

Interactive comment on “Biogeochemical evidence of heterotrophic N₂ fixation in the Gulf of Aqaba (Israel), Red Sea” by Angela M. Kuhn et al.

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Reviewer 1 has strongly encouraged our manuscript for publication and described it as "clear, concise, and present(ing) well-designed modelling experiments". We are grateful for the positive assessment and appreciate the constructive comments, which we respond to in more detail below.

(Responses to each comment are in bold text)

Main comments:

N* model reproduction While the model (H3 in particular) does a very good job at representing NO₃ and O₂ observed concentrations, the model seems to be quite off from the N* data. This is not well enough highlighted in the paper which currently

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presents heterotrophic nitrogen fixers being key on reproducing the N* values. Because the model (even H3) is not able to capture most of the observed variability in N*, I don't think the model results support well enough this conclusion. I wonder what could be missing in the model and if you have thought about the role of preferential P remineralisation on N*. I wrote a paper in 2012 investigating the role of preferential remineralisation of P on the distribution of N* in the North Atlantic. In that region, this mechanism is necessary to reproduce the observed sub-surface maximum of N*. Here your model seems to have too small values in PO₄ and NO₃ at depth. Would it possible to test in your model the effect of preferential remineralisation of P to see if that helps re- producing the observed N* variability? If not, at least mention it. In our 2012 model, preferential remineralisation of P helped to get higher concentration of P, enhanced nitrogen fixation and then resulted in higher N* value at depth (as N* increases after the remineralisation of diazotrophic matter). Monteiro, F. M., Follows, M. J. (2012). On nitrogen fixation and preferential remineralization of phosphorus. *Geophysical Research Letters*, 39(6).

Thank you for suggesting this paper. We will include the citation and acknowledge preferential demineralization of P as a mechanism acting to generate the observed N* values. Nevertheless, as suggested by other reviewers, we will move our focus away from N* by removing figures 3 and 6 from the manuscript.

Minor comments P2, Line 1: Need to mention about atmospheric N sources

Response: Taken. The sentence will be changed to: “Locally the supply of new nitrogen can occur through several mechanisms, including microbially mediated N₂ fixation, diapycnal mixing injecting deep nitrate (NO₃) into the surface, lateral transport, atmospheric N sources and riverine input.”

P3, Line 5: Need to justify in the introduction why the Gulf of Aqaba is an interesting region to study nitrogen fixation.

Response: Taken. The referred paragraph in the introduction will be changed

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to: “In this study we explore the biogeochemical signatures that result from different assumptions about the ecological niches occupied by diazotrophs. We use the Gulf of Aqaba, a northern extension of the Red Sea. Aside from the reported presence of diverse diazotrophs types, the morphology of the Gulf of Aqaba limits horizontal transport of deep waters, thus allowing us to simplify the physical complexity of the model and focus of the biological component.”

P3, Lines 21-23: Sentence not clear. Can you amend?

Response: Taken. We will remove the phrase “(…) *the Gulf does not have permanent vertical stratification* (…).” for clarity. The modified sentence will read: “*Since inflow is restricted to warm surface waters, the Gulf’s deep water masses (>300 m) are locally formed (Wolf-Vecht et al., 1992; Biton et al., 2008) and have negligible horizontal transport toward the exterior (Klinker et al., 1976; Manasrah et al., 2006).*”

P5, Line 16: The model description is confusing on if/how H0 and H0’ represent N2 fixation. Here you say “without explicit N2 fixation . . . and follows the model equations described in Fennel et al. (2006, 2013)”, which would mean that there is a non explicit representation of N2 fixation in the model. If so, replace with “with non-explicit N2 fixation representation” and then describe briefly how it is done in Fennel’s papers. But later on there are many references to “with no N2 fixation” (P5/Line 18, Figure 4, P10 Line 5, P14 Line 20, . . .). Can you amend accordingly?

Response: We intend to modify the description as follows: “*H0 is the base model without diazotrophic plankton groups and follows the model equations described in Fennel et al., (2006, 2013). We test this model with and without a sediment denitrification flux, denoted as H0 and H0’, respectively. In other words, H0 includes denitrification but no N2 fixation, thus neglecting the importance of N2 fixation. H0’ does include neither denitrification, nor N2 fixation, and thus the underlying assumption of this model version is that inputs from N2*

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fixation and losses of fixed nitrogen due to denitrification are balanced.”

P6, Line 5: Isn’t there any evidence of DDA in this region or atmospheric source of N? Please add comments as they can be potential important sources of N.

Response: Taken. Discussion will be added on other potential sources of nitrogen. Below are some initial thoughts:

a) DDA DNA has been detected in the region, but not as abundant as unicellular, Trichodesmium and proteobacteria (Kimor et al. 1992, Foster et al., 2009). In general, due to the oligotrophic characteristics of the Gulf of Aqaba, small phytoplankton species (<8 micrometers) contribute more than 90% of the chlorophyll-a standing stock (Lindell and Post 1995). Dinoflagellates and diatoms together correspond to less than 5% of the phytoplankton biomass, except during ephemeral diatom blooms during spring when they can account for nearly 50% of the total biomass (Al-Najjar et al., 2007).

b) Recently, it has been shown that atmospheric dust input does not correlate with chlorophyll variability in the surface waters of the Gulf of Aqaba (Torfstein and Kienast, 2018). Nevertheless, based on measurements of local aerosols composition and a dust deposition model, it is estimated that atmospheric deposition nitrogen flux could support over 10% of surface primary production (Chen et al., 2007). This estimate has a relatively large uncertainty due to errors associated with the deposition flux calculation and the temporal variability in dust flux (Chen et al., 2007). Moreover, very low N concentrations and lower than Redfield N:P ratios from the surface down to 80 m were observed during the same time period of Chen et al. study (Foster et al., 2009).

References:

Kimor, B., Gordon, N., Neori, A. (1992). Symbiotic associations among the microplankton in oligotrophic marine environments, with special reference to the

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Gulf of Aqaba, Red Sea. *Journal of Plankton research*, 14(9), 1217-1231.

Foster, R. A., Paytan, A., Zehr, J. P. (2009). Seasonality of N₂ fixation and nifH gene diversity in the Gulf of Aqaba (Red Sea). *Limnology and Oceanography*, 54(1), 219-233.

Lindell, D., Post, A. F. (1995). Ultraphytoplankton succession is triggered by deep winter mixing in the Gulf of Aqaba (Eilat), Red Sea. *Limnology and Oceanography*, 40(6), 1130-1141.

Al-Najjar, T., Badran, M. I., Richter, C., Meyerhoefer, M., Sommer, U. (2007). Seasonal dynamics of phytoplankton in the Gulf of Aqaba, Red Sea. *Hydrobiologia*, 579(1), 69-83.

Torfstein, A., Kienast, S. S. (2018). No Correlation Between Atmospheric Dust and Surface Ocean Chlorophyll *a* in the Oligotrophic Gulf of Aqaba, Northern Red Sea. *Journal of Geophysical Research: Biogeosciences*, 123(2), 391-405.

Chen, Y., Mills, S., Street, J., Golan, D., Post, A., Jacobson, M., Paytan, A. (2007). Estimates of atmospheric dry deposition and associated input of nutrients to Gulf of Aqaba seawater. *Journal of Geophysical Research: Atmospheres*, 112(D4).

P7, Line 2: I could not find how denitrification is represented in the model. Can you make sure it is described?

Response: The denitrification representation was included in the model description in the supplement: *“When present, the denitrification flux follows Fennel et al. (2013) with a loss fraction 6 mol N₂ per mol of organic matter remineralized”*. We will include this descriptive sentence in the main text for clarity.

P9, Line 10: Need to comment on the model performance for PO₄. Especially for 100-600 m where H3 matches well observations for NO₃, still cannot get PO₄ right.

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Response: We will expand the text to remark that models have a similar performance with respect to PO₄, all underestimating total deep PO₄ by the end of the series. This suggests that all models lack a process affecting PO₄. As mentioned in Monteiro et al., (2012), processes affecting PO₄, and not considered in this simplified model, may 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.

P9, Line 11-12: Can you refer to Figure 3 here?

Response: This is likely to change, as Figure 3 will be removed following the comments of other reviewers.

P9, Lines 20-24: This paragraph does not add much so I would be inclined to remove it.

Response: Taken. Note that Figure 6 (N*) will also be removed.

P10, Line 9: It would be good to add a statement on why heterotrophic N₂ fixers improve the representation of deep oxygen as a key result of your paper.

Response: Taken. We will explore deeper why this occurs and include it.

P10, Line 18-20: Need to comment on potential reasons why H3 accumulates NO₃ over time.

Response: Taken. The revised version will include discussion on this matter. Our initial thought is that all models that consider nitrogen fixation accumulate nitrogen at different rates, as they enrich the nitrogen content of detritus, which is then remineralized at depth over time. The accumulation rate is also affected by the frequency of inter-annual deep-mixing events. An appropriate amount of N₂ fixation is achieved with H3 with heterotrophic fixers. While similar amounts of fixed N could also be achieved by the autotrophic groups alone if their parameters were changed, this would require increased N₂ fixation by autotrophic

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organisms and likely lead to unrealistic rates of N₂ fixation in the surface. A full validation and calibration of the simulated balance in contributions from each diazotrophic group can only be achieved when more observational information becomes available.

P11, Line 3-8: Need to add comments on why H3 and H2 have much higher N₂ fixation rate than observations between 0-DCM in the Summer 2010.

Response: Taken. In H3 and H2 the large N₂ fixation rates during summer are due to the contribution of blooming in the simulated behavior of Trichodesmium. This is likely to be overestimated under the current model configuration, as anecdotal evidence suggests that Trichodesmium blooms are more ephemeral and do not have a predictable seasonal cycle. Unfortunately, we do not count with routine sampling to be able to affirm the above. This model will require further calibration as more information becomes available to verify the seasonal cycle of nitrogen fixation in the region.

P11, Line 17-18: I would be more subtle about the models' abilities to replicate N* as it is still quite far from the observations (see my main comments above).

Response: Following this and other reviewer's comments we have decided to remove the figures and discussion concerning N*.

P12, Lines 17-32: While this is an interesting section about the contribution of N₂ fixation on PP, why include results from H3a which is not as realistic as H3? **Response: Taken, will be removed.**

P13, Section 5.3: One of the main points of the paper is to highlight the important role of heterotrophic nitrogen fixers. I feel this section could then be a lot stronger highlighting all the effect of heterotrophic N₂ fixers on the ocean biogeochemistry of this region. Here for instance I would mention that heterotrophic N₂ fixers improve the NO₃ and O₂ concentrations at depth, as well as the contribution of heterotrophic

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bacteria to total N₂ fixation. Also, would it be possible to plot the model difference between H2 and H3 to show the impact of heterotrophic N₂ fixation?

Response: Taken. We will add more emphasis on the effects on ocean biochemistry and add a figure of the difference in N₂ fixation between H2 and H3. Already, this difference, in terms of total N₂ fixation, can be seen in Figure 10b.

Figure 10 is not mentioned in the main text. It looks interesting so probably worse describing it at some point.

Response: Thank you for pointing this out. The results in this figure are briefly mentioned in section 5.2 (How does N₂ fixation Contribute to Primary Production?), however we did not referred specifically to the figure by mistake.

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

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