Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-550-AC3, 2018

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

Angela M. Kuhn et al.

akuhncordova@ucsd.edu

Received and published: 26 May 2018

(Reviews are included in regular font; Responses are in bold font)

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.

Response: We are grateful for the overall positive assessment and appreciate the constructive comments, which we respond to in more detail below.

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.

Response: This is a very valuable point. We will perform the sensitivity analysis suggested and include the results either on the main manuscript or as part of the supplement. Results of the sensitivity analysis will be addressed and discussed

within the manuscript.

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.

Response: Agree, the laboratory experiments cited consider only Trichodesmium, while the model study of Dutkiewicz et al. 2015 considers a set of generic diazotrophic organisms with various sizes and growth rates. The following line will be added: "To our knowledge, these laboratory experiments have only explored the reaction of Trichodesmium and less information is available about the effects of climate trends on other diazotrophic organisms"

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

Response: Thank you for pointing out this typo.

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.

Response: Thank you for pointing out this issue with the clarity of the description of our methods. This part of the methods is described in section 3.3.1 (Optimized Parameters), however it may be more useful to introduce it before describing the optimization method. We will happily address it in the revised version.

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

Response: Thank you for pointing us to this updated reference. We will modify the text accordingly in the revised manuscript.

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.

Response: Agree, see response to major comment above.

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).

Response: We will happily include your suggestion as we also consider this to be a likely mechanism.

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?

Response: That is indeed a possibility that we have not explored. As brought up by Reviewer 1, other alternatives include horizontal physical transport in and out of the domain, phytoplankton stoichiometry, atmospheric deposition, and preferential PO4 remineralization. The latest may particularly affect PO4 at depth, while transport may affect surface values during summer (when exchange of surface waters with exterior waters has been reported to occur). Nevertheless, given the reservations about our discussion of N* that were expressed by several of the Reviewers we have decided to remove this figure and other references to N* in the text.

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.

Response: Agree, we will expand the model description on the main text to include details such as the denitrification process brought up by Reviewers 1 and 2. Also, as requested by Reviewer 2, we will further expand the supplement to include all model equations explicitly. However, we consider that including the complete model description in the main text may make it excessively long and diverge the attention from the results.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-550, 2018.