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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.

T. Hirata et al.

tahi@ees.hokudai.ac.jp

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Reviewer 3

*In this manuscript the authors present significant correlations between total Chl a and phytoplankton functional types (PFTs) and phytoplankton size classes (PSCs). Based on these relationships, the authors were able to derive global synoptic distributions of PFTs and PSCs from satellite Chl a data. As stated by the authors, this research bridges the gap between the current suite of PFT algorithms, which either derive the dominant PFT or PSC without estimating its fractional contribution to total chlorophyll, or derive the fractional contribution of a small number of PFTs or PSCs.

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In the analysis presented here, the fractional contributions of a combination of nine different PSCs/PFTs are examined. In fact these 'fractional contribution' results are of much more use to global biogeochemical- circulation modeling studies, than merely the "dominant phytoplankton" results presented in many earlier studies. The former can be used to quantitatively evaluate models, which is much more difficult to do if one has information only on which PSC/PFT dominates. The significant limitation of this methodology is that, like most empirically derived formulations, it cannot be used to forecast future changes in PFT distributions. If global climate change causes chlorophyll to double in a particular region, the relationships derived here would no longer apply. The relationships and correlations described here are only valid over the time period during which the in situ data used to derive the equations were collected. In fact, it might be interesting to see whether the relationships presented here changed if only the earliest 70% of the data set were used, rather than a randomly selected 70% of the data set. Overall, this is a well-written article that I would highly recommend for publication in Biogeosciences. Nonetheless, the manuscript can be improved by including additional emphasis on the two points above, as well as addressing the comments discussed below.

Our response to the review above:

(1) Thank you for the constructive comments. Satellite observation is meant, just like an in situ observation, to observe the present state or situation, rather than predicting the future. We are aware that the relationship quantified here may also vary over time under the future climate change. We discussed in the original manuscript that a calibration of the relationship over time is desired just like an in situ and laboratory experiments in which instruments should be calibrated constantly over time.

(2) We have removed a particular data set (CHORS data) and re-analysed to find that the result did not change significantly. We are aware that this is not the way suggested to see how robust the presented relationships between PFTs and Chla are. However, it does support the robustness of the relationship.

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General comments: 1. In the abstract, the authors state that they use nine phytoplankton functional types (PFTs), but then proceed to list three phytoplankton size classes (micro-, nano- and micro-) followed by only six PFTs. This needs to be made clearer throughout the manuscript. Six PFTs and 3 PSCs are being examined, not nine PFTs. In fact, I would recommend separating the PFT and PSC results in the figures, rather than having both included in the same figures. In addition the authors need to be clearer up front about which PFTs belong to which PSCs.

The text is modified to clarify 3 size classes and 6 functional types.

2. The authors gave a nice introduction of phytoplankton functional types, however, they need to be more careful when using terms to describe phytoplankton community structure, i.e., "functional types", "phytoplankton groups", "taxonomy" and "size-class". Sometimes these terms seem interchangeable and unfortunately many papers in the published literature do use them in a sloppy fashion. It would be helpful if within the introduction the authors could elaborate a little more on the subtle differences among these terms.

Terminology was reviewed and changed not to confuse readers.

3. I do agree the authors should not regress all PFTs to maintain a mass balance, however, I'm a little concerned about the choice of which PFTs should not be regressed. The manuscript did mention that "the best statistical fit was found in our data set when % Chla (nanoplankton) was not regressed". Can the authors specify what kind of statistical fit they are referring to? The RMSE listed in Fig. 3? In fact, on P. 6685 line 9-10, nanoplankton were found to be associated with maximum mean uncertainty. Would this be the case if the nanoplankton were regressed, as opposed to found via difference? Also, on P. 6686 line 2-3, the authors point out "microplankton and Picoplankton are inversely correlated". Wouldn't it be more reasonable to leave out one of these two tightly correlated PFTs when conducting the regression analysis?

We have used Nelder-Mead method for an unconstrained nonlinear minimization of the

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sum of squared residuals. We did regress nanoplankton, either with micro or pico, but it gave worth result in validation. As written in the original manuscript, the best result was found by regressing micro and pico. However, your comment is useful and we would like to investigate it in the future. One of the reason why we got such a result may be the relationship between Chla and nannoplankton is a non-monotonic function (Fig.2b) whereas that between Chla and microplankton or picoplankton is a monotonic function (Fig.2a and c) so that the accuracy of the fit is more sensitive to the selection of the fitting function.

4. Figure 5 nicely shows the surface PFT distributions in terms of the fractional contribution to total Chl a (in %). Although it's understandable that the manuscript focuses on the relative abundance, some readers may still be interested in seeing a similar map but with absolute Chl a concentrations (in mg m-3) contributed by each PFT. This will also help visually support the argument on P. 6688 line 14-15, "picoplankton may also be viewed as background community when absolute Chl a".

A new plot to show PSCs/PFTs in the unit of [mgChla/m3] is also shown in the revised manuscript, and the discussion about the background PFT is now removed.

5. I agree with the authors' decision not to show dinoflagellates in Fig. 5, according to the argument at P. 6686, line 26, "dinoflagellates are not considered here due to a poor result in the validation". For the same reason, I'd recommend the authors not include the dinoflagellates curves in Fig. 6.

Removed.

6. I have a general concern about the timing of the in situ data vs satellite data. The in situ pigment data cited in the article were collected over a time period that is different from that of the SeaWiFS ocean color data, e.g., the NERC AMT cruises were from year 1995-2005 whereas the satellite Chl a data covered 1998-2009. As the authors state, these algorithms will need to be recalibrated so they continue to be representative of the current state of the ocean. Should data have been used that preceded

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SeaWiFS? In addition, it seems that the dates of the in situ data used in this study were not provided, but that information should be included in the manuscript.

We use several data sets shown in Fig. 1, which covers 1997-2008. Satellite data from some months in 2009 were not used in this manuscript because of spatial coverage was poor in these months. Therefore, we believe that the time gap between in situ data and satellite data is minimal. Sampling period of the in situ data used is now added in the figure caption of Fig.1.

7. It would be helpful if more discussion was included as to how the results of this method compare with results of other recently published methods. Are these r-squared values more significant than those determined using other methods? Along these same lines, I was surprised to see that the recent Brewin et al. (2010) comparison article (in press, Remote Sensing of the Environment) wasn't discussed, since there is considerable overlap in the author lists of that paper and this manuscript!

Although there are no other method that derives 9 phytoplankton groups (6 PFTs + 3 size classes), we attempted to compare our result with others which derive 3 size classes (Uitz et al., 2006 and Brewin et al., 2010). Results are shown in the revised manuscript. In addition, some discussions are added, based on Brewin et al.(in press, Rem. Sens. Environ.) who compared "dominance"of 3 size classes derived from several satellite algorithms.

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.

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.

8. This manuscript would be easier to read if all the captions included a bit more information. It would be helpful for the reader if the Figure 2 caption reminded us that microplankton = diatoms +dinoflagellates, and picoplankton=picoeukaryotes+prokaryotes, etc. . . I was expecting to see that Figure 2 d through i summed to 100%, and was confused by this initially. It would almost be clearer if separate plots were made for the PFTs and for the PSCs. As in my first comment above, this distinction needs to be made much more clear throughout the manuscript.

Figure captions changed.

9. I think it would be more informative for the reader if Figure 6 showed one typical seasonal cycle in more detail (January 1st to December 31st), rather than the whole 12-year time series. There is clearly very little interannual variability – the seasonal variability is much stronger and would be a much more interesting discussion topic. Also, plotting both PSCs and PFTs on the same plot doesn't make sense to me. Again, I think two separate plots, one showing the three PSCs (which sum to 100%) and a second PFT plot would be of much greater interest to the reader. Another way to plot the PSCs would be to plot a line for total chlorophyll, and shade the region under the curve in three different colors, for micro-, nano- and picophytoplankton.

The 12-year time series is now replaced by the monthly climatology and discussions on seasonality are added. Since (1) the figure caption is now improved in the previous figure (Fig. 5) (e.g. to show microplankton = diato +dinoflagellates etc) and (2) there are lots of figures after revisions (reflecting comments from other reviewers), we decided not to separate figures of PSCs from those of PFTs.

Specific comments: 1. P. 6677, line23: Should be "ubiquitous throughout"

Changed.

2. P. 6677, line 25: Should be “limited to” not “limited in”

Changed

3. P. 6677, line 27: It is impossible to validate, or for that matter constrain, global marine ecosystem models. I would replace “constrain or validate” with “evaluate”. The fields derived using the methods described here would also be of great use as initial and/or boundary conditions for such models. This is another example of how these results are of much more use than methods that simply produce the dominant PFT rather than fractional contributions.

Changed.

3. P. 6678, line 12: please define abbreviation DMSp.

Defined

4. P. 6680, line 16: What is meant by “monthly 1 satellite”?

Corrected to “monthly satellite”

5. Section 2.1: Include dates of collection for data used.

There are too many data to show their collection date (e.g. the AMT data set includes several cruises) so that we have added years. Please note that the most of data were published as shown in Fig.2 captions.

7. P. 6681, line 3: For the in situ data, was there only one water sample <10m depth for all data sources? Or were the data averaged if more than one sample was collected between the surface and 10m deep?

Not one sample. Mostly more than one sample were collected, and not averaged.

8. P. 6681, line 27: Are the shelf data masked out in the in situ data as well? No.

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9. P. 6684, line 1: the unit for Chl “mg m3” should be “mg m-3”.

Corrected.

10. P. 6685, line 23: “varis” should be “varies”

Changed

11. P. 6686, line 21: This is an extremely important statement, and perhaps should be alluded to in the abstract. I would reword this as: “ Physiological changes in the phytoplankton due to environmental changes may necessitate a regular recalibration of the relationships over time.”

We did not reflect the statement in the abstract since it is only a discussion, not a result.

12. P. 6687, line 28, “. . . the continental shelf, which is not considered here due to a limitation of DPA.” It would be helpful if the authors could highlight what exactly is the limitation for applying DPA to coastal water.

Highlighted.

13. P. 6688, line 22: “midhigh” should be “mid to high”

Corrected.

14. P. 6689, line 10: “. . .re-calibration of the algorithm may be required constantly over time to reduce the ambiguity. Such a calibration of the algorithm has been conducted several times. . .” This is a confusing sentence, but is a very important point. I believe that the first reference to the word ‘algorithm’ above refers to the types of algorithms derived in this manuscript. The word ‘algorithm’ in the next sentence seems to refer to NASA ocean color chlorophyll algorithm. This should be made clearer. Have the algorithms developed in this paper for PFTs been recalibrated several times? The text above makes it sound as if they have been.

Changed.

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10. P. 6696, Table 2 caption: In the first column of Table 2, why are there slashes preceding diatoms and dinoflagellates in the first column?

Removed.

15. P. 6700, Fig. 2: In the caption, "(g) Prokaryotes, (h) Prochlorococcus sp." should be "(g) Dinoflagellates, (h) Prokaryotes, (i) Prochlorococcus sp." A similar correction is required on the Fig. 3 caption.

Corrected.

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