Comment on egusphere-2022-579

Nicholas Bock (Referee)

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-RC3, 2022

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 in equations 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.

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.

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

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

L220 — 221 - More information on how and why this transformation was performed would be useful.

L219 – 234 - This text feels more appropriate for 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.

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

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.