Biogeosciences Discuss., 4, S1160–S1162, 2007
www.biogeosciences-discuss.net/4/S1160/2007/

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.

Received and published: 14 August 2007

We are thankful to the reviewer for his objective and clear review. We have addressed below most of his comments and suggestions. Some of our responses here are redundant with our responses to the review of Michael Behrenfeld but are repeated for completeness.

General comments

1) We have changed the title to one similar to that proposed by the reviewer; it provides a very clear description of what we are addressing in the paper.

2) The reviewer makes some interesting points. Several experiments have indeed shown in culture that all the biomass proxies used here are very well correlated (for a given species) with phytoplankton biomass. However, this is not surprising as cultures

are usually grown under conditions where they are the main particulate component in the water. Variability between species is, however, clear which makes extrapolation to the field with a largely unknown particle mixture difficult. We have added a short section describing some results from cultures. We do not have access to independent measures of standing stock in the datasets collected in our cruise. The flow cytometry determination of phytoplankton biovolumes, are the closest we come to an independent measure of phytoplankton carbon biomass. As the reviewer points out, it is not perfect. What matter most to us, however, and this has been clarified in the present version of the paper, is to evaluate how well the biomass proxies allow estimate of the photosynthetic parameters.

Specific comments

1) The errors on the parameters put a minimum average error (or r2) that can be achieved in their estimates. Simply, scatter would exist if two independent measures of alpha were done for every sample and compared with each other; whereas in a world without measurement errors the correlation coefficient would equal 1 and the average error 0. However, because we use the same dataset to compare with the independent proxies, this is not a problem. It, however, limits our ability to distinguish (obtain a significant difference) between two similar indexes as the differences observed have to be greater to obtain a significant difference. Errors on alpha and the independent variables are likely the reason why, for example, Tchla appears a better (though not significantly) estimator of alpha than aps, despite the fact that aps is theoretically better. We have added a new table 2 which allows viewing which variables are significantly different.

2) We were aware of this limitation and this is why we had mentioned "The maximum cell diameter observed with the instrument settings used during the cruise was 3 micrometer, this included most of the phytoplankton cells in oligotrophic waters but missed a significant fraction in more eutrophic waters." We hadn't provided a similar statement about the oligotrophic waters. We have now added: "Similarly, the absence of Prochlorococcus may miss a significant fraction of the biomass in oligotrophic wa-

BGD
4, S1160–S1162, 2007

EGU

ters." We do not know, however, of a rigorous method to assess the potential bias involved. All we can do is limit our conclusion to the size range measured with the flow cytometer.

3) We have removed this sentence altogether as it was unnecessary (though we note that an "If" was placed before the part quoted by the reviewer). We were referring to the study of MacIntyre not to the measurements of biovolumes in this sentence; but this was not sufficiently clear.

4) To keep the focus on our dataset, we dropped sections 4.4 and 4.6. We do not believe section 4.5 was digressive or speculative and we kept it. The results in this section changed slightly following a reprocessing of the backscattering and scattering data. We hope that these changes are satisfactory.

5) Corrected

6) Corrected

7) Estimates were done using Divinyl chla and information on the growth irradiance therefore accounting at least partly for photoacclimation. However, this has no bearing on our paper as we do not use these data.

8) This was correct.

9) Corrected

10) We have improved the figures legibility by splitting figures 3 and 4 each into two figures.

11) Yes, done.

12) Corrected

13) Corrected

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