

Biogeosciences Discuss., referee comment RC1  
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## **Comment on bg-2021-123**

Anonymous Referee #1

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Referee comment on "Biological production in two contrasted regions of the Mediterranean Sea during the oligotrophic period: an estimate based on the diel cycle of optical properties measured by BioGeoChemical-Argo profiling floats" by Marie Barbieux et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-123-RC1>, 2021

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**Review of ms entitled "Biological production in two contrasted regions of the Mediterranean Sea during the oligotrophic period: An estimate based on the diel cycle of optical properties measured by BGC-Argo profiling floats" by Barbieux et al.**

### GENERAL COMMENTS

This study is an original contribution to the field of bio-optical oceanography. Though the approach used in this study is not new, the ms provide a detailed analysis of a unique dataset that includes high-resolution vertical measurements of biogeochemical properties acquired in two ecoregions of the Mediterranean Sea, during summer oligotrophy.

The application of the cp-based approach to estimate particles production from such data provides new findings, that significantly improve our understanding of particles dynamics in oligotrophic areas. The ms successfully addresses the challenge of filling typical observation gaps in a traditionally under-sampled ocean.

The introduction clearly introduces the general context and specific objectives of the present study.

The M&M is detailed, sound and robust. The structure of the M&M is sometimes a bit confusing, and there is some overlap between sections 2.3 – 2.5 and section 2.6. I have two major methodological comments (see below comment #2), and would suggest to display more examples cp and bbp measurements (see below comment #1).

The Results and Discussion, which are merged in a single section, are nicely structured and generally well supported.

The conclusion is too long, and there is some degree of redundancy with the discussion. Synthesizing the conclusion to the main results and "take-home messages" would improve the article.

#### SPECIFIC COMMENTS

1) As the whole study is built on cp and bbp measurements, it is necessary to show the data (an appendix could be used for that purpose). A schematic representation of the cp diel variability (Figure 2) is useful to explain the method, and the example provided in Fig. 5 (Ionian Sea) is interesting, but this is not sufficient.

2) Here, I raise two methodological comments concerning section 2.3. "Characterization of the diel cycle of the bio-optical properties".

First comment: in the literature, the diel variability is generally defined as the change in cp between sunrise and sunset (Siegel et al., 1989; Cullen et al., 1992, etc.). Such daytime increase in cp has been previously associated to particle growth and production.

In the present study, the diel variability is computed as the relative variation between two sunrises (in the Ionian Sea, eq. 3) or two noons (in the Ligurian Sea). Did the authors also characterize the sunrise-to-sunset variability?

Second comment: the time reference used to characterize the daily changes in cp are not the same in the Ionian Sea (reference = sunrise) and in the Ligurian Sea (reference = noon).

This introduces a bias in the comparison between the results from both study sites, and should be discussed. Would it be possible to compute the diel variability using the same time reference (e.g. sunrise) in both cases?

The same two comments also apply to Section 2.6.3 "Calculation of the production rate". In eqs. 9-12, the authors explain how a daily and depth-integrated production of particles, P can be inferred from diel changes in cp. Is the variable "P" a proxy of the net community production (NCP) as defined in Claustre et al 2008? Or is it something different? It would be useful to add this precision in subsection 2.6.3. It is not clear whether the production

has been inferred using the day-to-day or the daytime (sunrise to sunset) change in the cp-derived POC.

How was the gross community production (GCP) estimated?

Here also, the time reference differs between the Ligurian and Ionian Sea (L343-345). How is it expected to influence the results? Would it be possible to standardize the method?

3) As the instruments deployed at BOUSSOLE provide high-frequency measurements, it would be very interesting to compare the surface cp time-series acquired by the fLig float with the BOUSSOLE data (accounting the lag of 2 days identified in L371-380). Such a comparison would be useful to assess if some information is missed by the lower (but still high) temporal resolution of the BGC Argo float, as well as to assess the spatial representativity of BOUSSOLE point-based measurements.

#### TECHNICAL COMMENTS

In the title, the term "optical properties" is maybe to general, and could be more detailed (e.g. beam attenuation coefficient).

The variables cp, bbp, Zeu, Zpd, etc. should be italicized throughout the ms

L167-175 how was the time period of the two floats selected?

L265 typo in "Loisel et al."

L287-300 and Table 1. The same cp-to-POC relationship is used in the Ligurian and Ionian Sea, despite the bio-optical differences between the two basins. Two different relationships have also been reported in the literature (Oubelkheir et al 2005, Loisel et al 2011). The authors decided to apply the results from Oubelkheir et al. (2005). Why not from Loisel et al (2011)? This could be added to the discussion in L502-508.

Figure1: it would be useful to display the summertime-averaged satellite-derived Chl concentration in Figure 1

Figure 2: adding grey vertical bar to represent nighttime would help reading the graph

Figure 3: I suggest to add "Ligurian Sea" and "Ionian Sea" at the top of the left and right panels, respectively (and same comment for Figures 4, 6 – 10). In Figure 3, displaying the cp-to-Chl ratio would also be useful to help reading section 3.1 (in particular L389 - 408)

L482-485 the results also compare with the "delta POC" estimates reported in Gernez et al. (2011; see their fig. 14)

L539-542 Here the authors assumed that negative values could be associated with particles transport. Please note that negative values could also occur if the losses exceed particles growth (which could occur if the community is dominated by heterotrophs)

L543-549 this is very interesting. It would be useful to provide the averaged cp/Chl (at the depth of the SCM) of the Ligurian Sea and Ionian Sea. Besides photo-acclimation, are there other ecophysiological hypothesis that would be consistent with this hypothesis? (e.g. composition and size distribution of the community of living particles, higher influence of nutrient stress in the Ionian Sea?)

L607-623 when discussing the range of variability of cp/Chl, please consider referring to Loisel & Morel (1998)

L653 "it appears to result from changes in light conditions": light and/or nutrients?

L679-689 as acknowledged by the authors, these are very hypothetical statements that are not supported by the present study. I therefore suggest to remove the quantitative results of such crude estimations from the conclusion (i.e. remove or re-write L745-747)