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

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RESPONSE to Anonymous Referee #2

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.

Thank you.

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.

Thank you for your comments that have helped us to strengthen the manuscript. We describe below in more detail why we focus on linear trends. We do compare to MODIS data in Figure 2.

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

Thank you for this positive assessment.

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.

The reasons for the hindcast ending are found in the original manuscript on Lines 123-125. While, this is unfortunate, the model does cover the prime viewing period of SeaWiFS whose trends we aim to explain and thus it is a useful tool for the desired purpose.

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.

We sense that the reviewer is concerned that our focus on trends implies a focus on long-term trend, perhaps even the suggestion of climate-change driven trends. This is most definitely not what we imply. These are trends over a specific period, 1998-2007, as observed by SeaWiFS – but they are in the context of interannual variability, as highlighted explicitly in the manuscript by the timeseries correlation analysis for the full model experiment (1949-2009). In the southeast (Figure 2a), the MODIS does indeed change sign, but this occurs after 2010 which is beyond our prime analysis period, and thus there is no inconsistency between the model, SeaWiFS and MODIS for the 1998-2007 period as suggested by this reviewer comment.

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.

We show clearly with the figure several figures that the model does agree well with the satellite-observed biomass trends, and also reference previous manuscripts that have shown model fidelity against other datasets. Further, the analysis presented in the manuscript is precisely of the mechanisms driving these interannual changes, as asked for by the reviewer with this comment. Clearly, there is a need to clarify for the reader. Thus, we add text to the end of introduction that clarifies that this is a mechanistic analysis of the drivers of a particular set of SeaWiFS-observed changes in biomass – which are best quantified as linear trends given the 10 year period available to us. This mechanistic analysis is combined with an effort to understand the degree to which these drivers are responsible for variability across the full model experiment. We also contrast this analysis to others that could be done based on the primary modes of variability across many decades, such as using an Empirical Orthogonal Function (EOFs) – work of the type that this author team has published extensively. The negative of EOF-type analysis is that at best, it tends to explain at most 30% of the large-scale variance over timeframes longer than most datasets – thus EOFs do not fully explain the observations. This paper is a case study of a particular period in which we are able to fully explain the drivers of the observed changes as estimated using a reasonable modeling tool (Figure 9,10).

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.

We agree that alternative choices could have been made in the presentation of the biomass – peak vs. annual mean, for example. What is actually critical is that we are consistent between treatment of the observations and of the model, as we are. In response also to Reviewer 1, we add a figure of zooplankton biomass trends to the supplementary. This figure shows that top-down drivers are not driving the changes in this model. As discussed, this certainly does not rule out top-down drivers being important in the real ocean, but they are not required to capture the observed changes in phytoplankton biomass.

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.

We add mention that a lag of 2 or 4 months does not substantially change results. Results are also similar with 0 lag or 1 month lag, but correlations are weaker. Given that our focus is on the northern North Atlantic, north of 40N, where deep mixing precedes the bloom by several months, some temporal lag is reasonable when annual means are being considered. Again, the use of annual means is a choice that must be made early in the analysis. As stated above, what is critical is that we aim to explain annual mean changes in SeaWiFS observed biomass, and that we do so with annual mean changes in the model. We agree that an analysis of also of monthly fields could be interesting, it is beyond the scope of the work already presented here. We add this suggestion in the last paragraph of the Discussion.

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.

Thank you for noting the need for clarification. We now include region-mean percent changes in the text: for model (seawifs): -17% (-19%) in SE, -10% (-15%) in NE, +9% (6%) in NW. The changes are based on the same model output and data shown in figures 2 and S1.

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.

Thank you for suggesting we take another look at this. The differences are very minor, and thus additional supplemental figures would only be confusing. For example, this comparison of the NE box with MODIS and SeaWiFS boxes shifted to the north and east by 5degrees is essentially indistinguishable from Figure 2b without the shift.

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.

Thank you – we look forward to seeing what the experts in analysis of these data find with respect to temporal changes in large-scale nutrient fields between the WOCE and CLIVAR eras given the spatial and temporal heterogeneity of these data. To perform this data analysis ourselves is clearly beyond the scope of this work.

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.

As stated, the analysis is based on annual averages throughout for consistency. These limitations terms are very relevant to the biology and are the primary mechanism explored, so there is no need to "enhance the biological relevance" as suggested. Any weighting undertaken would also require additional analysis choices and thus a simple annual mean for all fields is the most straightforward approach when our goal is to explain annual mean SeaWiFS biomass changes.

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

Yes it does, we add reference to Vage et al. 2008. Thank you.

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".

Thank you, we have clarified the text as "flux convergence" and "flux divergence" throughout.

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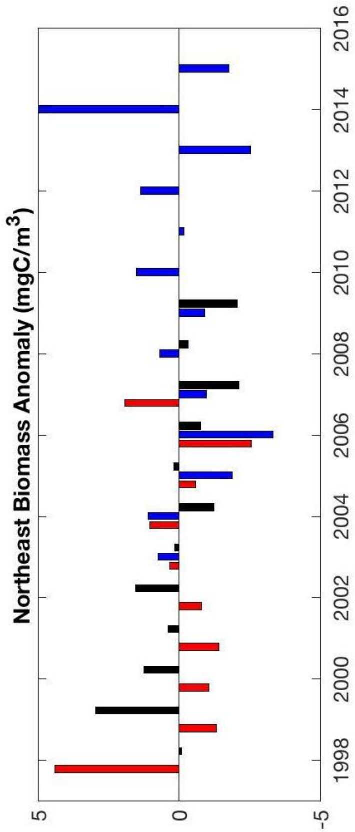

Fig. 1. comparison figure for timeseries NE with shift