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Interactive comment on "Contribution of dinitrogen fixation to bacterial and primary productivity in the Gulf of Aqaba (Red Sea)" *by* E. Rahav et al.

Anonymous Referee #1

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The present study examined nitrogen fixation, primary production, and bacterial production with accompanying phosphorus enrichment experiments in May, July and September at a station of Gulf of Aqaba. In addition, the authors collected RNA samples in September when they claimed the water column was under full thermal stratification, and conducted metatranscriptomic analyses. From these results, the authors concluded that main player of nitrogen fixation between heterotrophic and autotrophic diazotroph would change with season in Gulf of Aqaba. Cyanobacterial diazotrophs have been considered as the major contributors of nitrogen fixation in most of the ocean, and non-cyanobacterial diazotrophs have been ignored. However, recent study demonstrated potential importance of non-cyanobacterial nitrogen fixation in some re-

gions (Riemann et al., 2010; Farnelid et al., 2011, PLoS one; Fernandez et al., 2011, PLoS one; Halm et al., 2012, ISME J). Therefore, if the finding in the present study is true, it will be of interest to the journal's readership. However, I am afraid the conclusion the authors claimed lacks satisfactory evidences for the following reasons.

1) The authors claimed that they conducted the experiments at "a representative pelagic station". What are the grounds that the station was a representative one in Gulf of Aqaba? Previous study in the Gulf of Aqaba indicated that diazotrophic community structure varied at different stations even in the same season (see Fig. 5 of Foster et al., 2009). Furthermore, although the authors highlighted the seasonal difference of nitrogen fixation (in Abstract), the difference ($\sim$0.1 and $\sim$0.4 nmolN l^-1 d^-1) generally occur in the same season (Foster et al., 2009).

2) The authors claimed they conducted the experiments during the mixed winter (March 2010) and the stratified summer periods (September 2010 and July 2012). However, the temperature profile in September 2010 does not indicate stratified water (Fig. 2a). When thermocline is defined as the depth where the temperature is 0.5°C lower than the sea surface temperature, the depth of thermocline in September 2010 would be similar with in March 2010. Hence, the related description in the manuscript is not very convincing.

The manuscript lacks spatial and temporal extent of the data to reach the conclusion. Based on these criticisms, I cannot recommend it publication in Biogeosciences.

Introduction

P10330 L5-9 As written above, seasonal contribution of heterotrophic and autotrophic diazotrophy to the total nitrogen fixation cannot evaluate from the obtained data.

Materials and methods

P10330 L16-17 Why were the RNA samples for metatranscriptome analyses collected only in September? The sample should also be taken during the mixed winter period,

and the results should be compared between different seasons.

P10331 L1-2 “The incubation were shaded with neutral density screening to mimic in situ irradiance conditions” How did the author determine in situ irradiance conditions? Primary productivity is strongly influenced by light intensity, and thereby the activity could be change if the incubation was not simulated light condition properly. In the present study, the authors compared between primary productivity and nitrogen fixation (Fig. 6b), and the results might be change.

P10332 L19-20 When did the authors conduct the incubation for bacterial production? Previous study indicated that the expression of heterotrophic nifH genes showed a slight diel cycle (Church et al., 2005, AEM; Zehr and Pearl, 2008, Microbial Ecology of the Ocean). Thus when the incubation was conducted could be important for the comparison between bacterial production and nitrogen fixation (Fig. 6a).

Results P10334 L13-14 As written above, this description is not very convincing.

Discussion

P10341 L4-P10342 L7 What is the definition of Trichodesmium bloom for the authors? Generally, concentration of Trichodesmium spp. in a bloom is >1000$\times$10ˆ3 trichomes mˆ-3 (LaRoche and Breitbarth, 2005, JSR). The number 2.3$\pm$2.0$\times$10ˆ3 trichome mˆ-3 indicate that 2.3$\pm$2.0 trichomes were included in the samples when they were taken by 1 L bottles. Since Trichodesmium spp. are heterogeneously distributed, 2 or 3 trichomes difference in 1 L would not suggest the end of bloom. The authors mentioned that concentration of NO2+NO3 and PO4 increase after a flood. Then, they should show the evidence of the flood. Furthermore, the authors claimed Trichodesmium were subjected to co-limitation by N and P prior to their observation. Are really Trichodesmium co-limited by N and P? The two references cited in the manuscript are both modeling study, and are not the proper reference.

As above, the evidences are very weak in this part of discussion

Reference

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