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

**Anonymous Referee #2**

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Review of "Mechanisms of northern North Atlantic biomass variability" Galen A. McKinley, Alexis L. Ritzer, and Nicole S. Lovenduski Biogeosciences, submitted.

Summary The manuscript integrates satellite ocean color observations and a coupled ocean physical-biogeochemical model to quantify plankton trends in the temperate and subpolar North Atlantic and evaluate potential underlying mechanisms. The model is an essential component for identifying physical transport effects on plankton dynamics. The study builds on a substantial literature on this important scientific question.

Overall, I found the manuscript to be relatively weak. The modeling study focuses on linear trends that are likely not robust over such a small time window (1998-2007) for SeaWiFS data. The trend analysis leaves out the longer integrated SeaWiFS-MODIS data set, which exhibits substantial interannual to decadal variability, often with different



temporal patterns than inferred from the shorter linear trend analysis. The numerical modelling is also limited to 2009 and thus is not compared with later MODIS data.

ocean and is a significant contribution that fits well within the scope of Biogeosciences journal. The manuscript is generally well written.

Methodology The manuscript utilizes well-documented satellite ocean color data from SeaWiFS and MODIS-Aqua and an established ocean physical-biogeochemical model and hindcasting techniques. The study utilizes a set of monthly diagnostics for the physical and biological terms affecting the phosphate budget. The biological diagnostics are available for the SeaWiFS period (1998-onward) but not prior to 1998 (model output lost; Line 135). This subtracts some from the trend analysis over longer time periods 1949-2009 where only model biomass is available. Also, it is not clear why the model hindcast stops in 2009, now more than 8 years ago.

Results One limitation with the analysis is the focus on linear trends over a relatively short analysis window (1998-2007) (Figure 1). As is clearly shown by Figure 2, the regional temporal patterns primarily exhibit inter-annual to decadal trends and any linear trend is relatively small and sensitive to the choice of time window (an issue that the lead author is well familiar with and has published on previously). For example, the manuscript identifies declining biomass east of 30-35 deg. W (e.g. Line 11-12; Line 201-202), however, this is not consistent with the observations. The SeaWiFS-era trends in (Figure 1c) show only a small region of declining trends in the northeast (north of 55 deg.) with a substantial region of positive trends (though admittedly not statistically significant). The regional trends in Figure 2 that include MODIS data do not show such clear trends, and in fact from the merged SeaWiFS-MODIS data the trend in the southeast actually change signs.

The manuscript would be much stronger if the focus was expanded to include the agreement (or disagreement) of model and observed interannual variability and underlying mechanisms. At a minimum, there needs to be more up front discussion of the rationale for and limitations of focusing on linear trends.

The singular focus on annual means in the model and data analysis neglects the substantial seasonality in bloom dynamics in the subpolar North Atlantic. This raises two issues. First, there is no discussion of the robustness of annual mean satellite observations because of sampling biases, particularly during winter. Second, it is not clear if annual mean biomass is the biologically most important indicator; would more relevant indicators be peak surface biomass concentration or peak integrated biomass (ala arguments of Berhenfeld and Boss). Implicitly the analysis also assumes that only bottom-up factors (light and nutrient limitation) influence trends in phytoplankton biomass, neglecting possible top-down factors. This may be true for the model, but perhaps is an incomplete picture of the actual ocean.

Specific comments Line 180: I have some concerns regarding the following paragraph: “This analysis is based on annual mean fields. A 3-month lag of the biology diagnostics and biomass fields after physical diagnostics and other physical fields is employed to account for the maximum physical forcing occurring in the winter prior to the spring bloom. Thus, annual mean physical fields are averaged from October of the prior year to September of the year in question. Biological fields are January to December averages.” I understand the need perhaps to adjust the year window to capture the relevant Fall and early winter pre-conditioning of subsequent spring bloom, but the text is not framed in terms of pre-conditioning. Rather a somewhat arbitrary 3-month lag is argued, inconsistent with the well-observed latitudinal seasonal patterns in the timing or phenology of bloom dynamics for the North Atlantic. Further, it is not clear that the relevant physical quantities are annual means for variable such as mixed layer depth with large seasonal variation and where it is more likely that the maximum winter mixed layer depth is more biologically relevant. Given the richness of monthly model output, a more nuanced data analysis would be warranted.

Line 209-212 “In both observations and models, the magnitudes of these changes are large in comparison to the mean. In the declining regions, where mean biomass is 15-



25 mgC m-3 (Fig. 1, S1), trends -0.5 to -1.5 mgC m-3 yr-1 over 10 years imply biomass reductions of 30-50 %. To the west of 30-35 deg. W, increases of a comparable percentage are implied.” The magnitude of these trends may be appropriate for some pixel level trends but the magnitude of the trends are roughly an order of magnitude smaller at the regional scale in Figure 2.

Figures 1 and 2; Lines 213-219: Regional analysis boxes are identified for the model in Figure 1d and linked to the time-series in Figure 2. It appears from the text that the same regional boxes are used for model and satellite observations, but it is unclear if this is appropriate given the spatial mismatch in the model and observed mean and trend patterns. The text (Lines 226-229) argue that this has a minimal effect but this should probably be shown in some figures in the supplement.

Line 237-239: There is considerable nutrient data (though still sparse) from the CLIVAR Repeat Hydrography for the sub-polar North Atlantic, particularly from the German and Canadian occupied lines; worth looking in to.

Line 250: Are the annual means for phosphate and light limitation terms simply the straight means? Was there any consideration of weighting the limitation with seasonal variations in biomass or NPP, which might enhance the biological relevance.

Line 285: Does the model mixed layer trend agree with observations?

Line 296-299: The wording of the text here is awkward; would be phrased as, for example, “horizontal advective divergence” (or convergence), etc. As written, the text and figure labels (Figure 8 and 9) confuse “flux” with “flux divergence”.

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