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## ***Interactive comment on* “The effects of nutrient additions on particulate and dissolved primary production in surface waters of three Mediterranean eddies” by A. Lagaria et al.**

### **Anonymous Referee #1**

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#### General

Lagaria and co-workers report in this new BOUM manuscript the effect of inorganic nutrient additions on the partitioning of primary production into the particulate and dissolved fractions. It is still quite infrequent to include the latter, potentially important carbon flux, especially in oligotrophic environments such as open Mediterranean waters. I would highlight two interesting results. First, their initial percent extracellular release (PER) values were relatively low (9–18%) and not significantly different between sites that were expected to differ, at least according to their location along the well-described east-west gradient of increasing oligotrophy in the Mediterranean. By

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the way, this gradient may well be general but it was not evident at all in this study (opposite results in Table 1 are worth of further explanation). Secondly, they found strong evidence against the claimed P-limitation in the Mediterranean (work by Thingstad and colleagues). The addition of phosphorus alone did not enhance primary production or decrease PER values as hypothesized by, among others, Obernosterer and Herndl 1995, MEPS 115: 247-257, please consider this paper in your study). However, in my opinion none of these two findings is sufficiently discussed. This is my first requirement of any subsequent revision.

Carbon requirements of heterotrophic prokaryotes were only (very) roughly estimated and this should be clearly stated in the abstract and elsewhere in the text. I may agree that bacterial respiration (BR) should lie between 50% and 100% of total community respiration (CR) but this is certainly too large a range so as to derive sound conclusions. I can envision that statistics are difficult to apply to Table 5 data for the aforementioned reasons, but the authors should then be much more cautious when making statements of the relationships between the (assumed but not demonstrated) degree in oligotrophy and BCD:PP ratios in the +P treatments.

More importantly, the experimental design to estimate dissolved primary production (PPd) is slightly flawed. Unlike particulate primary production (PPp), PPd was only measured in one of the triplicate microcosms and the authors are surely aware that PPd is usually much more variable than PPp. In my opinion, this methodological constraint importantly affects all subsequent analysis. Also, the fact that control PPd and PER values at station A were below “detection limits” (sic) compromises any comparison between sites when total number of experiments were 3! Similarly, the large errors (standard deviations) associated with O<sub>2</sub> measurements (Table 2) preclude drawing significant conclusions about differences between sites.

Stating that 10-20% PER values “closely approximated” 30% (López-Sandoval et al. 2010) is largely missing the point. Please re-write and avoid ambiguous statements such “(PER values in the microcosms) . . . were reasonable”. Also, López-Sandoval re-

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sults from the same cruise are exactly the opposite to Lagaria and colleagues' Fig. 3, i.e. PER was constant (mean 37% rather than 30%, see above) along the west-east productivity gradient. The authors should discuss this discrepancy rather than only using supporting references. Since both papers are to appear in the same Biogeosciences special volume, the authors must carefully consider the paper by López-Sandoval et al. and discuss the serious discrepancies accordingly.

The afore-mentioned concerns need to be carefully addressed before considering the possibility of resubmission and final publication in the BOUM special volume. In conclusion the paper is not acceptable in its present version.

### Specific

Please include “inorganic” before nitrogen and phosphorus in the abstract.

The metabolic rates of the osmotrophic community as defined by the authors (phytoplankton plus heterotrophic prokaryotes) were not directly measured. They estimated total respiration, thus including the contribution of other heterotrophs (heterotrophic nanoflagellates, ciliates, larger metazooplankton?).

Some indications on the depth of the experiments or whether their analysis of the relationships between PPD and PPp was performed with volumetric or areal units is needed in the abstract. Regarding the latter issue I suggest the authors read carefully the papers by Marañón and co-workers (2004 L&O, 2005 MEPS), and use them in the discussion of their own results. Please see also my next comment of a companion BOUM paper (López-Sandoval et al. 2010 Biogeosciences Discussions).

Why do the authors use gross community production (GCP) rather than the more common term gross primary production (GPP)? If there was no other oxygenic phototroph in their water samples rather than phytoplankton I believe the correct term is the latter. In any case, please discuss in Material and Methods your choice.

It is not exactly true that planktonic microbes [what do they mean exactly, heterotrophic

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prokaryotes (bacteria) or microzooplankton?] make up consistently >50% of total respiration. Please check Robinson (2008) chapter in Kirchman's book Microbial Ecology of the Ocean (2nd edition) for values below 50%.

The fact that GPP is derived from NCP and CR estimates seriously compromises any consideration about the relative importance of GPP (GCP) or CR in driving NCP values. Unfortunately, this is quite common in ecosystem metabolism (i.e. O<sub>2</sub> fluxes) studies, but the authors should consider it explicitly.

A more exhaustive literature review on the factors than may affect primary production partitioning into P<sub>PP</sub> and P<sub>Pd</sub> would be appreciated.

The authors apparently follow the paper by Morán et al. (2002) dealing with phytoplankton-bacterioplankton coupling when considering the role of P<sub>Pd</sub> and P<sub>PP</sub> in meeting bacterial carbon demand (BCD, please use lower case for the full words). However, their suggestion of comparing total rather than dissolved primary production with BCD differs from what the aforementioned authors use. This should be detailed in the introduction and/or discussion and justified. Do the authors imply that bacteria (and only bacteria) are able to use all primary production concurrently produced in their experiments?

The authors should revise their text for unnecessary verbosity at some parts (e.g. "it is now generally recognized", "needs further to be investigated", etc. ) as well as the repetition of results in the discussion section.

Why are there 24 data points in the figure? Assuming that control time final at station A was lost I would have expected 26 measurements (11+12+3 initial conditions).

Table 2 should include some indication of significant differences between sites.

Tables 3 and 5. Please state that these are mean ratios and provide significant differences if any.

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