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Biogeosciences Discussions

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

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General The reviews received have pointed out some areas that need improvement, particularly as to increasing the precision of the conclusions and acknowledging limitations of the study due to the narrow time window when the two cruises were conducted. We believe these criticisms are appropriate and have revised the manuscript to address them, resulting in a stronger manuscript where all claims are substantiated by the results presented.

Action taken to accommodate Reviewer#2 The referee thinks that there are some general aspects that can improve the manuscript as following

Reviewer#2- The authors have only sampled 6 days in May 2006 and 14 days in June

2007. Therefore I consider incorrect and too ambitious the conclusions of the authors saying, that the net heterotrophy nature of the studied section of the Mediterranean Sea, acting as a CO2 source. Therefore, I suggest to change the conclusion should be changed taking in to account the lack of seasonality in the current study.

Response: We agree that our dataset is too restricted to conclude that Mediterranean Sea is heterotrophic on a yearly basis and that it is better restrict the conclusions for early summer, when data are taken.

Action: We will make explicit throughout the text the limitations of the study, indicating always that the conclusions refer to late spring and early summer. This should avoid misunderstanding.

Reviewer#2- There appears to be missing a statistical section in material and methods. I imagine that the test-t used to compare the GPP during the thresholds 2006 and 2007 is notpaired but this is not mentioned in the text. In addition, I think it is incorrect to use this test (which is an ANOVA – one way) because the data are not balanced (14 data vs 7 data), also the homogeneity of variances is not mentioned. I would suggest in order toachieve the spatial variability of planktonic metabolism in oligotrophic Mediterranean ecosystems an alternative statistical analysis as a multivariate ordination analysis. This analyse will let you to study the variation of all physiological parameters together. I think the authors could develop and expand more the results presented and therefore will enrich the manuscript.

Response: We agree. We will add details on the statistical analyses used in the manuscript in the revised methods section; will also provide additional supporting variables (e.g. DOC, nutrient concentrations and bacterial abundance), and will explore these relationships (although the exploratory analyses we have conducted shows that there are no robust relationships with metabolic rates), as suggested, and will explore multivariate techniques in an attempt to examine whether they reveals new features of the results. However, multivariate techniques are useful for exploratory purposes and not well suited to test specific hypotheses, for which we will use more conventional, bivariate methods.

Reviewer#2- Page 8574, line 16. Regarding to "spurious" correlations, already it has been lot of debate and controversy about their uses (Kenney 1982, Prairie and Bird 1989, Jacksonand Somers 1991, Berges 1997). Therefore, I think it should be used with caution mainly when the r2 is very low. Thus, I believe there is not correlation between GPP and R, this observation is very interesting from the fact that you conclude the allochthonous carbon is an important source to subsidise planktonic metabolism in the Mediterranea Sea. Your data shows that respiration is more constant than production and the implication is that there must be important reservoirs of substrates to sustain the respiration. In addition it is much more difficult to sample production rates than respiration rates because it is much easier to miss occasional burst of production in nature. What happen if you grouped the samples by NCP (autotrophic vs heterotrophic) and make and ANOVA of the GPP? Then, if there are significantly differences, what the authors confirm will be true and communities will tend to be net heterotrophic at low GPP and net autotrophic at high GPP

Response: We are well aware of the spurious correlation debate (Prairie and Bird were co-workers of Duarte, at the same lab when their seminal paper was written). The "spurious" correlation problem does not mean that the correlation between a ratio and a component of the ratio is an artefact, but simply that the null hypothesis is no longer than the slope = 0. However, comparisons between the P/R ratio and either P or R are informative, as they can inform, for instance, of the magnitude of GPP for P/R to equal 1, as we do in the text. We agree that the R2 is low (< 0.4), and that inferences from this relationship should be considered with caution. We will group, as you correctly suggested, the samples by NCP autotrophic or heterotrophic and make an ANOVA of the GPP. As we will report (we did this analysis already), there are indeed significant differences in GPP between autotrophic and heterotrophic stations, once the major differences in GPP among cruise are considered. This will provide further support to

the inferences based on the regression analysis, without the caveats that may result from use of a ratio.

Reviewer#2- The slope of the r.m.a. regression (Figure 3) is extremely low and does not correspond with the solid line shown in Figure 3 that seems to be close to one.

Response: Thank you for highlights this point. Somehow a typo was introduced and the slope reported was in error and indeed did not correspond to the actual slope. We will make the necessary corrections to the figure legend.

Reviewer#2- I do not consider that the incubations were carried out at in situ temperatures, when samples from 40m and 120m are incubated with water from 5m, even if the error of the estimates are considered and not corrected. Therefore this paragraph should be modified. The authors should also indicate when the samples were taken e.g. before sunrise to avoid the photo shock on the samples?

Response: We agree and will explain better the incubation temperatures, and indicate that the samples were incubated at 5 m depth temperature, which is more precise. We did not sample the seawater before the sunrise, but to avoid any photo chock, we protected the sampled seawater by a dark screen avoiding exposure to solar irradiance before the incubation.

Reviewer#2- Figure 3 is very difficult to visualise, a better alternative would be a table with the data.

Response: We explored using a table and encountered that it would be far too large. We, however, agree that the data cannot be well assessed from the figure. Hence, in a revised version we will (a) use colour (free of charge in BGS) to enhanced and improve visibility of the elements of the figure; and (b) add, as supplementary material (since it will be too large to fit in the manuscript), the table with the data.

Reviewer#2- Page 8575 line 20. Turley observed significant differences between West and east p<0.05 and, although the authors observed similar trends to Turley, p were

>0.05. This paragraph should be revised.

Response: Thank you for the comments. The changes have been done.

Reviewer#2- The authors should actualise the bibliography, there is just one article of 2008 which is still in press??. I know there are more articles about the Mediterranean more recent already in press (e.g. González et al).

Response: We agree. We have done a new search of the published literature to update the data and have included the González et al. (2008) paper, which is the only one reporting metabolic rates of the Mediterranean not included in our manuscript.

Interactive comment on Biogeosciences Discuss., 6, 8569, 2009.