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Discussions

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

Anonymous Referee #2

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The authors quantified plankton metabolic rates along the Red Sea. They have shown that Chla and plankton 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.

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 nutrients 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 conclusion and the abstract is confusing. In my opinion, there is a large flaw in the experimental design proposed and it is difficult to resolve. 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.

Other figures such as 4-6 do not show crucial information in the current format.

Figure 3 and Table 3 to me are redundant.

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 collected between 7 to 9 and to me this sounds very late to incubate and obtain the full light period not precisely.

In The net community metabolism..... page 7, NCP should be estimated during the light

period (NCP 6 to 12 hours). The authors should show, the variation coefficient of the pool data and also the original CR, NCP and GPP data including their SE. Because, in these oligrotrophic areas the metabolic rates are very low and can be difficult to detect them. Therefore, the methodology needs to be very precise in the processes of filling, incubating and fixing the bottles.

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

The paragraph 10 in page 10. It should be transfered from the Results to the Discussion. And also, the first paragraph of the 3.2 Variability of plankton . . .. Is already mentioned in M and M.

The paragraph 10 in page 13. There are lots of references within oligotrophic areas very interesting and different to the authors ones that the authors should also include in the MS.

Figure 1. The name of the KAUST is excessive. I would include a unique bigger map with different colours or shapes to show the stations at each survey or season.

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 be included.

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. In the figure 9, the RMA analyses have been included but it is not necessary 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.

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