Biogeosciences Discuss., 6, C2445–C2447, 2009
www.biogeosciences-discuss.net/6/C2445/2009/

Biogeosciences Discussions

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

Anonymous Referee #2

Received and published: 8 October 2009

The manuscript presented by the authors is interesting with regard to the lack of metabolism data predominantly in open waters and also in the Eastern basin. However there are some general aspects that I consider would improve the manuscript.

1. 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.

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 not paired 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.

3. 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, Jackson and 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 is 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.

4. 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.

5. 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?

6. Figure 3 is very difficult to visualise, a better alternative would be a table with the data.

7. 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.

8. 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).

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