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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 #3

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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. 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.

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.

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

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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.

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?

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".

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.

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

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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 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.

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!

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.

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.

Specific comments:

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1. P. 6677, line23: Should be “ubiquitous throughout”

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

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.

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

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

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

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?

8. P. 6681, line 27: Are the shelf data masked out in the in situ data as well?

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

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

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.”

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.

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

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.

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?

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.

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