Biogeosciences Discuss., 10, C598–C603, 2013
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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 #2

Received and published: 28 March 2013

General Comments. The manuscript presents spring and summer observations of temperature, stratification, Chl-a, inorganic nutrients and primary production (PP) for a transect in the Eastern North Atlantic. PP is quantification for five phytoplankton groups and statistical techniques are then used to assess the correlations between Chla, PP and environmental variables. The paper addresses scientific questions relevant to Biogeosciences and the text, figures and tables are well presented. However, I have some significant concerns regarding the validation of the PP model, which I outline below. I would also urge the authors to highlight the novel aspects of the work. Hopefully my concerns can be relatively easily addressed. I have also provided some specific comments, which I hope will be helpful in a revision.

Specific Comments.

Model validation: I feel that more information is needed in order for the reader to assess the suitability and accuracy of the model.

As is noted by the authors in the discussion, the primary production model is not validated: P1812 L28: “Validation of the productivity calculations with field productivity estimates was not possible in the present study. Therefore the current productivity estimates should be viewed as potential productivity estimates, rather than actual measurements”. This, to me, seems a major problem. The inability to validate a model does not justify using an un-validated one. If validation against directly comparable PP (or P vs E) measurements is not possible, then I suggest that a more comprehensive comparison to previous PP measurements is necessary (i.e. extend section 4.3). Are there additional published datasets to include here? What different methods were used? What are the reasons for any differences?

The lack of validation is particularly worrying because, to my mind, there are two key assumptions that could lead to substantial errors. 1) The model assumes community structure obtained using CHEMTAX, and 2) the model assumes photosynthesis versus irradiance responses for species grown in culture as representative of the in situ community (and assumes the parameters are fixed for each group, although high and low light parameters are assigned in stratified waters).

1) Using CHEMTAX to obtain community structure can be problematic, particularly when applied over large special scales as in the case in this study. Presumably, details are in Mojica et al. (submitted), but I feel more information is needed here as well because the model, and much of the interpretation, is highly dependent on it. In particular, please state how well CHEMTAX performed against validation and ground truthing. PP estimates presented in this manuscript are entirely dependent on results in Mojica et al. (submitted), as such, they cannot fully be evaluated until Mojica et al.is published.

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2) Please provide more details on the P vs E culture data, and justify their use in the PP model. For example, what were the culture conditions? Are the cultured phytoplankton suitable representatives of the population in the study region? What are the implications of assuming fixed P vs E parameters for each group? Could the culture conditions bias the modeled PP contribution of different groups in any way? Culture details are presumably in Kulk et al. 2012, but it would be useful to include more detail here because they are a crucial component of the model. Is it possible to validate, for example, the modeled bulk community P vs E curves against curves measured during this or previous studies in the region? Halsey et al. 2011 is cited to justify assuming no affect of nutrient limitation on the P vs E parameters for each group. Should this reference be Halsey et al. 2010 Photosynth Res 103:125-137? From what I understand, whether or not P vs E curve parameters vary as a result of nutrient availability depends on the method used to obtain the P vs E curve. In particular, the timescale over which the experiments were conducted and whether or not they quantify net or gross (and C or Chl –specific) PP. Please include sufficient information on the P vs E data to reassure the reader that this assumption is appropriate for the current study.

Other specific comments: I would urge the authors to clearly highlight the novel aspects of the work. The main conclusions (Section 5) focus on the statistical relationships between SST, nutrient, Chl-a and PP, but it is not entirely clear how these relationships build on current understanding? It would also be helpful to specify what is learnt from the (novel) group-specific PP estimates?

Throughout the manuscript, chlorophyll-a concentration is assumed to represent phytoplankton biomass. I don't feel this assumption is appropriate or necessary for the current study. The factors that decouple chl-a from biomass (incl. temperature, nutrients and light availability, community composition) are explicitly dealt with in the manuscript. I suggest simply referring to Chl-a concentration throughout i.e. simply change the term "biomass" to "chl-a". (e.g. see Perez et al. 2006 DSR-I 53:1616-1634 for the difference between Chl-a and carbon biomass in the oligotrophic N. Atlantic).

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Title: “Phytoplankton biomass, composition and productivity along a temperature and stratification gradient in the Northeastern Atlantic Ocean”. This is a little misleading because no biomass or species composition data are presented in the paper. I suggest changing the title to something like: “Primary production of phytoplankton groups along a temperature and stratification gradient in the Northeastern Atlantic Ocean”.

Section 2.6.2. It would be helpful to include specific details on the primary production model, including key model equations.

P1795 L25: “Phytoplankton growth in the oceans depends on seasonal and inter-annual climatological cycles that determines the availability of nutrients and light.” Also mention top-down (grazer) controls.

P1798 L23: “potential (1-125m)…” What is meant by “potential” euphotic zone? Do you mean “entire”?

P1800 L15: “Depth integrated chl-a was then calculated for the euphotic zone and for defined depth intervals…. total depth-integrated Chl-a (surface to 200-410m)” It would be helpful to state what determines the depth interval for each location (is Chl-a negligible at these depths?) in order to reassure the reader that the variability in the integration depth does not influence the patterns shown in Fig 4.

P1799 L4 “We defined oligotrophic stations as those stations where NO3 in the upper euphotic zone was below the detection limit” Please quote the detection limit.

P1799 L18: “The spectrally weighted mean specific absorption coefficient (a) was calculated as the sum of a*ph between 400-700 nm, and corrected by a normalized solar spectrum (maximum set to one). “ Does this mean that the change in light spectrum with depth was not accounted for in the spectral correction of a*ph? If so, please make this clear and acknowledge any potential errors resulting from this assumption. To reduce these errors, you could consider obtaining underwater light spectra from measured Chl-a profiles using generic Kd(λ) vs. Chl-a relationships (e.g. Morel and

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Antoine (1994) Journal of Physical Oceanography 24: 1652-1665), i.e. assume the solar spectrum at the surface and calculate the change in spectrum with depth. The relationships are based on global data but could be better than assuming a surface light spectrum at all depths.

P1801 L10: “The current study focused on five phytoplankton groups used in the primary production model”. To make a clearer distinction between the groups identified by CHEMTAX and the groups used in the primary production model, consider changing to: “In the current study, five of the eight identified phytoplankton groups were resolved in the primary production model”. Also, does this mean primary production is likely to be underestimated, because not all groups are considered in the primary production model? If so, please give some indication of the magnitude of the underestimation.

P1801 L25: “The daily light dose at each station was obtained using data (level 3, 9 d average) from the . . .. MODIS satellite”. Please specify the name of the data product.

P1808 L13-L25: “The inverse relationships between SST and near surface phytoplankton biomass and PP0-50m for stratified stations suggests that within the SST range of 13-23oC, North Atlantic open ocean productivity can co-vary with seasonal, inter annual and multi-decadal SST changes. This also implies that anthropogenic warming of the ocean has a negative influence on phytoplankton biomass and productivity in the stratified open ocean within this temperature range. . . .. etc” Take care when using correlations measured along a transect to predict future changes in response to long-term or climate warming. Statistical correlations to SST do not indicate causation and the processes at work are complex, varying in space and time. I suggest either removing these kinds of assertions or substantiating them with due consideration of the relevant processes (including the extensive knowledge of these processes in the literature).

To me, the novelty of the work lies in the group-specific PP estimates. I was left with many questions about the variability in group-specific PP - e.g. Does the contribution to PP of the different groups simply reflect their contribution to total Chl-a? How much does the group-specific photo-physiology matter? To my mind, significant insight could be gained by including some information on community structure (from Mojita et al. submitted) and /or the photo-physiology of the different groups (from in Kulk et al. 2012). E.g. Adding panels showing Group "% of Chl-a" to Fig 7 would be very informative.

Technical corrections:

Define abbreviations on first use. E.g. P1798 L6: "CTD" P1798 L19: "NOX" P1799 L16: "HPLC"

After defining an abbreviation, only use abbreviated terms. E.g. P1799 L14, change "Chlorophyll" to "Chl a".

Check consistency of abbreviations used. E.g. P1804 L12-14: Check use of abbreviation N and P – should they be NO3 and PO4? Note that the abbreviation "N" is elsewhere used to mean "North". Also change N to NO3 in Table 1 and caption.

P1806 L25: Should "Fig 7" be Fig 5c?

P1805 L13: "Oligotrophic stations showed low surface Chl a, whereas higher concentrations were found in the deep chlorophyll maximum". This is implicit in the term "deep chlorophyll maximum", so is this sentence necessary?

Figure 2. Should the figure names be (A,B), (C,D,) and (E,F) instead of (A), (B), (C)?

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