Biogeosciences Discuss., 3, S671–S677, 2006
www.biogeosciences-discuss.net/3/S671/2006/

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.

J.-P. Gattuso et al.

Received and published: 27 October 2006

We thank James Nelson for his valuable comments and provide our reply below.

General comment

This manuscript indeed comprises two clearly defined parts (global irradiance and biological importance). However, we do not want to contribute to the so-called "salami-science" by unecessarily splitting manuscripts. However, we agree with James Nelson that one must ascertain that this is not at the expense of readability. The manuscript has been revised to make the outline clearer to the reader (last paragraph of the introduction and begining of sections 3 and 4.2).

Specific comments

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

  We believe that the original title better describes our study .

- While some of the suggestions regarding the structure of the manuscript make sense, we feel that implementing theses changes would entail too much work for a marginally significant return.

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

  Agreed. This was added in the revised version of the manuscript.

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

  The word "average" is meaningless in this context and was deleted.

- *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).*

  The land signal is removed from Level-3 data. The definition of proximal and distal piwles is now given in the legend of Fig. 3.

BGD

3, S671–S677, 2006

- *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.*

  The following definitions have been added to the manuscript:

  > There are several levels of SeaWiFS data, two of which were used in the present paper. Level-2 data are geophysical products such as the chlorophyll concentration or the diffuse attenuation coefficient, geographically referenced, and provided on a orbit per orbit (or scene by scene) basis, at the spatial resolution of the satellite sensor. Level-3 data are averages of individual Level-2 data on a spatial grid (e.g., 9 km global grid) and over a given time period (e.g., a month).

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

  Yes, the manuscript has been revised accordingly.

- *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.*

  See revised discussion.

- *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*

BGD

3, S671–S677, 2006

Interactive Comment

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?

The Weinberg (1976) formulation provded marginally better correlations and is the only one shown in the figure for the sake of clarity. The WO data are point data collected during field campaigns.

- *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).*

  We changed the structure of the manuscript several times and settled on the present one. We feel that the option proposed by James Nelson would be awkward. We think that the literature review must be in the discussion in order to avoid making too many pieces (some in the introduction and the reminder in the discussion).

- *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).*

  The original figure is fine but was greatly reduced by the production office. We will ask it to include a larger version in the final paper.

- *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*

BGD

3, S671–S677, 2006

Interactive Comment

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 surface area receiving a certain irradiance threshold is larger for Case 2 than for Case 1 waters because the former are shallower than the later (52 vs. 83 m; see Table 1). This has been added in the revised version of the manuscript.

- *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?*

  We agree. The legend of figure 4 has been modified accordingly.

- *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).*

  The average depth of pixels is shown and the negative depth convention is indeed not necessary. The table has been modified accordingly.

BGD

3, S671–S677, 2006

Interactive Comment

- *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).*

  Agreed. The manuscript has been modified accordingly and the table simplified as the light levels are not required here; they can be calculated with the equations provided in the legend of Fig. 4.

- *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.*

  The literature review was left in the discussion but an 'òverview" has been added, as suggested.

- *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?).*

  Indeed, the distribution of Australian seagrasses is used for validation purposes and to outline limitations. This is clearly mentioned in the revised version of the manuscript.

- *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*

BGD

3, S671–S677, 2006

Interactive Comment

the more likely explanation. Many diatoms seem capable of rather prolonged survival without light sufficient for photosynthesis.

We could not find any reference to the "record" depth in Cahoon (1999) and the report of which we are aware specifically mentions "apparently functional chlorophyll a". Hence the text was left unchanged. We believe that the text is very cautious about this "record" and want to emphasize that it was not used to delineate the potential distribution of microphytobenthos. The parameter used is the irradiance at which photosynthesis was detected rather than $E_{c\ \mathrm{growth}}$

- *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).*

  Agreed. this was done.

Specific comments

- *Section 2.5 title - "Comparaison with estimates" $\rightarrow$ "Comparison with K_{PAR} estimates"*

  OK, done.

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

  OK, done.

Interactive comment on Biogeosciences Discuss., 3, 895, 2006.