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Biogeosciences
Discussions

Interactive comment on "Phytoplankton biomass, composition, and productivity along a temperature and stratification gradient in the Northeast Atlantic Ocean" *by* W. H. van de Poll et al.

Anonymous Referee #1

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The present manuscript is a very nice work addressing the seasonal variability of phytoplankton biomass, productivity and community composition along a latitudinal gradient in the Atlantic Ocean, covering from the low-latitudes to the highest. The relationship between these changes and the nutrient availability, water-column stratification and the sea surface temperature is also analyzed. Biomass was estimated from Chl a concentrations and phytoplankton taxonomic composition was determined by HPLC analysis. Phytoplankton productivity in different groups was estimated using a diagnostic bio-optical model. As a result, a comprehensive study of the effect of the environmental forcing is presented, with significant outcomes related to the effect of future climate-driven changes in stratification and SST in the organization and function of

marine phytoplankton communities. The introduction, methods, results and discussion sections are very well organized and addressed, so they are the figures and tables presented.

Below are some comments that, from my point of view, will help to improve this manuscript.

General comments:

1) Introduction. Although the introduction is easy to follow, I miss the inclusion in the text of the hypothesis that motivated this study and the results that the authors were expecting to get. I mean, why do the authors decide to carry out this analysis? What is the difference between this study and the rest of previous works carried out in this field? Addressing these questions at the end of the Introduction section may improve it.

2) Do the authors have any explanation to the fact that no correlation was found between N, P and stratification index in the stratified stations? I would have thought that a higher stratification would imply low nutrient concentrations so I was expecting an inverse significant relationship.

3) Other result that surprises me is the inverse correlation found between diatoms and N, P and the positive with SST for stratified stations. Considering the advantages of diatoms in highly dynamic and nutrient rich ecosystems in terms of nutrient storage and nutrient uptake I would have expected an inverse pattern that the observed (inverse correlation with temperature, and positive correlation with nutrients). Do the authors have any explanation for this?

Specific comments:

1) Page 1795. Line 5. Please clarify at which absorption the authors are referring.

2) Page 1796, line 1. Please include a reference at the end of "...phytoplankton growth"

3) Page 1796, line 7. Please include a reference at the end of "...nutrient concentrations"

4) Page 1799. Lines 2-5. The authors distinguished the oligotrophic stations because their nitrate concentrations in the euphotic zone where below the detection limit. However, nitrate concentrations for the euphotic layer are represented in figure 2 for the oligotrophic stations...Thus, how did the authors measure this if the concentrations were below the detection limit?

5)Page 1803. Line 3. Why do the authors assume that phytoplankton was low acclimated when surface Chl a exceeded 0.5 mg m-3?

6) Page 1804-1805. Please include the standard errors for the N:P ratios described.

7) Page 1805. Section 3.3. Please include the physiological/ecological meaning of the Chl a specific absorption measured here. Is it considered here as a proxy for the light availability?

8) Page 1806. Lines 4-7. I am not sure if there is an errata in table II related to the correlation of surface Chl a and Chl a 0-50m with SST, N, P and stratification index. In lines 5-8 the authors says there is a inverse correlation with STT and a positive correlation with N, P0-50m, whereas this was not found for stratification. However, as I see in table II the correlations coefficients for this latter variable were significant (-0.62 and -0.60 for Chl a and Chl a 0-50m respectively). Please clarify.

9) Page 1807. Line 24. I think that the authors should add the term PP when enumerating the biological variables that correlate with N and P, if they want to state that nutrient availability controlled phytoplankton biomass and productivity.

10) Page 1808. Lines 6 to 10. These statements result confusing.

11) Page 1809. Line 9. There are other studies carried out in the Atlantic Ocean that have focused on changes in the size structure of phytoplankton community depending on the environmental conditions, such as Maranon et al. 2001. MEPS. 216: 43-56.

12) Page 18010. Lines 10-11. I would say that pre-bloom conditions were MORE RICH in terms of depth integrated Chl, instead of "more productive". The use of the term productive suggests that the authors are talking about primary production.

13) Page 1811. First paragraph. This paragraph is not very clear. Are the authors explaining why the diatoms didn't show any correlation with SST, N and P in stratified stations?

14) Page 1812. Lines 20-24. I think that in these statements the authors are saying two opposite things. First, they say that the model assumes that nutrient availability yields differences in phytoplankton biomass (that is chl a here) and production. And next, the authors say that this would be supported by the report about not influence on Chl a specific net PP by nutrient availability. But, I cannot see how the study of Hasley et al. is supporting that assumption in the model...In my opinion they are saying the contrary.

Interactive comment on Biogeosciences Discuss., 10, 1793, 2013.