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Biogeosciences Discussions

Interactive comment on "Light availability in the coastal ocean: impact on the distribution of benthic photosynthetic organisms and contribution to primary production" *by* J.-P. Gattuso et al.

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General comments:

Overall, I feel this is an important "first step" global study which combines analysis of potential light availability (from a global bathymetry dataset and estimates of KPAR from composited SeaWiFS ocean color imagery) and a thorough review of the literature on light requirements of various major classes of benthic photoautotrophs. There are certainly limitations/uncertainties associated with this analysis (e.g., use of the Case 1 chl-dependent estimate of KPAR of Morel, 1988 for Case 2 coastal waters), but overall I feel that the authors do a good job noting the issues and pointing to possible future refinements of this initial global analysis. I recommend publication of the manuscript with some minor revision.

The audience for this manuscript could be rather broad. A general suggestion would be for the authors to keep this in mind in making revisions. In my opinion, this is really something of a 2-part study: (1) SeaWiFS KPAR/global bathymetry analysis; 2) estimating relevant light-photosynthesis parameters for various classes of benthic photoautotrophs from an extensive review of the available literature. In the present form, I feel these components are not as cleanly coupled as they might be with a little reorganization of the paper. Some sections took several readings to understand the points being made regarding the Figures and Tables. Some revision to better define the study components in the Introduction (let the reader know how the study is being approached) and to link the pieces in the Discussion could strengthen the paper. Also, some brief additions to the description of the satellite analyses might make that section more readily understood by readers not closely familiar with ocean color measurements, the various products derived from SeaWiFS, and associated uncertainties.

Specific comments:

Title: At this point, I think it would be more accurate to qualify this as "Ěpotential contribution to primary production."

Structure/organization of the paper: The Methods and Results sections are basically the SeaWiFS analyses, followed by a brief analysis section for the satellite results in the beginning of the Discussion. The remainder of the Discussion section is basically the literature review (some 16 pages) and derivation of summary information for light compensation, etc. for various major functional groups of benthic photoautotrophs. This compilation from the literature (a significant accomplishment) was noted very briefly in the Methods (Section 2.6). I think it would help the reader follow the line of argument and approach to the study if these separate parts of the study were more clearly organized. I question whether the long literature review and derivation of compensation intensities for various functional groups, etc. (sections 4.2, pgs 908-924; Figs 4-8; Tables 2-8) is really appropriate as "Discussion" section material. It seems to me that, while a literature review, much of this material in the associated Tables and Figures would be more in the Results area. And finally it is the combined satellite/functional group information (Tables 5, 8) that is being the central to the overall thrust of the paper. But in my view, the discussion of these Tables is somewhat subsumed into the literature review section. A suggestion would be to consider narrowing the Discussion section to where the summary of compensation irradiance, etc. for functional groups is combined with the SeaWiFS global analysis.

Introduction: Last paragraph - Related to the previous comment, it might be useful for the reader if the brief overview of the organization of the paper were expanded somewhat to clearly define the study components and how these will be presented.

Methods: Section 2.1, Line 3 - "average depths" in what sense? Over the satellite pixel areas?

Section 2.1, Line 20 - Does the fact that "proximal" pixels include some portion of the coastline impact the derived product estimates (chl, R(555), Kpar)? Or is the land signal adequately removed? Also, I missed the definition of "proximal" and "distal" initially and was thus confused by these terms subsequently (e.g., Fig. 3, Table 1, Sections 3.2, 4.1). The definition is there, but perhaps a sentence could be added to the Table 1 header or Fig. 3 caption to refer the reader to Methods for the definition. (See comments regarding Fig. 3 below).

Pg 900, section 2.2 - For the reader not familiar with the SeaWiFS data types, a brief definition of "Level 2", "Level 3" might be useful.

Pg 900, line 27 - "actual chlorophyll-a concentration" means what? In situ measurements?

Pg 902, section 2.4 - A comment: The uncertainty associated with chlorophyll (Csat) estimates for Case 2 waters are appropriately noted. I agree with the authors that this still provides a useful starting point for a global analysis.

Pg. 902, Section 2.5: I think it would be useful to state the objective of this analysis (Section 2.5, comparison of SeaWiFS versus Secchi disk estimates of KPAR) was clearly stated up front. The approach seems to me to be something of a climatological comparison. Is the comparison with Secchi-derived estimates of KPAR considered to be validation of the satellite-derived estimates? Or is this more a comparison to another source (best available with recognized limitations)? That is, results are presented in the Methods section (Fig. 2), but I didn't get a good idea of how the SeaWiFS- versus Secchi-derived KPAR estimates were interpreted by the authors.

Pg. 902, Section 2.5: Both the Holmes (1970) and Weinberg (1976) relationships are noted, and it is stated that both were used. But unless I missed it, only the Weinberg estimates are shown in comparison to the SeaWiFS Chl-dependent estimates (Fig. 2). Also, are the Secchi depths from the WO database provided on a monthly basis? Annual?

Results. Comment: It would seem appropriate to me to explicitly separate SeaWiFS results in sub-section titles, then follow as part of the Results section with the parameters derived from the literature summary of compensation irradiances, etc. Overall, I would interpret the "Results" of the study to include more than is presented in Section 3 (pgs 903-904; Table 1, Figs. 3,4).

Fig. 3 - distal/proximal panels - I think the pink/green panels in Fig. 3 could use a better explanation in the figure caption. I found this to be a bit confusing and had to look at this for awhile (and zoom in on the axes labels in the pdf file - the print version axes labels are very small font) before understanding that this was the distal/proximal ratio for the UNAVAILABLE pixels. (That is, try to anticipate what might be misinterpreted by the reader).

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Section 3.3, Fig. 4, pg 904: Fig. 4 is perhaps the most important result from the combined analyses of the global data sets for coastal ocean bathymetry and SeaWiFS-derived KPAR ("P-functions" defining relationships between benthic habitat surface area and daily irradiance levels). I was somewhat confused by the presentation in Fig. 4 and explanation in the figure caption and Results (section 3.3, pg 904). I think I did get the point after a good look, but I feel the authors should focus on making very clear what is presented in this figure. The large difference between Case 1 and 2 components of the total draws one's attention, but seems to get limited explanation/discussion. Would the range of monthly P-functions for Case 1, Case 2 be comparable to that illustrated for the annual (black, gray) P-functions? Why do Case 2 waters appear to receive more light at the bottom? Is this primarily a function of Case 2 dominance of shallow areas (maybe not if I am interpreting Table 2 correctly). This point is also relevant to Table 8 (% Case 2 area $>$ % Case 1 area for irradiance above daily community compensation level).

The polynomials in Fig. 4 seem to be expressions or such as opposed to equations (i.e., one side of an equation; ie., % area = is not included). And is it appropriate to express the coefficients for these expressions to 7-8 significant figures?

Table 1: Header and table rows - "depth" and "pixel depth" mean what here? Averages over the pixels? Is the negative depth convention necessary here? If space allows, I think the readability of the Table would be improved if a line space was inserted after each pair of rows (fraction pixels, depth of that set of pixels).

Table 2: I did not find Table 2 referenced in the text (after a search of the pdf file). It seems like this Table should be referenced in the Results section for the SeaWiFS analysis or in Section 4.1 (pgs 906-907).

A comment, pg 907 - Good points are made concerning reporting of absolute versus relative benthic irradiance.

Discussion Again, whether the thorough literature review and derivation of the summary information for various benthic photoautotroph functional groups is really "Discussion" material is not clear to me. The transition between the discussion of the satellite analyses and the literature review was rather abrupt (starting at Section 4.2). Whether or not that section remains in the Discussion, I feel that the reader might benefit from a brief overview of what is to follow and how that will be linked to the satellite results.

Section 4.2: As noted above, this is a thorough review and covers a diverse literature. This is normally what would be part of a literature review paper. It is rather lengthy, but reads well, and rather than formally separating the two parts of the study into two papers, I think it is useful to keep these combined with some attention by the authors to a bit better integration of these components. I have a few minor comments on the literature review section.

Seagrasses section, pgs 914-915, Fig. 7 - The Fig. 7 discussion (comparison with Australian seagrass distribution data) is somewhat of a deviation from the more global-scale focus of the rest of the paper. The point of this might be more clearly stated (Validation? Bringing out uncertainties in the approach, such as the limited information on bottom types?).

Microphytobenthos, pg 918 – While viable benthic diatoms (as opposed to "functional chlorophyll-a") were found at slope depths by Cahoon (1999), I would hesitate to interpret this report as evidence of cells making their living by photoautotrophy at those depths. Transport over the shelf break and down-slope seems the more likely explanation. Many diatoms seem capable of rather prolonged survival without light sufficient for photosynthesis.

Section 5. Conclusions and perspectives: Overall, good general points are made here. Perhaps a little more discussion on some of the specific findings would be appropriate (e.g., the Case 1 versus Case 2 results of Fig. 4, Tables 1, 8).

Technical Corrections.

Section 2.5 title - "Comparaison with estimatesĚ" => "Comparison with KPAR estimatesĚ"

pg 923 and References – "Richard et al." => "Richards et al."

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