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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.

Anonymous Referee #2

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Referee Comments for

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

General Comments

The paper "Synoptic relationships quantified between surface Chlorophyll-a and diagnostic pigments specific to phytoplankton functional types" by Hirata et al. uses a global HPLC pigment data set and the diagnostic pigment method in order to derive functional relationships between Chl-a and several phytoplankton functional types. In the context of the current community effort to move beyond just chlorophyll retrievals

from ocean color satellite data and derive alternative estimates of biomass and partition the biomass and its productivity into groups with distinct biogeochemical goals, the goals of the proposed manuscript are an important contribution and fit within the scope and style of Biogeosciences. Thus I recommend the paper for publication; however, only after some substantial revisions in order to address the comments below.

Most importantly, we as a scientific community need to address the issue of how the different biomass estimates (Chl, accessory pigments, POC, living carbon) relate to each other and how physiological responses and adaptations of the different species affect such proxies of biomass. I realize that a thorough discussion is outside of the scope of this paper, but the issue needs to be clearly stated and discussed a bit at the cellular physiological level. We need to understand whether accessory pigments change in tandem with Chl at the species level, i.e. with physiological adaptation for a given species, do ratios of pigments to Chl and between the pigments themselves change? This is an essential part of the error budget discussion of a Chl-based PFT parameterization, especially if one hopes to apply it globally.

In the introduction, the authors need to state more clearly what has been accomplished so far and what their new contribution is in that context. For example, it does not become easily clear what the improvements over Uitz et al. (2006) really are. There are other relevant PFT algorithms that the others cite; however, a brief overview of the available approaches is needed, stating where the current contribution belongs and how it is new. The approach of Kostadinov et al. (2010) needs to be added in the discussion since it uses a very different methodology.

Also, a comparison is needed between one or two different existing PFT models and the proposed algorithm, e.g. compare global climatologies with one that uses similar methodology (e.g. Uitz et al. (2006)), and one that uses different methodology (e.g. Kostadinov et al. (2010)).

Specific Comments

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You raise an important issue in Sect. 4.3 when you mention the secondary bloom in the North Pacific. It would be useful to expand this discussion further, comparing your results to the biogeographical province characteristics of Longhurst (2007) and discuss how the North Pacific and North Atlantic ecosystems are different, and how their blooms may differ in terms of timing, species composition, Chl and biomass. Can your data help explain the observed differences, which have been for example attributed to the HNLC character of the station PAPA region? Can you speculate on whether the Chl blooms are necessarily related to a biomass growth/species changes? Consider looking at a certain area around stations NABE and PAPA and generating a figure to compare and contrast those sites in terms of your PFT monthly climatology cycles.

The error budget needs to be clarified and discussed a bit further; can you for example make a map of the uncertainties derived for each group for the mission-mean Chl-a field? I suggest adding a figure formatted like Fig. 5 with the mean uncertainty fields. You also need to discuss in more detail various assumptions of the model and sources of error such as the lack of complete correspondence between size and diagnostic pigments, physiological variability (see above), etc. Then discuss which of these sources of error are captured by the regression residuals that you use as an error estimate.

HPLC data from the CHORS laboratory has been found to be unreliable by an extensive report from NASA, see http://oceancolor.gsfc.nasa.gov/DOCS/CHORS_Final_Report_Sec.pdf Are you using any of these data? If so, you need to remove it form the analysis.

You need to cite Sieburth et al. (1978) when you first mention pico- , nano- and microplankton.

Technical Corrections Please see the attached annotated PDF file for technical comments and corrections and additional suggestions for improving the manuscript.

References: Kostadinov, T. S., Siegel, D. A., and Maritorena, S.: Global variability of phytoplankton functional types from space: assessment via the particle size distribu-

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McClain, C. R.: A decade of satellite ocean color observations, Annual Review of Marine Science, 1, 19–42, 2009.

Sieburth, J. M., Smetacek, V., and Lenz, J.: Pelagic ecosystem structure: heterotrophic compartments of the plankton and their relationship to plankton size fractions, Limnol. Oceanogr., 23, 1256–1263, 1978.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/7/C3519/2010/bgd-7-C3519-2010-supplement.pdf

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