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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 #2

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General: The authors investigated the variability in the dissolved and particulate organic carbon production (DOC_p and POC_p, respectively) and the relative contribution of DOC_p to total primary production, along an east-west transect in the Mediterranean Sea. They also explore the coupling between DOC_p and bacterial production. They conclude that DOC_p is a relatively constant fraction of total primary production along the transect, irrespective of the phototrophic community composition. They find that bacterial carbon demand exceeded the DOC_p and hence heterotrophic bacteria would rely on additional sources of C to meet their carbon requirements and there seem to be only a “small degree” of coupling between DOC_p and “bacterial metabolism”.

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For the most part this is a well written paper. This work may not be novel in the sense of measurements and results, but is complementary to previous work by Lopez-Sandoval et al., and was conducted in previously under-sampled region with regard to DOC production. As such it is a valuable contribution to the increasing knowledge of phytoplankton and bacterial metabolism in oligotrophic marine environments.

Specific comments: I believe the authors have ignored some important factors in their assessment of their results. The most significant, perhaps, is the lack of acknowledgement, up front, that DOC_p, as measured by ¹⁴C-DOC released from phytoplankton, will not only represent exudation of photosynthate from healthy cells, but will include DOC released as a results of cell death and disintegration through grazing and viral lysis for example. How is that distinguished from exudates using this protocol? The other main point is the statement that bacterial carbon demand (BCD) seems to exceed the DOC_p. However, DOC_p here is a net measurement, after 24 hours incubation. This would affect the estimated rate of DOC_p due to consumption of DOC_p by, presumably, heterotrophic bacteria. Have you ever looked at the DOC_p just after the end of the light period or in time course incubations to assess this potential loss of DOC_p? Also, there is no mentioning of the size of the ambient DOC pool as reference to BCD. What turnover times would that pool have (I presume long)? If the DOC exudate, as suggested, is of low quality in P-limited environments, would that not increase BCD, i.e. increase respiration, and decrease BGE, and hence draw down the more of the DOC pool?

On the algorithms to compute BGE (equations 1 and 2): it would be welcomed if a brief explanation was to be added as to what the different factors are in these. For example what does the 1.8 represent in eq 1? Also, what limitations are associated with these two approaches?

P 8601, In 10. What was the relative contribution of the larger phytoplankton components? It is mentioned that community composition did not significantly change the PER. However, there is no data presented here on the size, or taxon, distribution of

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the phytoplankton community along the transect. Was there any data collected on size fractionated primary production?

Other comments:

Throughout the manuscript the wording “importance” of DOCp is used, without a qualifier as to what it is important. I suggest changing this to “contribution” or “relative fraction” as “importance” seems to be used in reference to the relative amount of DOCp to POCp.

I would have welcomed a more thorough materials and methods section (despite the reference to Moutin et al.). This is especially so for data that is used in the manuscript, such as chlorophyll.

P 8595, ln 10: suggest adding - For a full description. . .see Moutin et al. (2010). ln 13: were the 6 depths determined by the light level, or were they fixed depths? ln 20: did you observe temperature effects when using on-deck incubations vs in situ, given that the on-deck incubators were cooled with surface seawater only?

P 8596, ln 5: what brand name was the cocktail? ln 15: suggest stressing here that DOCp is a net measurement and that a portion of it most likely is consumed, especially during the dark period when new production should be limited.

P 8597, ln 6: “. . .close to detection limit” . . .by what method? ln 17: “. . .dominated mostly by.” suggest omitting “mostly” here.

P 8598, ln 1-4: Are there any error estimates in the integrated production values ? (Your samples were in triplicates for each depth, so there should be data). ln 10: change “tree” to “three” ln 10-12: Also, why is approximate values (~) used sometimes, where “precise” values are used elsewhere when describing ranges? ln 13: change “tend” to “tended” ln 17-18: suggest rewording to “. . .the fraction of DOCp to total primary production was relatively constant throughout the study area.

P 8600, ln 1. Suggest rewording to” The consistency in the relative contribution of

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DOCp.. Ln 3: suggest changing “taking into account all the sampled stations” to “across all stations” Ln 15-20: How do you distinguish the DOCp derived from exudates or through cell death via grazing, viral lysis etc?

P 8602, ln 3: suggest rewording “..has resulted in that during most field studies DOCp measurements are not routinely..” to “..has resulted in that routine DOCp measurements are not carried out.” Ln 19: change “analized” to “analyzed”

P 8604, ln18: change “Hangström et al.” to “Hagström et al.”

Interactive comment on Biogeosciences Discuss., 7, 8591, 2010.

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