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Biogeosciences Discussions

Interactive comment on "Does chlorophyll *a* provide the best index of phytoplankton biomass for primary productivity studies?" *by* Y. Huot et al.

Anonymous Referee #1

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General comments

This paper addresses the use of different indicators of phytoplankton biomass in studies of primary productivity in the ocean. In particular, it relates phytoplankton or phytoplankton-related properties, which can be detected remotely, to photosynthetic parameters, in the wider context of bio-optical modelling of primary production. This topic is of interest to the readership of Biogeosciences and the authors use an extensive dataset - albeit one that covers only temperate and low-latitude regions. The authors conclude that chlorophyll a concentration is superior to other indicators of phytoplankton biomass because it correlates better with the photosynthetic parameters Pmax and alpha, particularly when additional data such as depth and irradiance are also considered.

I have two main comments to make. The first refers to the validity of the question asked. The second refers to the validity of the approach used to answer it.

1) The question asked may not be the rigth one: is there anything like the best indicator of phytoplankton biomass? It may well be that there is no unequivocal answer to this question. Even within the context of remote sensing studies of primary productivity, different studies may have different purposes, as well as different constraints in terms of available data. As the authors themselves acknowledge, their study has a limited latitudinal coverage. The conclusion that chla remains the best indicator of phytoplankton biomass (without qualifiers) is not warranted, unless other regions such as higher latitude ones were also considered. In addition, it may well be that a particular index works better when compared with alpha, and less well when compared with Pmax. Again, depending on the particular objectives of the study, a choice can be made as to which is the best indicator. A more 'neutral', if less catching, title could be something along the lines: Relationship between photosynthetic parameters and different proxies of phytoplankton biomass in the ocean'

2) The best and most obvious way to evaluate accurately if a proxy is good is to compare it with the variable for which it is a proxy. Obviously, phytoplankton biomass (carbon) is a difficult thing to measure in the field, but laboratory cultures may offer a way ahead, as they provide the means to simultaneously measure phytoplankton carbon (using elemental analysis) and various other properties such as chla concentration, absorption, scattering, etc. In fact, it is likely that these measurements are already available in the literature and may allow at least a preliminary analysis - which could complement the one presented here. Trying to compare pigment and optical properties to phytoplankton biomass in situ is not impossible either: different techniques exist to estimate phytoplankton carbon in natural populations. They are all subject to error, but at least they are independent estimates of phytoplankton standing stock only (while Pmax or alpha depend not only on standing stocks but also on photosynthetic activity). The bottom line is that, if a particular biomass proxy does not correlate well with Pmax

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or alpha, it does not necessarily mean that variable is not well correlated with phytoplankton biomass. The authors should acknowledge the limitations of their approach to evaluate the performance of the different biomass indicators.

Specific comments

1) Alpha is much more difficult to measure accurately than Pmax. Perhaps some biomass proxies do a poor job at predicting alpha not because they are not correlated with biomass, but simply because alpha is more noisy.

2) A subtantial amount of biomass is left out when Prochlorococcus is not measured in ultraoligotrophic waters. In more productive waters, given that larger phytoplankton cannot be measured by the flow cytometer, a major part of the biovolume is again missed. This puts a serious question mark on any comparison between biovolume and other variables made in this study (Figs. 3, 4). Authors should try to evaluate the degree to which this may affect their conclusions.

3) Related to point 2 above, on p. 728, 1st paragraph, the authors write 'Pmax is a good proxy for phytoplankton carbon'. But phytoplankton carbon has never been measured in this study. The biovolume estimates used here are problematic, because they underestimate real biovolume by an unkown degree. In addition, the relationship between cell volume and cell carbon is complicated: it changes with taxon, physiological status, cell size, etc. In any case, the reasoning on top of p. 728 seems to be misleading. The authors go on to suggest that Tchla is the best way to predict phytoplankton carbon: however, it is known for a fact that the C to Chla ratio in the surface ocean changes widely in response to light and nutrient availability.

4) Sections 4.4, 4.5 and, in particular, 4.6 are digressive and speculative and should be greatly reduced.

Minor points

-p 713, line 8: linked to the remote

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-p 713, line 27 PROSOPE

-p 720 lines 1-2. This may not have any relevance for the paper, but was Prochlorococcus abundance really estimated from divinyl chl a concentration? Cell content of this pigment, like any other pigment, is highly variable, so it is hard to see how one can convert pigment concentration into cell abundance.

-p 723 line 11. Shouldn't it be Pmax $<$ 1 instead of $<$ 0.1?

-p 729, line 24 from different

-Plots in figures 1, 3 and 4 are very small - I could only read them by using a 300% zoom factor. In particular, all fonts should be much larger.

-In Fig 1, the color scale bar should be shown only once.

-In Fig. 1B, why Tchla is not shown as logTchla, as it is in the others plots?

-In Figs 3 and 4, biovolume units should be um3/ml

END OF REVIEW

Interactive comment on Biogeosciences Discuss., 4, 707, 2007.