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Discussions

Interactive comment on "Rates and drivers of Red Sea plankton community metabolism" *by* Daffne C. López-Sandoval et al.

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Reviewer 2 general comments: The authors quantified plankton metabolic rates along the Red Sea. They have shown that Chla and planton community metabolism (GPP and CR) increase with temperature. Contrary to previous results they have observed a higher Activation Energy for GPP than for CR showing a positive relationship between NCP and Temperature. These results have been explained by the authors as a consequence of the high nutrient availability in warmer waters and the lack of external organic carbon sources to sustain a heterotrophic metabolism constraining the CR. The dataset are very interesting and merit been published, however, the way how the results have been presented, the lack of statistical analyses and the methodology proposed are not the most suitable to achieve the main goal proposed in the manuscript.

Therefore, I consider the ms still needs major revision in order to be published and providing the authors follow the reviewers recommendations.

Reply to reviewer general comments:

Thank you for your comments and suggestions. We have addressed the changes and recommendations of the reviewer and in the following section is a detailed answer to each of the points made by R2. First, we totally agree with the reviewer that a detailed description of the statistical analyses performed was missing in the methodology section. We have included a detailed description of the statistical analyses in a new section (2.4), and this change can be tracked now between lines 176–189 in the latest version of the manuscript. Regarding the primary concern of the reviewer 2, which was the methodological approach we used to quantify planktonic metabolic rates, we think, as explained to R1, there is a misunderstanding. The methodology used to quantify planktonic metabolism is based on the extensively used dark and light method (in combination with the Winkler titration method). The reviewer indicated that the methodology used was not suitable, and suggested that a shorter incubation period (6 –12 h) was more appropriate to quantify NCP. We want to point out that NCP represents the organic matter remaining after consumption of the GPP through respiration by plants (autotrophs), microbes (either autotrophs or heterotrophs), and animals (heterotrophs) (Ducklow and Doney 2013), and to account those process, the standard incubation time for in vitro incubations is 24 h. This incubation length is needed because contrary to photosynthesis that can be resolved during daylight, the losses due to respiration (which are necessary to define NCP) also occurs at night.

Reply to specific comments:

Reviewer Comments (RC) Author Reply (AC)

RC1: First, according to the title and the abstract the authors consider as drivers of the plankton community metabolism in the Red Sea, the Chla and temperature. However, other important parameters such as, temporal and spatial variability, salinity and nutri-

ents seem to govern the plankton community metabolism within this particular ecosystem and are not included in the abstract. Therefore, this lack of agreement between the ms, the consclusion and the abstract. is confusing. In my opinion, there is a large floor in the experimental design proposed and it is difficult to resolve.

AC1: We appreciate the reviewers' comment, and agree that the abstract highlighted our main findings and did not detail all the results. The abstract was indeed mostly oriented to the effect of temperature and nutrients availability on metabolic rates as we found that those were main controlling drivers. That was consistently explained on our results, discussion and conclusion, therefore we do not find disagreement in our statements.

RC2: All samples included the deepest ones have been incubated on deck with surface water. During some of the surveys there is an important thermal variability. The authors have attempted to mitigate the issue by including just those samples above the thermocline. However, Material and Methods mention that changes in temperature and PAR in the incubation tanks were recorded with HOBO data loggers. Therefore, those data should be shown in a table in order to select objectively the samples for the analyses. Hence, eliminating those samples that register thermal differences above 2_C with the in situ temperatures. In addition, samples adapted to cool temperatures such as those at the bottom will respond more drastically to artificial increments of temperature than surface ones (for example. Apple et al. 2006. AME. 43: 243–254) resulting in erroneous conclusions. Therefore, Figure A1 is important and should be included in the Ms.

AC2: Thank you for your comment and reference, we have moved Figure A1 into the text results section and now is presented as the main figure.

RC3: Other figures such as 4-6 do not show crucial information in the current format. Figure 3 and Table 3 to me are redundant.

AC3: We moved figures 4–6 as supplementary information. Figure 3 and table 3 are

complementary as table 3 indicates the correlation between metabolic rates environmental variables.

RC4: The paragraph 10-15 page 6 the authors should indicate if samples were collected before sunrise (to avoid any light on the samples) and if the incubation started at the sunrise to estimate the full light period. The authors say, the samples were colleted between 7 to 9 and to me this sounds very late to incubate and obtain the full light period nor precisely.

AC4: Samples were incubated for 24-h covering an entire light-dark period.

RC5: In The net community metabolism..... page 7, NCP should be estimated during the light period (NCP 6 to 12 hours).

AC5: We believe that there is a misunderstanding regarding the process we measured. The reviewer comment seemed to suggest that our work was focused on primary production, which is performed by the photosynthetic components of the plankton community during the daytime and that has gross and net components (as phytoplankton excrete and respire carbon). However, our paper focuses on the entire plankton community, both photosynthetic and heterotrophic (e.g. bacteria), where the net community production (NCP) represents the organic matter remaining after consumption of the GPP through respiration by plants (autotrophs), microbes (either autotrophs or heterotrophs), and animals (heterotrophs) (Ducklow and Doney 2013). Studies that focus on the photosynthetic component of the plankton community (e.g. Net Primary Production, NPP) report values for the daylight period only. Whereas studies, such as this, report (24 h) rates. For instance, published synthesis of community metabolism rates report values per day (24 h), e.g., Robinson and Williams 2005, Regaudie-de-Gioux 2012, 2013). The use of 24 h to report rates is justified as the metabolic budget need be resolved over 24 h to be completed, for photosynthesis this does not proceed at night, but respiration, which is necessary to define net community production, occurs at night as well.

RC6: The authors should show, the variation coefficient of the pool data and also the original CR, NCP and GPP data including their SE.

AC6: We already presented the mean and SE of our metabolism measurements in Table 3, and now we have included CV.

RC7: Because, In these oligrotrophic areas the metabolic rates are very low and can be difficult to detect. Therefore, the methodology needs to be very precise in the processes of filling, incubating and fixing the bottles.

AC7: We agree with the reviewer, however, the information about the filling and all special cares during the sampling are already detailed in methods section 2.3. Now between lines 136–155.

RC8: The paragraph 20 in page 8 It should be indicated the Arrhenius plots the authors mention.

AC8: The Arrhenius plots described in the methods section 2.3 (P8, between lines 13 and 20) where already shown and explained in the results section 3.3, when we described the response of planktonic metabolism and temperature. Now between lines 305–315.

RC9: The paragraph 10 in page 10 should be transfered from the Results to the Discussion.

AC9: The sentence in P10, between lines 8–10 is a closing statement with the main results shown in previous paragraphs, and there we are not discussing any results. Therefore, we prefer to keep it as it is.

RC10: And also the first paragraph of the 3.2 Variability of plankton : : :. Is already mentioned in M and M.

AC10: We modified the text as suggested

RC11: The paragraph 10 in page 13 There are lots of references within oligotrophic

areas very intersting and different to the authors ones that the authors should also include in the MS.

Thank you for pointing this out, we added new relevant references.

RC12: Figure 1. The name of the KAUST is excessive. I would use just one larger map with different colours or shapes to show the stations at each survey or season.

AC12: We decided to not modify the figure as some of the stations are sampled on the same location more than twice, and different shapes or points will be overlapped.

RC13: Figure 2. I consider in this figure is difficult to detect the thermocline and the vertical profiles of Chla and salinity. I consider that nutrient profiles should also included

AC13: It is not possible to determine the depth of the thermocline in Figure 2, and it is not intended to do so. The figure summarises the main characteristics of water column properties at different optical depths. Perhaps the reviewer meant figure 1A?. If so, nitrate+nitrate concentration is plotted.

RC14: Figure 8, 9 and 10. To test one of the main conclusions, if AE is higher for GPP than for CR, authors should test statistically if the slopes are different. I would test also the slopes for the figures 9 and 10 explaining the consequences of the statistical differences in the cases observed

AC14: We did perform a test (an analysis of covariance, ANCOVA) to compare the regression lines and test if the interaction of the metabolic rates with the inverse of temperature was significantly different from zero (meaning that the effect of temperature on metabolism depends on the level (e.g., season). The results of the analyses were described between lines 1–6 (page 12) and discussed in section 4.2. In the new version of the manuscript, we have included a new section (2.4) detailing all the statistical analyses. Methods section 2.4, this can be tracked between lines 176–189.

RC15: In the figure 9, the RMA analyses have been included but it is not necesary in this case because temperature is not a rate. In addition, the authors have not explained

when the RMA or OLS should be used in M and M.

AC15: We agree with the reviewer that it is not necessary to provide the results of the RMA analyses. Therefore, we decided to remove from our manuscript.

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