

## ***Interactive comment on “Dissolved and particulate primary production along a longitudinal gradient in the Mediterranean Sea” by D. C. López-Sandoval et al.***

**Anonymous Referee #1**

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This manuscript presents information on particulate and dissolved primary production in the Algero-provençal, Ionian and Levantine regions of the Mediterranean Sea. The topic is interesting, since there are few previous measurements of dissolved primary production in the Mediterranean, although it does not seem to be appropriate to qualify studies such as those in the Almeria-Oran Front of the Alboran Sea as "local observations, mostly in coastal waters . . ." (page 8594, lines 17-19). The conclusions are plausible, but some important aspects of the methodology need to be adequately addressed. Some specific comments follow.

The incubation period for the primary production experiments was 24 hours. During

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this time, as the authors recognize, heterotrophic consumption of released DOC might have occurred. For example, Fernández et al. (1994) reported that bacterial assimilation of excreted material ranged from 21 to 78% of the total net release during a 6 h incubation period. Although the methodology used by Fernández et al. (1994) has its shortcomings, this (and other works) indicate that bacterial uptake of released DOC may be important. In addition, there are other processes that could affect the results in a long incubation. In this context, the interpretation of the end point DOC<sub>p</sub> values reported in the present manuscript and their comparison with bacterial production rates need a much deeper and more detailed consideration than simply stating that the DOC results "must be regarded as net production rates" (lines 14-15 of page 8596).

Given its importance in the context of this manuscript, it would be helpful to point out potential limitations and discuss alternatives to the models used to calculate bacterial respiration.

The DCM Chl *a* data in Table 1 shows the west to east gradient in oligotrophy, but also high variability within the three regions studied. Perhaps the authors could consider and discuss additional indicators of trophic situation. The paragraph in lines 13-19 of Page 8600 states that there was no relationship between PER and taxonomic composition of phytoplankton. It would be helpful here to indicate what phytoplankton groups were considered.

The BOUM and the Celtic Sea data in Fig. 6 do not seem to belong to the same regression line. Could the authors discuss this point?

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