

***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.**

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We thank Rick Jahnke for his valuable comments and provide our reply below.

**General comments**

1. We agree that more irradiance data collected very near the sea floor would provide an opportunity to ground truth satellite-based estimates and have modified the abstract, discussion and conclusion accordingly.
2. We chose community compensation irradiance as the relevant light parameter

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to estimate the role of benthic community production in coastal ecosystems because the paper focuses on the autotrophic vs heterotrophic status. As indicated in the abstract and elsewhere in the manuscript, we consider net community production, rather than net primary production, because respiration of the autotrophs and heterotrophs cannot be disentangled in the database that we compiled. We agree that respiration can be partly sustained from allochthonous sources and, hence, that net primary production can be significant even though net community production is negative. We have carefully screened the manuscript in order to make sure that it is clear that the purpose of this work is NCP.

3. We also agree that the trophic status is subject to seasonal changes not only through the effect of changes in irradiance, but also changes in parameters such as temperature and concentrations of nutrients. As indicated in the conclusions, *The temporal scale was not addressed in this study despite its obvious importance*. Another sentence was added for the sake of clarity:

The temporal scale was not addressed in this study despite its obvious importance. At time scale smaller than one year, changes in irradiance and other parameters such as temperature and concentrations of nutrients control control community metabolism. At longer time scales, sediment delivery is affected by human activities in opposite...

### Specific comments

Note that the referee's comments refer to the line number of the submitted manuscript rather than those of the discussion paper.

- *line 110: I am not sure this method would consistently “overestimate” the diffuse attenuation coefficient since it can not incorporate vertical variations in turbidity such as might be caused by a bottom nepheloid layer.*

Agreed. This is in fact mentioned on line 144: *The impact on the computation of the diffuse attenuation coefficient is therefore unpredictable.* The statement on line 110 is incorrect and has been deleted.

- *line 238. The authors discuss the drawbacks of using annual average images and mention seasonal variations in chlorophyll...*

We disagree. This sentence is about light penetration, not about chlorophyll concentration. Nevertheless, parameters affecting light penetration have been added to section 4.1 of the revised manuscript.

- *line 249. The authors mention that among the impediments to accurate chlorophyll estimates from ocean color remote sensing is the presence of CDOM. Again, in coastal settings, many factors may impede the accuracy of remotely sensed estimates of chlorophyll...*

Agreed. The manuscript has been revised accordingly (see above).

## Technical corrections

- *line 76: Define and estimate impact of “registration error”.*

One of the authors who reported the registration error of the ETOPO2 data set (W. H. F. Smith) was contacted. Unfortunately, there seems to be no way to estimate its impact but provide additional information on this error in the revised version of the manuscript.

- *line 94. Edit, explain or remove reference to “David” etc.*

This comment was not in the discussion paper.

- *line 159: Should use a model II regression analysis since there is uncertainty in both variables.*

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Model II or geometric regressions were used (see legend of Fig. 2). Their calculation requires Model I regression parameters which are also given in the legend of figure 2.

- *lines 270- 275. I am not sure of the purpose of these lines since the Jahnke et al. reference explicitly relates benthic primary production to absolute light flux on a Ein m-2 hr-1 basis (see their Figure 5).*

They were used to illustrate previous estimates of benthic irradiance in some localities. Data shown in Fig. 5 of Jahnke et al. were not used because PAR is expressed per hour. Units per day are used throughout our study and we did not know how to convert units as the duration of the chamber deployment is not precisely reported (“6-8 h”; Jahnke et al., 2000 p. 113).

- *line 323. In this section, the authors conclude that Ec growth and Ec comm are the relevant irradiance-thresholds for benthic communities - see general comment #2.*

See reply above.

- *lines 644, 654 & 1000. “Richard” should be “Richards”*

OK.

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