

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 #3

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Interactive comment on "Phytoplankton biomass, composition, and productivity along a temperature and stratification gradient in the Northeast Atlantic Ocean" Authors: WH van de Poll et al. Manuscript number: bgd-10-1793-2013

General Comments The manuscript by Poll et al. describes the chlorophyll-biomass and primary productivity of phytoplankton taxa on a latitudinal transect from ~30°N to >60°N (i.e. from subtropical to subpolar waters). The authors use a pigment-analysis programme (CHEMTAX) to proportion chlorophyll between major phytoplankton groups and then combine these estimates with a bio-optical model to estimate group-specific primary productivity. Currently, the study lacks any real focus in terms of identifiable

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goals or testable hypotheses with which to judge if the paper represents a 'substantial contribution to scientific progress in terms of new concepts, ideas, methods or data'. There are statements in the discussion and conclusions that hint at the proposed focus for the study, e.g., the influence of temperature changes on phytoplankton community composition and productivity. The results and conclusions, and discussion, could also do with some level of hypotheses to add structure and order.

The use of CHEMTAX to assign chlorophyll biomass between phytoplankton groups is well used in the literature, and there are recognised limitations to this approach which require careful consideration and ground-truthing. The accompanying study by Mojica et al. (submitted to L&O and unavailable for review) potentially holds such information but currently it is not clear whether this paper supports the appropriation of biomass between phytoplankton groups. Neither is it clear whether these two papers contain the same data/information or conclusions. Furthermore, the bio-optical model used has numerous assumptions and although these are discussed in the methods, it is unclear how these assumptions influence the results/conclusions and if different assumptions were included, what the conclusions would be.

Specific Comments - the abstract lacks a statement of the purpose of the study. - the introduction is well written but reads more as a literature review than as a well structured set up for the study. Clear goals and hypotheses need adding to clarify these issues. - first line of the introduction: phytoplankton growth ultimately depends on the availability of nutrients and light (and mortality factors) on daily timescales rather than seasonal/interannual timescales. - pg 7, ln 4: oligotrophic waters are defined as below the detection limits, but what are the detection limits? - pg 8, ln 15: why choose 0.1% as depth of the euphotic zone rather than 1% as often used in other studies? - pg 8, ln 23: Were the CHEMTAX results ground-truthed in any way? Difficult to access this without access to Mojica et al. (submitted). This is key to the manuscript and modelling of the group specific production and so the lack of information makes it impossible to access the validity of the pigment or model data. What were the high light and low light

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acclimated initial pigment ratios (pg 9, ln 3)? - pg 8, ln 23: Why were samples grouped by latitude, and in what way were they? - pg 9, ln 12: What are the further details on phytoplankton species composition in Mojica et al.? - pg 9, ln 26: How is satellite derived irradiance data in situ? - pg 10, ln 8: How valid is a linear relationship between carbon fixation and temperature? How is the slope of a growth versus temperature experiment in units of $\text{mg C m}^{-2} \text{d}^{-1}$? - pg 10, ln 11: Is Chl-a a valid measure of phytoplankton biomass? - pg 10, ln 19: PVE parameters from nutrient replete cultures, growing under optimum irradiance conditions, do not seem appropriate for use with field samples? What was the light:dark cycle of these cultures? What were their daily photon fluxes and how did they correspond to the in situ conditions? - pg 11, ln 2: What is the basis for assuming that where Chl exceeds 0.5 mg m^{-3} , the phytoplankton community was low light adapted? Reference? How does this influence the results? - pg 12, ln 7: The correlation between SST and stratification is reported, with correlation coefficients, but not p values are reported. Where these statistically significant? - pg 14, ln 24: Only 30% of productivity from cyanobacteria: how does this compare with other studies? How do the contributions in spring and summer compare with similar studies? - pg 21, ln 2: Is this the aim of the study - "Overall, this study showed that the model approach can expand the use of phytoplankton pigments and provided useful insight in group specific productivity"? - Tables 1-4: bold values are 'significant' at what level?

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