reflecting a reduced coupling of phytoplankton biomass and grazing and, by extension, a strengthening of bottom-up controls). If this interpretation is beyond what can be determined based on the analysis (for instance because of large uncertainties in coefficient errors), this is not evident from the information provided.

Without this information on the MLR results, it is impossible to critically evaluate some of the the manuscript’s main conclusions. With this, and in light of the comments made by the first reviewer regarding issues with the authors’ analysis of the CESM results, I cannot recommend this manuscript for publication without major revisions. A few specific comments are provided below.

We agree with the reviewer. We have made multiple changes to the wording in the manuscript to address this point. These modifications are listed below.

We have changed the text on Line XX: "In these high-latitude regions, bottom-up controls (e.g., light, nutrients) have only a small effect on biomass variance. Rather, the declining internal variance in phytoplankton biomass co-occurs with a reduction in zooplankton grazing variability. Similar patterns emerge in the biogeochemically critical regions of the Southern Ocean and the Equatorial Pacific."

We have changed all wording about "driver / drivers" to "contribution / contributions" throughout the manuscript.

We have modified the topic sentence of the paragraph Line XX: "The declining internal variance in phytoplankton biomass co-occurs with a reduction in zooplankton grazing variability".

We have changed the language on Line XX: "The declining internal variance in phytoplankton biomass co-occurs with a reduction in zooplankton grazing variability." And on Line 275: "…".

We have modified the text on Line XX: "Statistical analysis of these specific regions reveal the decline in phytoplankton biomass variance to co-occur with a reduction in zooplankton grazing variability, consistent with previous studies (Bopp et al., 2001; Laufkötter et al., 2015)."

We have changed Line 289: "our study demonstrates a strong connectivity between phytoplankton and zooplankton grazing variance…."

We have changed Line XX: "Our study demonstrates a strong connectivity between phytoplankton and zooplankton grazing variance in the subpolar North Atlantic and the subpolar North Pacific."

We have modified Line 307: "Our regional analyses suggest that both phytoplankton and zooplankton grazing variance are likely to change with anthropogenic warming."

We have clarified Line XX: "However, our regional analyses suggest that both phytoplankton and zooplankton grazing variance are likely to change with anthropogenic warming."

Specific comments:

L114 – 115 – A quick review of the method used in Tagliabue et al. would be useful here. What were the multivariate statistical methods used? How were they applied? A map of the biomes would be informative as well.

This is an excellent suggestion. We've included a description of the methods used by Vichi et al. 2011.

Line 113: "We classified the marine environment into 11 ecological cohesive biomes as in Tagliabue et al., 2021 and Vichi et al., 2011, which are a consolidation of the 38 ecological regions defined in Longhurst et al., 2007. The provinces were aggregated using multivariate statistical analysis of physical (i.e., salinity, temperature, mixed layer depth) and biological (i.e., chlorophyll concentration) ocean parameters to group ocean regions with similar physical and environmental conditions (Vichi et al., 2011). Analyses were performed by randomly selecting from a

combination of model and observational datasets and testing for statistical significance using analysis of similarities (ANOSIM) (Vichi et al., 2011)."

L159 – 161 - This text feels more appropriate for conclusion/discussion.

We agree. We have removed this text from the manuscript.

"In the North Atlantic subpolar gyre, the phytoplankton biomass declines by 40-50% of its mean (Figures 3a, S3a)."

L215 - FAO citation and the associated reference seem to be improperly formatted

Thank you for this comment. We have reformatted the FAO citation in the references. The references is cited parenthetically as (*FAO*, *2020*). We will ask the editorial staff for clarification if/when the manuscript is accepted.

"FAO. 2020. *The State of World Fisheries and Aquaculture 2020*. *Sustainability in action*. Rome."

L219 – 234 - This text feels more appropriate for the methods section

Thank you for this suggestion. We have moved this text to the methods section.

L289 – 291 - Is this conclusion inconsistent with the disclaimer provided at L264 – 266?

Equations 3 & 4 — Why are the terms in the equations (e.g., Solar, SST, Nutrient, etc.), different from those included in figure 5? Were the equations in the text just providing a summary of the actual equations used? If so, this should be made explicitly clear, with some description of all the variables included.

Yes, the terms in the equations (e.g., Solar, SST, Nutrient, etc.) are the same as those included in Figure 5. We have added the terms parenthetically below the variable names in Figure 5 to clarify this point.

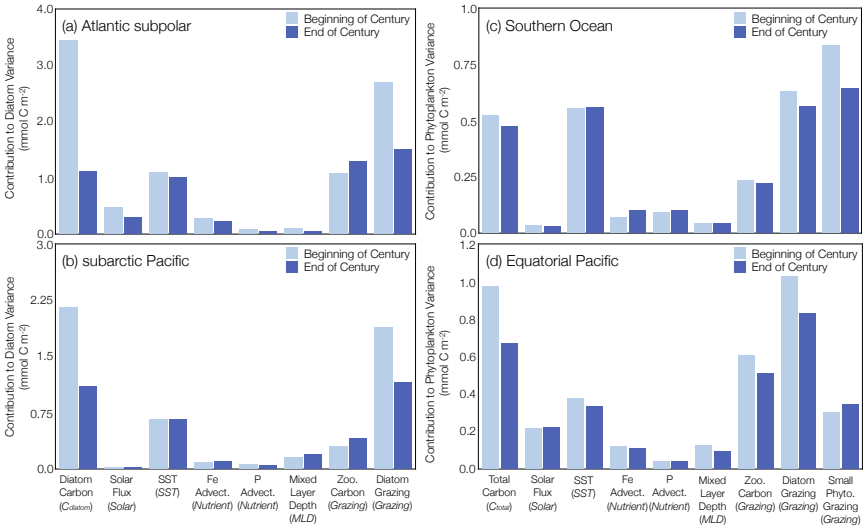
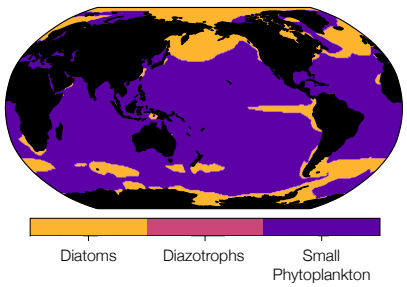


Figure 5: "Reconstructed changes in the contribution of each variable to phytoplankton biomass standard deviation across the RCP8.5 forcing scenario (2006 to 2100) with the beginning of the century shown in light blue and the end of the century shown in dark blue. Marine ecological regions are defined in Tagliabue et al. (2021). Regions were selected which aligned with the highest fisheries catch in the (a) Atlantic and (b) Pacific basins and the biogeochemically important (c) Southern Ocean and (d) Equatorial Pacific regions. The dominant phytoplankton functional type is considered in each region. In regions with a mixed ecological assemblage, total phytoplankton carbon is considered. The change in the coefficient of variance is calculated using averages across the first (2006 to 2016) and last (2090 to 2100) decades of the RCP8.5 forcing scenario."

Figure 4 — Minor tick marks not necessary on color scale; difficult to see regions dominated by diazotrophs. Maybe use color palette with more contrast?

Thank you for this suggestion. We have removed unnecessary tick marks from the color scale. Diazotrophs do not dominate in any regions of the global ocean and are not visible on Figure 4 for this reason.



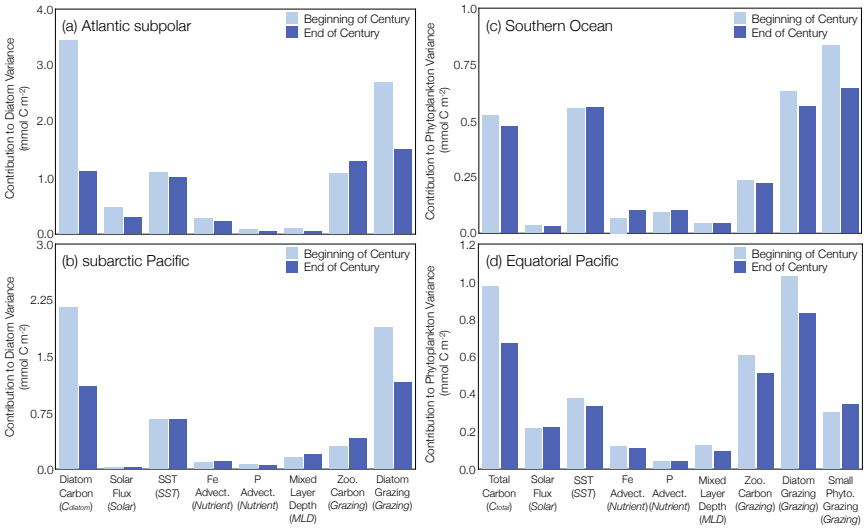
"Figure 4: Distribution of the dominant phytoplankton functional type in biomass carbon averaged across the RCP8.5 forcing scenario (2006 to 2100). The CESM1-LE simulates three phytoplankton functional types: diatoms, diazotrophs, and small phytoplankton. Regions where diatoms dominate

are shown in yellow, regions where diazotrophs dominate are shown in pink, and regions where small phytoplankton dominate are shown in purple."

Figure 5 —Note inconsistent capitalization of biomes in subplots; Are units correctly labeled? Are the units for "contribution to phytoplankton/diatom variance" really mmol C m^{-2}? On a related note, where did the values on the Y axis come from? Based on the axis label they don't correspond to the MLR coefficients, but I didn't see any details in the text.

Thank you for clarifying. We have changed the text in the figure caption to clarify that we show the phytoplankton biomass standard deviation.

We follow the province labels set forth in Tagliabue et al., 2021 in both Table 1 and Figure 5. Proper nouns are capitalized (e.g., Equatorial Pacific, Southern Ocean) while adjectives are lowercase (e.g., Atlantic subpolar, South Pacific subtropical gyre).



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