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***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 2

*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

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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)).

(1) While laboratory studies show phytoplankton pigment ratios to vary with environmental stimuli (nutrient forcing, light climate, refs), for in situ studies a much clearer relationship between phytoplankton community structure and pigment composition exists. Specifically, the ratio of chl_a to accessory pigments covaries with the abundance of different phytoplankton functional types (Fishwick et al. 2006, Aiken et al. 2007, Aiken et al. 2008, Hirata et al. 2008). Thus, shifts in phytoplankton community com-

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position rather than acclimation tend to dominate variability in surface oceanic pigment relationships, for example iron enrichment experiments have shown an increase in chl to be associated with a shift towards larger size classes (e.g. Gall et al. 2001). The link between phytoplankton-type specific chl and carbon (both POC and living carbon) is less well parameterised so care must be taken when converting between these different biomass measures. In any case, we appreciate the reviewer's comment, and discussions on determination of PFTs from HPLC pigments are added in the revised manuscript.

References: Fishwick, J. R., Aiken, J., Barlow, R., Sessions, H., Bernard, S. Ras, J.: Functional relationships and bio-optical properties derived from phytoplankton pigments, optical and photosynthesis parameters; a case study of the Benguela ecosystem. *Journal of the Mar. Biol. Assoc. U.K.*, 86, 1267-1280, 2007.

Aiken, J., Fishwick, J., Lavender, S., Barlow, R., Moore, G. F., Sessions, H., Bernard, S., Ras, J., and Hardman-Mountford, N.: Validation of MERIS reflectance and chlorophyll during the BENCAL cruise October 2002: preliminary validation of new demonstration products for phytoplankton functional types and photosynthetic parameters, *Int. J. Remote Sensing*, 28, 497-516, 2007.

Aiken, J., Hardman-Mountford, N. J., Barlow, R., Fishwick, J., Hirata, T., and Smyth, T.: Functional links between bioenergetics and bio-optical traits of phytoplankton taxonomic groups: an overreaching hypothesis with application for ocean colour remote sensing, *Journal of Phytoplankton Research*, 30, 165-181, 2008.

Hirata, T., Aiken, J., Hardman-Mountford, N., Smyth T.J. and Barlow, R.: An absorption model to determine phytoplankton size classes from satellite ocean colour, *Remote Sensing of Environment*, 112, 3153-3159, 2008.

Gall, M. P., Boyd, P. W., Hall, J. et al. (2001) Phytoplankton processes. Part 1: community structure during the Southern Ocean Iron Release Experiment (SOIREE), *Deep-Sea Research*, 48, 2551-2570, 2001.

(2) We further clarify the novelty of this work in the revised manuscript.

(3) Kostadinov's paper is now cited.

Kostadinov, T. S., Siegel, D. A., and Maritorea, S., Global variability of phytoplankton functional types from space: assessment via the particle size distribution, *Biogeosciences*, 7, 3239-3257, 2010.

(4) We compared our results with others as suggested (i.e. Uitz et al., 2006 and Brewin et al., 2010) and results and discussions are shown in the revised manuscript, whereas we were unable to compare with Kostadinov et al. (2010), since their data were not distributed in public (and also they claimed only $r^2 = 0.340$, 0.106 and 0.415 for pico-, nano- and microplankton, respectively, in their validation result). Please note that, in the original manuscript, we compared our algorithm against in situ data set. Also it is important to note that our work derives 6 PFTs as well as 3 size classes, and that no other algorithm has been offered to derive 3+6 groups of phytoplankton. Therefore we believe that a comparison with other methods should be an optional.

References: 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 Research.*, 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.

Specific Comments

*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

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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 questions above are scientifically very interesting. However, they are out of scope in the present manuscript.

*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.

It is not easy to derive the uncertainty for the whole globe since the in situ data were not available for each satellite pixel. However, we attempted to estimate the uncertainty and map it at the global scale, in the same format like Fig. 5 as suggested. Details can be found in the revised manuscript. Additional discussions regarding the size v.s. pigment issue and physiological variability are also added.

*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 from the analysis.

The CHORS data were removed and all analysis was repeated. However, we did not find a significant influence from the particular data set.

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*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.

Cited. Also thank you for the technical corrections. Much appreciated.

References: Kostadinov, T. S., Siegel, D. A., and Maritorea, S.: Global variability of phytoplankton functional types from space: assessment via the particle size distribution, *Biogeosciences*, 7, 3239–3257, doi:10.5194/bg-7-3239-2010, 2010.

Longhurst, A.: *Ecological Geography of the Sea*, 2nd Ed., Academic Press, San Diego, Calif, 2007.

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>

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

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