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Biogeosciences Discussions

Interactive comment on "Bio-optical provinces in the eastern Atlantic Ocean and their biogeographical relevance" *by* B. B. Taylor et al.

Anonymous Referee #2

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Taylor et al. discuss results from an interdisciplinary cruise aboard the R/V Polarstern, assessing phytoplankton assemblages, the associated optical properties of the water and their link with biogeochemical provinces in the eastern Atlantic Ocean.

This manuscript is interdisciplinary, linking physics, optics, phytoplankton community structure and using this information along with multivariate statistics to map bio-optical / biogeochemical provinces. The data analysis is interesting, and it addresses a relevant scientific question within the scope of BG and to a broad cross-section of the oceanography community. Due to the interdisciplinary nature of the paper, my review focuses on aspects of the study I am familiar with. Overall, I enjoyed reading this paper and I think that it is well written and should be accepted for publication in Biogeosciences, subject to some moderate corrections detailed below. While I believe this

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manuscript will make a substantial contribution, below are some general and specific questions (or suggestions) that should be addressed before publication.

General comments

Whereas the analysis and information (optical and biological) used for each sample was extensive, there was only 48 stations. It surprises me that there was not more comparison with AMT transect programme, considering it has transected the Atlantic over the past decade, including the Eastern Atlantic (e.g. see AMT 6 and 15). The pigment results shown in figure 3 are supported by a number of studies using AMT data (see especially Aiken et al. 2009), which is only briefly touched on (e.g. page 7179, line 14-16). More discussion on how the results presented in this paper compare with the AMT programme is likely to reinforce the conclusions of the manuscript.

Furthermore, the relationships between phytoplankton functional groups, bio-optical traits and biogeochemical provinces has been explored recently by Aiken et al. (2008), albeit less quantitatively, and again the results presented in this paper are in support of the conclusions reached by Aiken et al. (2008), using a number of globally representative datasets. A discussion on this is likely to reinforce the manuscripts conclusions.

Page 7174-7175, section 2.3.1 Interpretation of pigment data; and Page 7179-7180, section 3.2 Phytoplankton composition: If find it encouraging to observe similarities between the CHEMTAX and size-based pigment analysis, as highlighted in Figure 3. While the method of Vidussi et al. (2001), as refined by Uitz et al. (2006), has been used extensively in the literature, further refinements have been published recently such as fucoaxanthin adjustments (see Devred et al. 2011 and Hirata et al. 2011), to account for the fact that Fuco is also a precursor for 19HF and 19BT and, therefore, partially attributed to the nanoplankton. Also a pico-eukaryote adjustment (see Brewin et al. 2010; Hirata et al. 2011), to account for the fact that 19HF is also attributed to pico-eukaryotes and therefore partially attributed to the picoplankton at low chl-a concentrations. Would these refinements help to further reconcile the CHEMTAX and

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size-based pigment analysis?

In the abstract, it is stated that "This method has the potential to become an automated approach where satellite data could be used to identify shifting boundaries of established ecological provinces" yet there is very limited discussion on this. Can this be achieved using current satellite ocean-colour sensors, or, would this require further advancements in technology, e.g. better hyper-spectral sensors, in consideration of the results shown in Table 5? Could approaches that detect phytoplankton functional groups (PFTs) from satellite offer an additional route? Nothing is mentioned on the current approaches for detecting PFTs from satellite (to name a few, Ciotti and Bricaud 2006; Alvain et al. 2008; Raitsos et al. 2008; Bracher et al. 2009; Kostadinov et al. 2009; Mouw and Yoder 2010; Devred et al. 2011), or assessory pigments (e.g. Pan et al. 2011; Hirata et al. 2011), which is surprising given the statement in the abstract. Could this information be used to help map the bio-optical provinces using satellite data?

The final comparison to the Longhurst's provinces is interesting, and there appears to be cases where the two approaches compliment each other, such as the SATL province, and in cases where there is no clear correlation the authors have explained well possible causes of such discrepancies, for example the static nature of Longhurst's classification and the dynamic nature of certain provinces. Have the authors not thought about comparing with a more bio-optical based province classification, such as a the static classification of Hardman-Mountford et al. (2008) based on Chl-a, or alternatively, the more dynamic fuzzy logic classification of optical water types by Moore et al. (2009).

Specific comments

Page 7167, line 1-3: There are in fact a number of satellite models that link the bulk Chl-a distribution to the size structure of the phytoplankton (see Uitz et al. 2006; Hirata et al. 2008; Brewin et al. 2010) and even to the taxonomic groups of phytoplankton

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(e.g. Hirata et al. 2011)?

Page 7167, line 7-9: I would suggest adding the Uitz et al. (2006) reference to this list.

Page 7168, line 8-9: I am not sure if I would refer to primary production and carbon cycling as measured parameters, I would consider that a parameter is a quantity that relates functions and variables using a common variable. Primary production is a variable, and the partitioning of ocean provinces can help constrain this variable through assigning province-specific parameters that relate to the mathematical function used to estimate primary production.

Page 7172, line 12-21, Section 2.2.1 Pigment analysis: How were the pigment data used in the study quality controlled (QC)? Were specific QC methods used (e.g. Aiken et al. 2009)?

Page 7177, line 7: The word “Spearman” is miss-spelt “Spearmen”

Page 7179, line 8-9: The sentence needs re-wording (perhaps remove “of surface samples only are shown in” and replace with “for surface samples only”.

How does the analysis shown in figure 3 directly compare to that of Uitz et al. (2006), Brewin et al. (2010) or Hirata et al. (2011) (it would be interesting to use the November Globcolour Chl-a map as input to these satellite models (which are based on Chl-a) to derive a satellite map of the percentages, and map the in situ percentages for each station onto the satellite image, in a similar way to that to that of Figure 8 and 9?)

Page 7188, line 24-25: I would suggest adding the reference of Bouman et al. (2007) to this list.

Page 7189, line 23: the word “by” is repeated twice consecutively .

Page 7198, line 14: The word “presence” is miss-spelt “presnce”.

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Interactive comment on Biogeosciences Discuss., 8, 7165, 2011.