Final author comments: **Phytoplankton biomass, composition, and productivity along a temperature and stratification gradient in the Northeast Atlantic Ocean**

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The manuscript will undergo a major revision based on the valuable comments of the reviewers. We thank the reviewers for their work, which has improved the manuscript. In short, a supplement will be provided which states model equations, and group specific P vs E parameters. Furthermore, the supplement provides data on microscopy samples that were analyzed in concert with CHEMTAX analysis. Furthermore, pigment ratios used for CHEMTAX are provided in the supplement.

In addition changes were made to the photoacclimation assumptions for the productivity model. For the previous calculations low light acclimation was assumed at Chl-a concentrations in excess of 0.5 mg m^{-3}. This has been reconsidered and changed. The changes are supported by onboard experiments that investigated the photoacclimation state of the phytoplankton, using recovery of photosynthetic efficiency from excess light exposure as a measure for photoacclimation state. These experiments will be included in the revised ms. Due to these changes, productivity values higher latitudes in summer were higher (18%), whereas values for oligotrophic stations in spring were lower (26%). Graphs and correlations have changed accordingly. The changes had minor effects on the overall conclusions drawn from this work.

Please note that one co-author was added to the manuscript (P.D. Rozema), due to his involvement with the photoacclimation experiments.

Furthermore, hypotheses were included in the introduction.

Detailed enquiries are answered below.

Response to referee #1

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.

Hypothesis will be included in the introduction in ln 19: We hypothesized that SST influences phytoplankton biomass and composition by affecting nutrient concentrations in the upper open ocean. Therefore, relationships between SST and nutrient concentrations can be expected along existing temperature gradients. Furthermore, relationships between SST, phytoplankton biomass, composition and productivity can be expected along existing temperature gradients in the open ocean. Recent studies on temperature and stratification relationships with phytoplankton biomass and productivity have focused on the oligotrophic open ocean, where nutrient limitation of phytoplankton is a dominant feature (Behrenfeld et al., 2006; Polovina et al., 2008, Dave and Lozier, Lozier et al., 2011). In this context, temperate and higher latitude regions have received less attention and studies that include both oligotrophic and higher latitudes waters on this topic are currently lacking.

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.
It appeared that nutrient concentrations were to some extent uncoupled from the density differences in the upper 200 m in spring, in contrast to summer. Relationships were stronger with SST. This was probably due to the fact that the SST gradient remained intact in spring, whereas stratification was very weak. This will be included in the discussion p1808 ln 6. The relationships were stronger with SST than with the stratification index. In spring nutrient concentrations were most uncoupled from stratification, i.e. did not reflect the differences in density.

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?

Diatom abundance was low in most stations, significant correlations with diatom productivity were not observed for stratified stations. Diatom productivity was high in some non-startified stations, and showed significant correlations with SST (+) and N, P concentration (-). Our interpretation of these results was that temperatures below 8°C limited diatom productivity. This was stated in the discussion. In addition, diatom abundance may be influenced by silicic acid, but this was not measured in detail during the cruise.

1795 ln 5: **Chl-a specific absorption**

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? **Nitrate and phosphate concentrations for the upper euphotic zone (0-50m) were undetectable (detection limit phosphate 0.01 µmol/l, detection limit nitrate 0.03 µmol/l). The concentrations shown in figure 2 are from the upper and lower euphotic zone. Concentrations in the upper euphotic zone (> 50 m) were below the detection limits, whereas concentrations in the lower euphotic zone (50-125 m) were detectable for oligotrophic stations. The detection limit for nitrate and phosphate will be included in the method section.**

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?1803 ln 3. **This part has been reconsidered and this criterion was removed. After evaluation of our measurements the following photoacclimation considerations were included. For the summer, we used data for high light acclimated species up to the irradiance dose of 5.4 mol m^{-2} day $^{-1}$ (corresponding to**

the cultures that were grown at 125 µmol photons m^{-2} s^{-1}). For depths experiencing lower light doses, phytoplankton was assumed low light acclimated. For spring, we assumed low light acclimated phytoplankton for all depths. We used onboard experiments during which recovery of photosynthetic efficiency (Fv/Fm) of excess light exposed phytoplankton was monitored and used these results as indicator for photoacclimation state. These experiments showed that phytoplankton recovery after excess light exposure was significantly lower in spring compared to summer. Furthermore, differences between samples from the deep chlorophyll maximum and subsurface were negligible in spring, whereas they were clearly visible in summer. These experiments will be included in the method and results and will be presented in a graph.

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

N:P spring oligotrophic 0-50m: 9.6(±3.6) 50-125m: 14.0(3.7)

N:P spring mesotrophic 0-50m: 15.3(±0.9) 50-125m: 15.9(0.6)

N:P summer oligotrophic 0-50m: 12.0(±6.2) 50-125m: 16.5(0.9)

N:P summer mesotrophic 0-50m: 13.2(±1.4) 50-125m: 16.1(0.3)

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?

1805: We used the Chl-a specific absorption to differentiate between high light acclimated phytoplankton in the surface layer and low light acclimated cells at depth for CHEMTAX. When Chl-a specific absorption was not different, no differences in photoacclimation were assumed for CHEMTAX. This will be included.

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.
1806 ln 4-7: When tested for stratified stations (spring and summer combined), surface Chl *a* and Chl *a*$_{0–50m}$ showed significant inverse correlations with SST and a positive correlation with N, P$_{0–125m}$, whereas this was weaker for the stratification index (Table 2).

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.**1807 ln 24: The positive correlation between N, P$_{0–125m}$ and Chl *a,* PP suggested that open ocean phytoplankton biomass and productivity were controlled by the availability of these nutrients in the investigated region.**

10) Page 1808. Lines 6 to 10. These statements result confusing.1808 6-10 **Rephrased: Moreover, the fraction of the phytoplankton biomass below the euphotic showed a negative correlation with SST.**

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.**1809 ln 9: Reference was included,** Maranon et al. 2001. MEPS. 216: 43-56.