Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-89-AC1, 2018

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

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Received and published: 9 May 2018

RESPONSE to Anonymous Referee #1 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 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.

We address this concern by plotting zooplankton trends in the model over the main analysis period (new Figure S2 and included here). This figure shows that zooplankton trends occur of the same sign as of biomass. Were top-down processes driving the declines (increases) in biomass, then one would expect to see increasing (decreasing) zooplankton trends in the southeast and northeast (northwest). This is not what is occurring in the model. It is clear that nutrient and light trends are responsible for the modeled trends. We include mention of this analysis in the main text, with reference to this new Figure S2.

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 umax 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).

Thank you. We add mention of the temperature dependence of growth in equation 1. Grazing is not temperature dependent and this is also now mentioned. In this model, bottom-up drivers from nutrient and light limitation are responsible for the trends. In the context of zooplankton grazing, we add mention of Laufkötter et al., (2015), also noting that the difference of findings may be related also to the very different timeframes for trends in that paper (∼100 years), as opposed to this analysis (10 years).

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.

Thank you. CbPM is the only algorithm of which we are aware the estimates biomass from satellite. Biomass is the best point of comparison to the model since it is directly carried in the model.

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?

Thank you. We clarify that the primary macronutrient in the model is phosphorus, and that silicate is also limiting to the large phytoplankton class. Iron is a micronutrient. Consistent with other lower-complexity ecosystem models, such as BLING (Gailbraith et al. 2010, Biogeoscience), there is no nitrogen in this 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 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?).

Thank you. As shown above, zooplankton grazing follows biomass changes but does

not drive them.

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.

Thank you. We have modified the text to read "to LOCALLY increased stratification" so as to clarify this point.

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.

We agree that these are somewhat hard to see, but the difference plots are unfortunately not much easier to look at. We have included MLD changes in timeseries form already in Figure 7c-e, and have added the difference plot for barotropic streamfunction in Figure S4.

Technical/minor correctionsâĂÍ

Ln 25-28. I think some references are needed in this paragraph. Thank you, references added.

Ln 38. Type error. ". . .do not fit their. . ." Thank you, this has been fixed.

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. Thank you, we have added this reference.

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. Thank you. It is stated in the following sentence "There is evidence these changes occur in response to changing buoyancy forcing and wind stress, in turn associated with modes of climate variability,..” which clarifies the relationship to variability.

Ln 101. Space before units for consistency. “2200 m” Thank you, this has been fixed.

Ln 109. I think something should come after “small”. Small phytoplankton or nanophytoplankton? Thank you, this has been fixed.

Ln 172. Space before units for consistency. “100 m” Thank you, this has been fixed.

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. Thank you, this has been clarified.

Ln 246. Looking at Fig 4 small phytoplankton appear to dominate in the North (> 52âŮęN). Although to a lesser extent than in the South. If correct, this sentence should be amended. Thank you, we have amended this sentence to read “On the mean, in the open waters of the North Atlantic, large phytoplankton have a greater contribution to the total biomass in the north and west (Fig 4a), but small phytoplankton are dominant to biomass throughout the basin and particularly in the south and east (Fig 4b).”

Ln 269. To say “only 40% of total biomass” seems strange. We have modified this to state “a smaller portion (40%)”.

Ln 309. I would change the word “collaborative”. Thank you. We feel the term is useful and elect to keep it.

Ln 361-364. This seems more suitable to the discussion than the results. Thank you. We find this a good lead in to the next line where the Discussion begins.

Ln 367. Type error. Remove “in” or “since”. Thank you, fixed.

Ln 395. Type error. The use of “a” smooth climatological nutrient. . . Thank you, fixed.

Ln 409. Type error. . . .“on” the edges of. . . Thank you, fixed.

Ln 414-415. Suggest improving sentence readability. Perhaps: . . .“with the dominant mechanisms shifting across timescales” Thank you, fixed as suggested.

Ln 418. It is not clear to me what is meant by a “granular approach”. Thank you, we have clarified to state “smaller subregions”.

Ln 419. Type error. . . .“in” this region.âĂİ Thank you, fixed.

Ln 425. Type error. . . .“in the” northwest region.âĂİ Thank you, fixed.

Ln 434. Type error. “of” value or “valuable” Thank you, fixed.

Ln 443. Type error. Remove “in”. Thank you, fixed.

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 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

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Figure S2

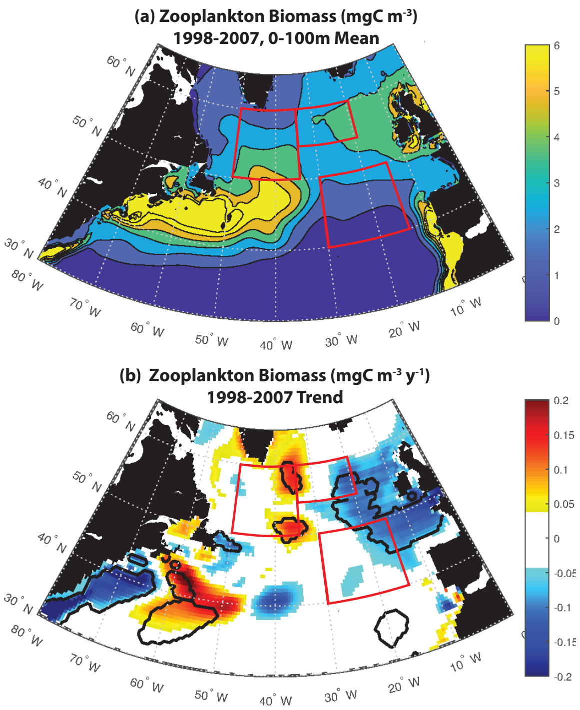

Fig. 1. new figure S2

Figure S4

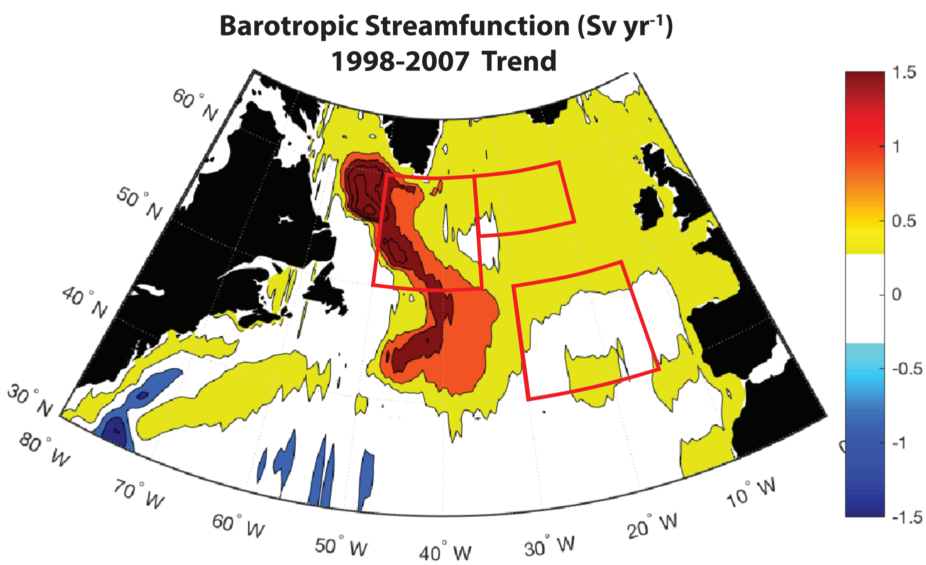

Fig. 2. new figure S4