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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.

Y. Huot et al.

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Part II

1) The reviewer writes:

At the end of section 4.2, your conclusion is that cp is the best estimator of Pmax and that bbp matches Tchl in estimating Pmax. Why then do you continue from this point on to invalidate the value of the optical indices and, to this end, your own findings?

We are not attempting to invalidate the value of the optical indexes, we simply want to examine in more detail the causative mechanisms underlying this finding. The point of view of the reviewer is perhaps misguided, when he states:

Here, the authors take a selected set of additional variables and throw them into a thoughtless stepwise regression to improve the performance of Tchl in explaining Pmax

variability.

As mentioned elsewhere in his review, it is not difficult to improve the $P_{\max}$ vs Tchla relationship, as photoadaptation can relatively easily be accounted for through a function of growth irradiance or depth. However, the relationship between $P_{\max}$ and b_p is much harder to improve, and this is why so many variables were tested — *not to improve the fit using Tchla*. Again, this is fully consistent with approaches in the past where environmental variables are expected to control $P_{\max}$ or growth rates (this is how, for example, Behrenfeld et al. 2005 validated the carbon-based approach). One could argue that the variables chosen are not the appropriate ones or that the form of the function is incorrect, however, the approach is not, in our opinion, fundamentally flawed.

The reviewer states:

Next, the authors need to consider the pertinent variables necessary to improve relationships between cp or bbp and Pmax. It is important to recognize that these are not the same variables as those necessary to improve the chl-Pmax relationship, so throwing together a set of variables (and variables-squared) that improve the chlorophyll fit but not the scattering fit is far from adequate. As discussed earlier, cp and bbp (and biovolume) are estimators of biomass, not Pmax. The key difference is growth rate.

Looking at the equation of biomass normalized $P_{\max}$ and expressing it in terms of growth rates (and ignoring respiration) we find the following:

and $P^C_{\max} = \mu_{\max}$.

This would imply that if we had a perfect measure of C, the only remaining variability would be in *growth rates*. If we had a perfect measure of *chlorophyll* we still need to account for the variability in *C/chl and growth rates*. It follows that it should be easier — not harder — to find environmental variables that account for the variability in $P_{\max}$ vs C (as estimated from b_p) than in $P_{\max}$ vs chl. Growth rate has to be accounted for in both normalizations.

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The misunderstanding over our objectives crops up again as the reviewer states:

It is also noteworthy that without the growth rate data, they cannot also covert their chlorophyll data (corrected for photoacclimation) to biomass and can therefore make no claims as to the accuracy

with which phytoplankton biomass can be assessed from chlorophyll.

Again, this is not what we are attempting to do. We hope this is very clear in the present version of the paper.

2) The reviewer writes the following concerning section 4.4:

This section begins with the following statement:

"In summary, simple functions of Tchla along with information about the irradiance or depth appear more accurate for estimating photosynthetic parameters (and eventually primary productivity) than scattering based methods."

I am hoping that this statement simply reflects a complete misunderstanding of the

scattering based approach.

No, this does not reflect a complete misunderstanding of the carbon-based approach, it simply summarizes our results. However, to avoid confusion with the carbon based approach, we will rephrase this section:

"In summary, simple functions of Tchla along with information about the irradiance or depth appear more accurate for estimating photosynthetic parameters (and eventually primary productivity) than similar functions using scattering or backscattering."

3) We dropped this section to keep the focus on the main findings of the paper. Below is our response to the reviewer's comments on this section. We are confident that our analysis was correct.

(i) The relative accuracy of retrieval of b_{bp} from space is central to the problem! If

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large errors in b_{bp} are present it means that equivalently large errors will be made in the retrieval of phytoplankton carbon, which are "added" to the natural variability in situ in the phytoplankton carbon to b_{bp} ratio. Of course, any model that does not use carbon will not be affected if the retrievals of carbon are erroneous. We were testing our regression-based approach for retrieving $P_{\max}$, not the carbon-based approach of Behrenfeld et al. (2005).

(ii) We disagree with the reviewer that deep-water samples are akin to a deep mixed layer. Species composition changes vastly between the deep layers of stratified systems and deeply mixed systems. As we note later in the paper, temperate and polar waters are not well represented in our dataset, as such they are representative more of tropical and subtropical systems. The title of the paper has been changed accordingly.

4) We are asking a simple question here, given a regression approach using Tchla (which was shown to provide more accurate prediction of $P_{\max}$ than the same approach using b_p). Is there any supplementary information available in the scattering coefficient? The reviewer tells us:

The way to improve the Chl:Pmax relationship after accounting for photoacclimation is to also address any nonlinearities or intercepts in the chl:C and growth rate relationship.

We can thus verify this by looking at the remaining variability in our relationship as a function of chl/b_p. This is what we attempted to do using our "stepwise regression soup", as the reviewer calls it (one he apparently found too salty!). Another approach is to plot the residual of our relationship as a function of Tchla/b_p and to look for any functional relationships or an intercept. If our regression doesn't account properly for the growth rate, and the ratio Tchla/b_p is a good proxy of it, then a relationship should emerge. This is what is done below:

There is no obvious trend or intercept. We kept the section "as is" because the two analyses are in essence the same, but changed the conclusions slightly because the scattering data was found to significantly (but only slightly) improve the regression.

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5) We apologize for this oversight; a sentence about potential errors in the data was present in an earlier draft, which was deleted inadvertently from the submitted version. We have now, however, removed this section altogether to keep the paper focused on the main findings.

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

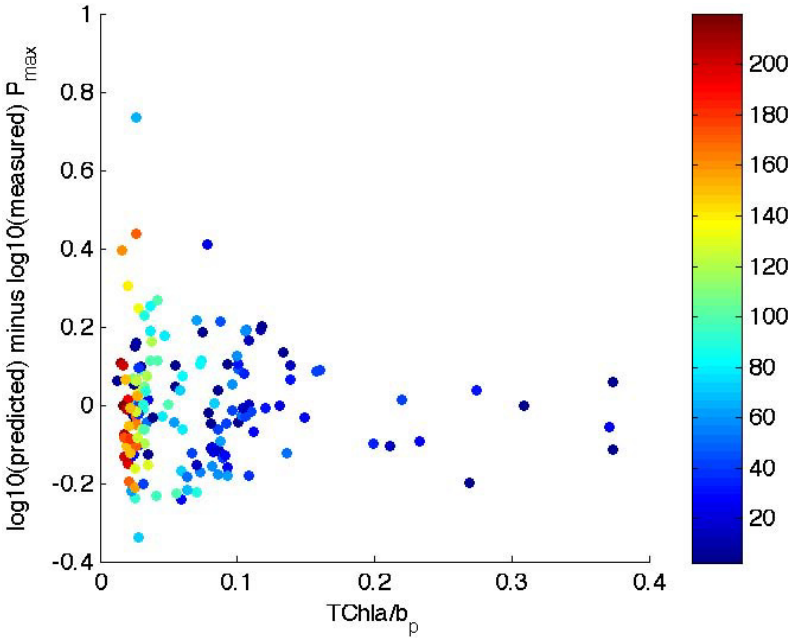

Figure 1:

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