

Interactive comment on “Mechanisms of northern North Atlantic biomass variability” by Galen A. McKinley et al.

Anonymous Referee #1

Received and published: 6 April 2018

This manuscript explores some of the mechanisms controlling phytoplankton biomass variability in the North Atlantic over the later 20th century and early 2000s. The manuscript is interesting and well written, and to some extent appears to challenge the view that nutrient dependent biomass variability is controlled only by the vertical nutrient supply. However, I have some reservations with the method adopted and cannot therefore recommend immediate publication. My main issue with the manuscript is the approach of using correlation coefficients between biomass and light/phosphate limitation terms as a means of attributing causality. This seems to be something of a shortcut given that these factors are likely to be somewhat collinear with other potential drivers of phytoplankton biomass. If possible, I think a more complete approach would be to recompute the model phytoplankton biomass using the limitation terms and other

C1

drivers (similar to what is done in Laufkötter et al., 2015 for several biogeochemistry models). This would allow the authors to assess the separate impacts of bottom-up processes (the influence of limitation terms on growth rates) as well as top-down loss terms (mortality/grazing). If this approach is not possible due to a lack of model output then I think some of the paper's conclusions should be toned down especially when using these correlation coefficients to infer the processes driving SeaWiFS variability.

My other issue is that a number of processes that could be responsible for some of the trends in biomass variability seem to be neglected. These are perhaps not included in the model but this should nonetheless be stated. What role does temperature play? Is u_{max} independent of temperature? What about zooplankton grazing rates? If grazing is temperature dependent does this explain any of the biomass variability? These sorts of things may be important given that certain models seem to show phytoplankton biomass declines despite increases in phytoplankton growth rates, due to overwhelming increases in losses to zooplankton grazing (Laufkötter et al., 2015).

Specific comments

Ln 90. What was the decision behind the use of the CbPM algorithm? Given that alternative algorithms can substantially differ it would be good to know that the trends described are robust to this algorithm choice. Perhaps a supplementary figure could be produced comparing CbPM mean state and trends in this region with an alternative algorithm such as VGPM.

Ln 107. I think more model details are needed here even if they are published elsewhere. Specifically, what is meant by a “phosphorus-based ecosystem”? It would be good to have some mention of N. Is everything assumed to be Redfield? If so, is this a potential limitation of using this sort of model in this context? Is there any N fixation in the model?

Ln 140-160. See general comments above. Where does temperature limitation fit in? If not at all then I think this should be mentioned. Also, this section focuses on the

C2

effects of limitation terms on growth rates yet the analysis focuses on biomass not growth rates. I think the authors could better describe how growth rates and biomass are related, mentioning the additional processes that affect biomass in their model (e.g. zooplankton grazing?).

Ln 401-404. Although declines in the horizontal nutrient supply may be the proximate driver is the ultimate driver not declines in the vertical supply to the west of the SE box? If so, perhaps this statement should be more nuanced.

Figs 6 and 7. The differences between panels a and b are difficult to see in these plots. It would be useful to add a panel to each of these figures that is the difference between these time slices.

Technical/minor corrections

Ln 25-28. I think some references are needed in this paragraph.

Ln 38. Type error. "...do not fit their..."

Ln 44-48. Within this context it might be useful to mention that Kwiatkowski et al., 2017 related interannual variability of productivity to long-term trends across an ESM ensemble. Capturing productivity variability may therefore help reduce long-term projection uncertainties.

Ln 74-75. "substantial change" reads as if there has been a climatic shift in the North Atlantic subpolar gyre. I think the authors are only referring to variability here and should clarify this.

Ln 101. Space before units for consistency. "2200 m"

Ln 109. I think something should come after "small". Small phytoplankton or nanophytoplankton?

Ln 172. Space before units for consistency. "100 m"

C3

Ln 224. I don't think "all three timeseries" is correct looking at Fig 2a. MODIS does not appear to have any positive anomalies prior to 2004.

Ln 246. Looking at Fig 4 small phytoplankton appear to dominate in the North (> 52°N). Although to a lesser extent than in the South. If correct, this sentence should be amended.

Ln 269. To say "only 40% of total biomass" seems strange.

Ln 309. I would change the word "collaborative".

Ln 361-364. This seems more suitable to the discussion than the results.

Ln 367. Type error. Remove "in" or "since".

Ln 395. Type error. The use of "a" smooth climatological nutrient...

Ln 409. Type error. "...on" the edges of...

Ln 414-415. Suggest improving sentence readability. Perhaps: "...with the dominant mechanisms shifting across timescales"

Ln 418. It is not clear to me what is meant by a "granular approach".

Ln 419. Type error. "...in" this region.

Ln 425. Type error. "...in the" northwest region.

Ln 434. Type error. "of" value or "valuable"

Ln 443. Type error. Remove "in".

References

Laufkötter, C., Vogt, M., Gruber, N., Aita-Noguchi, M., Aumont, O., Bopp, L., et al. (2015). Drivers and uncertainties of future global marine primary production in marine ecosystem models. *Biogeosciences*, 12(23), 6955–6984. <https://doi.org/10.5194/bg-12-6955-2015>

C4

Kwiatkowski, L., Bopp, L., Aumont, O., Ciais, P., Cox, P. M., Laufkötter, C., et al. (2017). Emergent constraints on projections of declining primary production in the tropical oceans. *Nature Climate Change*. <https://doi.org/10.1038/nclimate3265>

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-89>, 2018.