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Biogeosciences
Discussions

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

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 Reviewer1

We believe the reviewer is a little too demanding from this study, as not only we are expected to encompass the entire Mediterranean Sea, but we are also expected to do so along the year. It is impossible to have it both ways: we have published in the past the longest, to the best of our knowledge, time series on community metabolism (6 years, weekly sampling), but this referred to a single, coastal location (Duarte et al. 2004). In our opinion, the two cruises we conducted across the Mediterranean are unique in their coverage of the basins compared to any other precedent studies.

Reviewer1: 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..).

Comment: We agree.

Action: We propose to include data on nutrient concentrations, DOC, and bacterial abundance in the manuscript (revised Table 1) and explore possible patterns between metabolic rates and these properties. Unfortunately, bacterial production is available for a single cruise, and thus cannot be explored fully.

Reviewer1: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

Comment: 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. However, we never concluded that our results were relevant on a year basis, as the reviewer apparently understood.

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.

Reviewer1:The authors try to validate their results (heterotrophic behaviour) by men-

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tioning 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 behaviour 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.

Comment: The aim of comparing with pCO2 data is not to validate our results, since we are aware that pCO2 is the product of many factors, of which pelagic metabolism is but one. The aim was to examine whether the metabolic balance of the community was or was not consistent with pCO2. Indeed, our results showed consistency only for one of the cruises (2006, when 22 stations from 25 sampled showed a pCO2 flux going from the ocean to the atmosphere, i.e. the 88

Action: We now refer to D'Ortenzio et al. (2008), point out the consistency between our pCO2 assessments and the model they presented, and then use the conclusion of D'Ortenzio et al. that pCO2 is dominated by temperature variations to account the lack of consistency between the prevalence of heterotrophy in the 2007 cruise and the prevalence of undersaturation of pCO2 in that cruise (spring). We also refer to the analysis of seasonal data of Copin-Montegut et al. (2004) – in fact the same data used for model validation by D'Ortenzio et al. (2008), shows that changes in pCO2 along the spring-summer transition, the period covered by our study, are likely to be under temperature control that the pCO2 at sea surface during the early summer across the Mediterranean Sea is higher than pCO2 atmospheric. Copin-Montégut et al. (2008) observed that the annual pCO2 at sea surface, in DYFAMED site, corrected by the

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temperature effect, is lower than the pCO2 atmospheric, acting as sink for atmospheric CO2, but the annual flux strongly differ from 1 year to another. This can explain why different CO2 fluxes between sea surface and the atmosphere were observed between THRESHOLDS 2006 and 2007.

Reviewer1:- Does not make sense to compare published rates in the Eastern and Western Basins, being coastal stations.

Comment: There is confusion in the reviewer's appreciation: We tabulated all data available for the Mediterranean, dominated by coastal stations, but we used our own data on open water stations to formally (i.e. statistically) compare the metabolism between the Eastern and Western basins (figure 5). Therefore, no comparison between basins has been based on coastal stations.

Specific comments

Reviewer1: 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.

Comment: We measure the irradiance (PAR data) in the water column from surface to 200 m, to determine the different percentage of light reaching the sampled depth and adjust the incident natural irradiance to that received Winkler bottles in situ using neutral density screens. Satlantic OCP-100FF irradiance profiler measured the irradiance in the water column. We did not measure the light during the incubations on deck, as the irradiance was control to a percent of the incident irradiance, but placed the incubations on the highest deck, away from any shading due to ship structures. This information will be provided in a revised version of the manuscript.

Reviewer1: 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.

Comment: This appreciation is also in error. We did not use a Q10 value for Antarctic plankton to correct our data. We used the equation of the activation energy from Geider (1988) to determine the Q10 of the respiration and net production rates. The reference to Robinson Williams (1993) was to how to make these corrections, not to the specific Q10 value used. We will avoid this confusion by removing the reference to Robinson Williams (1993).

Reviewer1: 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.

Comment: We provide a compilation of estimates on pelagic metabolism for the Mediterranean so that the reader can evaluate the state of the art and therefore be able to assess the progress contributed by the data we report. The figure and table clearly show that most assessments in the past derive from shallow coastal stations. The points we make from this compilation are that most rates show negative NCP and that the rates derived in our cruises were somewhat higher. We agree that the comparisons between integrated rates we derived and those from shallow stations are misleading, and will remove that comparison from the revised text.

Reviewer1: Table 3 please explain your choice of a 1.25 ratio between O2 and C, or at least provide a reference. Comment: 1.25 ratio between O2 and C was taking from Williams et al. (1979). Action: we include this reference in the table legend.

Reviewer1: Figure 1. Numbers 7 and 8 have been switched.

Comment: The reviewer is correct. Action: We will correct the numbers.

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