

Interactive comment on “Patterns in planktonic metabolism in the Mediterranean Sea” by A. Regaudie-de-Gioux et al.

Anonymous Referee #1

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This manuscript presents data of planktonic metabolism (gross primary production, community respiration, net community production) in the Mediterranean Sea based on bottle incubations (on deck) during 2 cruises conducted in June 2006 and June 2007 (West-East transect, 7 and 14 stations in 2006 and 2007 respectively, 3 depths). What do the authors want to tell us with this manuscript (last paragraph of the discussion): the Mediterranean Sea is heterotrophic and a source of CO₂ to the atmosphere. Because heterotrophic, the Mediterranean needs organic matter from land or from the atmosphere. Therefore, regulations of nutrient inputs (rivers, atmosphere) and increases of human population on the coastline will affect the carbon budget of this region.

My general evaluation: I do not recommend this manuscript for publication in Biogeosciences. Although well written and presented, the study could be interesting in its

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objectives (west-east transect) but the dataset is way too restricted (only metabolic rates and chlorophyll data are presented here) to be of interest. It would be more appropriate to see this dataset integrated in a manuscript combining a comprehensive set of data acquired during these cruises (nutrient, organic matter, bacterial activities, etc..). Moreover and this is the most important point, the conclusions of this article are absolutely not supported by the dataset. To conclude on the metabolic rate of the Mediterranean Sea (on a yearly basis) based on 2 cruises conducted in June is very ambitious but wrong! Based on your dataset, you can tell that the Mediterranean was heterotrophic during that period of the year (what you actually say in the abstract) but you can surely not extrapolate. Therefore, the conclusion is: The Mediterranean is heterotrophic in June, during a post-bloom period (P8575, L13-14). Although this is a result, and as such of a certain interest, this is not a big surprise... The authors try to validate their results (heterotrophic behavior) by mentioning that they are consistent with pCO₂ data showing that the Mediterranean was a source of CO₂ for the atmosphere at least in 2006. There is some literature on the behavior of the Mediterranean with respect to CO₂ (source or sink). One of them is the study of Copin-Montegut et al. (2004) at the DYFAMED site showing that this region of the Mediterranean in a sink of CO₂ for the atmosphere and as such, following the present authors, should be autotrophic on a yearly basis. Another one (D’Ortenzio et al. 2008) is based on a modeling exercise for the whole Mediterranean and also shows that it acts as a slight sink of CO₂ for the atmosphere. Considering Copin-Montegut et al. (2004), it is showed that pCO₂ is the highest in summer, but by normalizing to a constant temperature, they also show that this is mostly due to a thermal effect (pCO₂ increases with temperature) and not to a biological effect. Finally, the authors compare the rates they measured to published ones in the Eastern and Western basins, as shown in Fig. 1, most of them being coastal. It is already hard to compare coastal sites to open sea sites but it clearly does not make sense to compare areal rates if you compare 2 sites with a difference of 50 m in the water column depth. At least, compare volumetric rates.

Specific comments:

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Material & Methods

P8572, L18, 19: Please detail more the protocol for the incubation of the bottles on deck at the right irradiance. Did you measure the irradiance? If yes, please mention it and which material you used. Did you measure the light during the incubations on deck? From my experience, I know that shade is something you easily find on the deck of a ship.

P8572, L24: Why did you choose a Q10 value for Antarctic plankton (i.e. Robinson & Williams, 1993). There has been some work done on temperate plankton assemblages (Lefevre et al. 1994). I would expect heterotrophic bacteria to be more sensitive to temperature increase than phytoplankton limited by both light and nutrients (post-bloom period). Please comment and justify your choice.

Table 3

Again, compare what is comparable. In this table you compare studies made on a yearly basis, studies at shallow coastal sites and yours made in June and for a relatively deep layer. This does not make sense. Also, please explain your choice of a 1.25 ratio between O₂ and C, or at least provide a reference.

Fig. 1

Numbers 7 and 8 have been switched.

Literature

Copin-Montegut C, Bégovic M, Merlivat L (2004) Variability of the partial pressure of CO₂ on diel to annual time scales in the Northwestern Mediterranean Sea. *Marine Chemistry* 85:169-189

D'Ortenzio F, Antoine D, Marullo S (2008) Satellite-driven modeling of the upper ocean mixed layer and air-sea CO₂ flux in the Mediterranean Sea. *Deep-Sea Research Part I-Oceanographic Research Papers* 55:405-434

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Lefevre, D., T. L. Bentley, C. Robinson, S. P. Blight, and P. J. L. Williams. 1994. The temperature response of gross and net community production and respiration in time-varying assemblages of temperate marine micro-plankton. *Journal of Experimental Marine Biology and Ecology* 184: 201-215.

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