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Interactive comment on "Biogeochemical evidence of heterotrophic N_2 fixation in the Gulf of Aqaba (Israel), Red Sea" *by* Angela M. Kuhn et al.

Anonymous Referee #3

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The authors present a study which compares a series of 1-D biogeochemical models of increasing complexity with respect to the representation of different diazotrophic organisms against an in situ time series data set from a location in the Gulf of Aqaba in the Red Sea. Comparison of the output of these models with the in situ data and specifically the nutrient concentrations and ratios/differences between N and P concentrations (as quantified by the derived N* variable) is subsequently used to argue for a substantive contribution by heterotrophic diazotrophs to N2 fixation within the region. The manuscript is well written and in general the study rationale and model experiments appeared well designed (although see specific comments below). The subsequent results and potential implications of the study were certainly interesting and overall I felt there was much of value within the study. However, as outlined below,

I have a few concerns I would like to see the reviewers address.

Major comment:

The authors appear to undertake a thorough job of optimising many of the parameters related to their model (see e.g. Page 6 and Table 2). However, given the key question(s) being addressed within the study I was somewhat surprised that potential variability in what I would consider to be the key parameters in dictating how the diazotrophs interact with the N and P cycles were fixed, with no exploration of potential variability in these parameters. Specifically, the values of the N:P ratio within both the non-diazotrophs and the diazotrophs were fixed at 16:1 (Page 1 of Supplm.) and 45:1 (Page 5 of Supplm.) respectively. In contrast it is now fairly well recognised not only that N:P ratios within organic material can vary (see e.g. Martiny et al. 2013 Nature Geo. 6 279-283) but also (and crucially within the current context), that inferences of N2 fixation rates and interactions between diazotrophy and the cycling of N and P are highly dependent on both assumed values of these ratios and any variability within these (Mills and Arrigo 2010 Nature Geo. 4 412-416; Weber and Deutsch 2012 Nature 489 419-422). Consequently, I would suggest the authors should at least consider the implications of their assumed fixed N:P ratios for their interpretation and conclusions and perhaps also consider performing some sensitivity analysis around these currently fixed assumptions.

Specific comments:

Page 3, Line 2: It would be worthwhile directly stating the laboratory based studies considered here were specific to Trichodesmium. As far as I am aware we have little information on how other groups might be expected to respond to the drivers mentioned.

Page 5, Line 21: '. . . a generic autotrophic diazotroph. . .'

Page 6, Lines 10-25: I was unclear whether this parameter optimisation method was performed for each of the models (H0 – H3 etc) independently or a single parameter set was used? Additionally, see major comment above, did the authors consider using the parameter optimization method for the non-diazoptroph and diazotroph N:P ratios? See also Page 14, Lines 11-16, I was unsure why this choice was made, it appears to be a big assumption within the current context.

Page 7, Line 13: maximum reported growth rates for Trichodesmium are actually >0.5 d-1, see Hong et al. (2017) Science 356 527-531

Page 8, Line 9: see major comment above. Either this assumption should be justified, or, preferably I would suggest, some effort could be made to perform a sensitivity analysis of how the assumption influences the results/conclusions.

Page 8, Line 14: again related to comments above, some speculation on how this happens would seem appropriate in the context of this study. As a suggestion, uptake at high N:P ratios by non-diazotrophs might be one potential mechanism for shifting from inputs of nutrients with an apparent 'excess' N (i.e. positive N*) to an apparent deficit (negative N*), see e.g. Mills and Arrigo (2010).

Page 9, Line 19 (also Page 12, Lines 7-8): it is notable that even the most complex model struggles to reproduce the observed range in N* and I wondered whether the restriction placed on the models through the assumption of the fixed N:P ratios may be responsible for this?

A final general point which is also related to many of those above, within the context of this study I felt that some of the important details relating to the model which were presented within the supplement would be more appropriately outlined within the main body of the text as they are likely fundamental to interpretation.

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