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Biogeosciences Discussions

Interactive comment on "Satellite-detected fluorescence reveals global physiology of ocean phytoplankton" *by* M. J. Behrenfeld et al.

M. J. Behrenfeld et al.

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Response to Referee #1: Ru Morrison

Reviewer's General Comments

The paper presents the first global scale analysis of phytoplankton fluorescence detected by satellite based sensors. Most of the variability in the fluorescence signal is demonstrated to be associated with changes in 1) phytoplankton biomass, 2) 'pigment packaging', and 3) phytoplankton physiology (non-photochemical quenching, NPQ). The quantum yield of fluorescence (the ratio of photons fluoresced to those absorbed) calculated after accounting for three dominant factors is shown to spatially co-vary with a modeled Growth Constraint Index that distinguishes iron-limited waters. Higher quantum yields are associated with increased iron stress due to changes in stoichiometry of

PSI and PSII reaction centers. This paper represents a truly significant increase in the understanding the phytoplankton fluorescence variability with potentially great impact on the understanding of phytoplankton physiology on a global scale and consequently estimates of oceanic primary productivity.

Authors Response: My coauthors and I appreciate the reviewers comments and recommendations.

Reviewer's Specific Comments

(Comment 1) The work presents a novel analysis of the satellite derived fluorescence line height and provides a detailed derivation of the approach in the Appendix. The new aspects are the recognition of the contribution of the excitation irradiance (iPAR) in the FLH product derived from the normalized water leaving radiances that has been missing from many previous satellite based studies and the normalization for the NPQ using the inverse of the iPAR. (Biomass and pigment packaging effects have been parameterized previously). With these corrections the authors produce a product they term the 'satellite fluorescence quantum yield'. This might not be the best terminology for this product as the quantum yield is the ratio of photons emitted to those absorbed which would include the effects of NPQ. A better term might be the NPQ-normalized fluorescence quantum yield.

Authors Response: As per the reviewer's comment, we have used the term 'NPQ-normalized fluorescence quantum yield'; in the revised manuscript (pages 4, 7, and 12)

(Comment 2) (from 'review of requested aspects of the work') Question 13: Should any parts of the paper be clarified, reduced, combined, or eliminated? Not really, but the color scales on the global distributions of the quantum yield (e.g. Figure 4A) appear to use only half the range given in the legends (i.e. all appear to be dark blue to cyan). Expanding the range of the color scale (0 to 1.2?) might give a better presentation of the global scale variability of this product.

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Interactive Comment

Authors Response: In the revised manuscript, we have expanded the scale for figures 4, 5, and 6 from 0 to 1.2% and used a red-blue color scheme to more clearly bring out features of the quantum yield data.

(Comment 3) Page 4239 Line 3: By accounting for NPQ the fluorescence quantum yield is not really derived, rather a NPQ normalized fluorescence yield product (see note in section above)

Authors Response: See response to Specific Comment (1) above

(Comment 4) Page 4240 Line 11: What was the atmospheric correction used (citation would be useful)

Authors Response: The SeaWiFS ocean color processing group uses an atmospheric correction scheme based on Gordon and Wang (1994). A citation to this paper has been added in the revised manuscript.

(Comment 5) Page 4240 Line 15: Were there water leaving radiances at 748 greater than zero for the non coastal areas included? If not would this simplify the derivation of Fsat.

Authors Response: In the processing of SeaWiFS data, the 748-nm nLw is from the NIR water-leaving radiance correction and is forced by this model to return a value of zero for chlorophyll concentrations $<$ 0.7 mg m-3. Thus, nLw(748) is generally zero in open ocean waters, but is not always zero (i.e., it can be greater than zero for pixels with chlorophyll $>$ 0.7). Consequently, the current description on the derivation of Fsat is globally correct and was not changed in the revised manuscript, even though a simplified description may be possible for many open ocean pixels.

(Comment 6) Page 4245 Line 9: Some details (citation) of the light utilization model might provide some useful information. What was the iPAR value at the equator used? What fraction of the absorbed light energy was emitted as fluorescence?

Authors Response: In the revised manuscript we have added the following statement

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Interactive Comment

(pg. 11-12):

(light saturation was described by an exponential function (Henley 1993); noon maximum iPAR was 1900 umol photon m-2 s-1)

Since the calculation being performed was aimed at assessing the ratio of absorbed light to photosynthetically utilized light, the absolute value of the photosynthesis-irradiance parameters is largely irrelevant, as is the absorbed light energy emitted as fluorescence (i.e., we are not calculating the fraction of energy fluoresced, only the fraction use for photosynthesis).

(Comment 7) Page 4245 Line 25: See previous comment about the terminology of the quantum yield product.

Authors Response: See response to Specific Comment (1) above

(8) Page 4252 Line 22: The FLH approach 'approximates'; the removal of the nonfluorescence contributions to the normalized water leaving radiance as the baseline is not linear.

Authors Response: In the revised Appendix A, we have added the term 'approximately'.

(Comment 9) Page 4256 Line 17: See previous comment about the terminology of the quantum yield product.

Authors Response: Unlike comments (1), (3), and (7) above, I believe no change is needed here because the sentence already reads, 'an NPQ and pigment packaging corrected satellite fluorescence quantum yield';

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Referee #2: Anonymous

Referee's General Comments

The world's oceans are the site of ˜50 % of global primary productivity. As a conse-

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quence, the photosynthetic activities of phytoplankton of the open ocean are an extremely important component of the global Carbon cycle. A major obstacle to gaining a thorough understanding of oceanic primary productivity however is one of scale. Biological oceanographers typically work with small discrete water samples and even if multiple samples are taken over a grid, they only represent a small portion of the ocean. Remote sensing of the oceans has dramatically changed our ability to obtain synoptic pictures of phytoplankton distributions in the oceans, but to date an estimate of their physiological condition has still been restricted to discrete, smaller scale sampling. The current paper by Behrenfeld et al. changes this. Using satellite based chlorophyll fluorescence signals, Behrenfeld et al. have been able to obtain synoptic data on three main features of phytoplankton fluorescence and physiology, namely chlorophyll concentration, non-photochemical quenching (NPQ - by which excess light energy is dissipated as heat) and the package effect (where there is self-shading which reduces the efficiency of light absorption). The authors illustrate the efficacy of this approach to examine the impacts of iron limitation. The approach is careful and methodical and the arguments and analyses clearly presented and well substantiated. The appendices outlining the approaches used are especially informative.

Authors Response: My coauthors and I appreciate the reviewer's comments.

Reviewer's Specific Comments

(Comment 1) The authors acknowledge that this is a first step in developing the approach and they have made a number of approximations and assumptions that are debatable; for instance they have used a single light function to correct for NPQ, whereas light acclimate and NPQ will vary greatly with, for example, latitude and other environmental factors. As with all remote sensing data, fluorescence signals are dominated by the top several meters and, as a result, the information and interpretation suffer from the fact that impacts of phytoplankton in deeper water are not detected. In many cases this might not matter, but in waters characterised by sub-surface maxima in chlorophyll distributions, this might be an issue. Nonetheless, this paper represents a novel and

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exciting new tool for biological oceanographers. Further refinement will undoubtedly lead to new and improved understanding of physiological processes in phytoplankton at the global scale.

Authors Response: The reviewer has made a very valid point here about passive ocean color remote sensing data: it is all very much limited to properties of the surface mixed layer. In the current case, our objective is to view physiological properties of the mixed layer phytoplankton detected through satellite sensing, not those of cells beneath this layer, so no change is needed in the manuscript. Nevertheless, one of the major future challenges facing biological oceanographers is the accurate representation of biological stocks and rates in the lower euphotic zone.

Interactive comment on Biogeosciences Discuss., 5, 4235, 2008.