

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

### **Anonymous Referee #1**

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This is a clearly written paper which fits well with the theme of the Biogeosciences special issue. It presents interesting data, but the evidence (essentially from 22 stations) may not be sufficient to support the strong conclusion implied by the title. The authors may care to consider the following points:

Section 2.1.3 – If radiometric data were checked according to the procedures of Wernand 2002, what was the outcome? What proportion of the readings was discarded?

Section 2.1.4 – The choice of  $F_0$  as a parameter for measuring chlorophyll seems odd, since (a) it is difficult to measure accurately and (b) it represents the state of maximum quenching. This probably explains why multiple recalibrations against HPLC data were required. Can the authors give any indication on the estimated uncertainties for their TChl-a measurements?

Sections 2.2.3 and 2.2.4 - Flow cytometric analysis tends to discriminate against large cells and colonies. Was there any evidence from the microscopy to indicate whether this occurred to a significant extent during the cruise in question?

Section 3.3 - the choice of parameters (wavelength ranges for example) appears to have been adjusted freely to provide the best correspondence between clusters derived from pigments and those derived from optics. Surely it is necessary to test whether the correspondence exists in an independent data set in order to validate this procedure?

Section 4.2 - this section, comparing the cruise results with the Longhurst system, appears to be inconclusive. Might this be a reason for inserting the words 'Possible' or 'Tentative' at the beginning of the paper title?

Section 5 - the conclusion seems to be that optical clusters derived from absorption or remote sensing measurements are congruous with those derived from pigment analyses, and therefore optical measurements could provide a short cut to identifying functional groups and bio-optical provinces. This argument depends critically on the degree to which the cluster relationships established for this data set are robust, and that surely depends on the acquisition and analysis of additional data which has not been used in the original clustering routines. Perhaps the authors could include this caution in their conclusions, or alternatively explain why it is irrelevant?

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

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