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Biogeosciences Discussions

Interactive comment on "Synoptic relationships quantified between surface Chlorophyll-a and diagnostic pigments specific to phytoplankton functional types" *by* T. Hirata et al.

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Reviewer 1

Error-quantified, synoptic-scale relationships between satellite based chlorophyll-a (Chla) and phytoplankton pigment groups at the sea surface are presented. A total of nine pigment groups were considered to represent nine phytoplankton functional types (PFTs) including microplankton, nanoplankton, picoplankton, diatoms, dinoflagellates, green algae, picoeukaryotes, prokaryotes and Prochlorococcus sp. The present paper applies the approach of Uitz et al. (2006) to get from satellite (SeaWiFS) chl-a directly information on the fractional contribution of various phytoplankton classes by a

parameterization based on a large data set on the distribution of diagnostic pigments obtained from global HPLC data sets. The paper extends the Uitz et al. 2006 approach to get more phytoplankton groups besides the three size classes, but it limits the approach to get only surface information instead of information on the vertical profile. The paper contributes with it's error estimates and a large (and extended as compared to Uitz et al. 2006) data base to the understanding of phytoplankton group dynamics for marine ecosystem and biogeochemical modelling and therefore fits well in the scope of the paper it has been submitted to. However, still a major revision of the paper is necessary to improve the results and discussion section: The discussion of the results in comparison to other methods has not be done and also the shortcoming of the presented method are not discussed in detail. One fundmental limitation of the method is that it is only able to retrieve from satellite chl-a phytoplankton groups dynamics which are expected. Unexpected shifts in PFT composition will not be detected by this method because no optical information measured by the ocean color satellite sensor is taken into account. In addition, the approach uses diagnostic pigments to infer the different phytoplankton groups globally, but this has not been verified globally by other techniques such as microscopy, particle size counter and flowcytometry. Since this is probably beyond the scope of the paper, this should at least be discussed as an limitation to the method. In addition, the paper should be extended by using the results of the fractional contribution of the various phytoplankton groups to the total chl-a to calculate their absolute chl-a and the dominant phytoplankton group for each pixel. In addition, it will be of great value to see the seasonal variation of relative and/or absolute phytoplankton composition. By that the results can directly be compared to other approaches to derive phytoplankton groups from satellite measurements and data are useful for further modelling studies. Also some citations in the text are not appropriate and should be changed.

OUR RESPONSE

(1) Comparison with other method has done and results are shown in the revised

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manuscript. The present method was compared to Uitz et al (2006) and Brewin et al. (2010), and the results and discussions are shown in the revised manuscript.

Reference: Uitz, J., Claustre, H., Morel, A. and Hooker, S. B.: Vertical distribution of phytoplankton communities in open ocean, An assessment based on surface chlorophyll. Journal of Geophysical Reseach., 111, C08005, doi:10:1029/2005JC003207, 2006

Brewin, R.J.W, Sathyendranath, S., Hirata, T., Lavender, S.J., Barciela, R.M. and Hardman-Mountford, N.J. 2010. A three-component model of phytoplankton size class for the Atlantic Ocean. Ecological Modelling, 221(11), 1472-1483.

(2) Discussions on unexpected shifts in PFTs and algorithm response to the shift is now added. Our global in situ data taken over 1995-2008 showed that a community shift at large spatial and temporal scales is not independent of a change in Chla of total phytoplankton community, and that the relationship between the community composition and Chla could be quantified. It is important for us to know whether the reviewer's assumption of the unexpected shift of PFTs, which significantly deviates from the relationship quantified in our manuscript, can indeed happen in reality at the appropriate spatial and temporal scales in the natural environment (i.e. basin and $\sim$10 years time scale). Therefore we appreciate the reviewer very much if he/she can provide us with a relevant literature that shows that the unexpected shift of PFTs actually happened and significantly deviated from the past relationships with Chla. We are aware that the quantified relationship shown in this manuscript can be degraded for smaller spatial and shorter temporal scales, but this has already been discussed in the original manuscript (6689, 14-L21). Also we suggested in the original manuscript how to deal with the proposed relationships deviated from reality (i.e. necessity of re-calibration of the relationship over time just like calibration of instruments for in situ observation). Nonetheless, we appreciate the reviewer's comment and therefore add a further discussion in the revised manuscript. (3) Discussions of pigment classification is now added. As the reviewer is aware, this is not a scope of this paper. However, we add

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some discussions on the problem to determine %Chla of PFT even from other in situ measurements. (4) New figures to show each PFT in the unit of [mgChla/m3] is added. (5) The figure showing the time series of each PFTs is replaced by that showing a seasonality in terms of monthly climatology.

In detail the following should be changed: - why write e.g. on page 6677 line 18 and ff. 5.8[% Chla] instead of 5.8% Chla? The brackets do not make sense.

The brackets are removed.

- Abstract,page 6677,line25ff. The sentence is unclear? If nanoplankton is the background globally with only 44%, why picoplankton with 47% globally is only the background for the subtropical gyres - this has to be expressed more clearly?

The sentence including the word “background” was modified in the abstract not to confuse readers.

-Page 6678, line 11-14 the citation of Nair et al. (2008) for the DMSP production in the ocean is not approriate! Better use Sunda et al. (2002), Nature 418, p317-320 Changed.

-Page 6678, line 21: the model is called “NOBM” not “NOMB”

Changed.

-Page 6680, line 3-11ff: you should also add for completeness to this sentence that another method to get information on the distribution of phytoplankton groups is the method by Bracher et al. (2009) which uses hyperspectral satellite data to get quantitative information on the distribution of phytoplankton groups (diatoms and cyanobacteria).

Added.

-Page 6688, Line 15: three words are missing after (“instead of % Chla”)- read again and change it.

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Changed.

-Page 6688, last paragraph- in order to judge if the different size classes but also other phytoplankton groups play a dominant rule overall or in certain regions, it would be helpful to calculate the dominant groups globally. Also add to the results the chl-a conc. of the different phytoplankton groups. Both will enable the comparison of results to other methods deriving PFT or size classes from satelite ocean color data.

Reviewer 3 points out that the fractional contribution (e.g. %Chla) is of much more use than merely the dominant phytoplankton presented in many earlier studies. In addition, inter-comparison of several algorithms, which determine the“dominance”, has already been done (Brewin et al., Rem. Sens. Envrion., in press). Therefore, we only add chl-a concentration of each PFTs in this manuscript.

Reference: Brewin, R.J.W., N.J. Hartman-Mountford, S.J. Lavender, D.E. Raitsos, T. Hirata, J. Uitz, E. Devred, A. Bricaud, A.M. Ciotti and B. Gentili. An intercomparison of bio-optical techniques for detecting dominant phytoplankton size class from satellite remote sensing. Remote Sensing of Environment, in press.

-Page 6689: as said above, improve the discussion by discussing the limitations to the presented method, e.g. that unexpected shifts in PFT composition will not be detected by this method and that to use diagnostic pigments in order to get certain variuos phytoplankton groups from HPLC data needs further verfication.

See our earlier comment.

- Page 6682: give size classes for micro-, nano- and Picoplankton in μm in the text in order to make clear what does this classification mean discuss the seasonal coverage of the supplied HPLC data

Size definition is now given. Temporal coverage of the data is added.

Interactive comment on Biogeosciences Discuss., 7, 6675, 2010.