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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 #4

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General evaluation

This ms reports a 1D biogeochemical model analysis of time-series data from the Gulf od Aqaba from 2006–2014. The authors compare the behaviour of models with different diazotroph community structures representing various combinations of autotrophic and heterotrophic diazotrophs. While all model versions perform similarly with respect to surface chlorophyll, only models with diazotrophy can reproduce observed nutrient (N:P) ratios and heterotrophic diazotrophy is required to explain the vertical structure of nutrient and O2 concentrations.

In general, I find this study somewhat unconvincing. The model is overly simplistic in its mechanistic foundation and ignores processes I consider essential for this kind of analysis. While I do not dispute the potential importance of heterotrophic diazotrophy

for marine biogeochemistry, the conclusions and particularly the title appear overly optimistic and not well justified. The ms also appears to have been prepared rather sloppily and not thought through. The main problem is that all diazotroph parameters are unconstrained by the data, which, as outlined below, may be a consequence of the overly simplistic nature of the model or of an inappropriate cost function. Thus, in order to turn this ms into a useful contribution, the model or the cost function (or both) must be redesigned so as to achieve sensitivity to the diazotroph-related parameters.

Specific points

1. Starting with the title, I find the wording inappropriate. While it might be possible to obtain biogeochemical evidence from a model analysis, this is certainly not the case here. I would suggest something like "Modelling heterotrophic N2 fixation ..."

2. Model structure. Although the authors stress that they intended to analyse mechanistic assumptions (l. 15, p. 14), I find that the model is mechanistically rather weakly founded. While simplicity is of course an important goal in model development, one must take care not to over-simplify and neglect essential processes. I think this should be at least discussed thoroughly to put the results into the right perspective. The two assumptions I find most troubling are those of (1) constant (Redfield) stoichiometry of the autotrophs and (2) obligate diazotrophy, both of which are mechanistically wrong. Fernandez-Castro et al., J. Plank. Res. 38:946 (2016), FC in the following, applied a model with variable stoichiometry and facultative diazotrophy in the subtropical North Atlantic, where the vertical distribution of N, P, and N* poses similar difficulties as in the present ms. The model of FC is otherwise very similar in structure to the present one (phytoplankton, diazotrophs, zooplankton, detritus, nutrients, DOM), so I think the differences should be discussed, particularly with respect to the relations among stoichiometry, export and remineralisation.

Comparing the parameter settings between FC and the present model, I notice a very strong discrepancy (more than a factor of 10) in the initial-slope parameter (alpha) for photosynthesis in diazotrophs, although the units are the same in both models. It is not clear from the ms how or why the very low alpha was chosen (no reference given and not optimised). But it appears to be an important parameter given that the analysis is about the vertical structure and alpha basically defines how deep in the water column autotrophic N2 fixation can occur.

Another parameter that appears rather low is the maximum growth rate of the autotrophic diazotrophs. For example, Holl & Montoya, J. Phycol. 44:929 (2008) reported growth rates greater than 0.6/d for Trichodesmium grown in a chemostat, so a maximum (actually potential) rate parameter of 0.25/d appears unrealistically low. My impression is that these low settings reduce diazotrophy too much, maybe just compensating for the assumption of obligate diazotrophy but maybe also being responsible for the requirement of aphotic N2 fixation in the present model.

Further, the authors say that the diazotroph parameters were unconstrained by the data and that the parameter setting were taken from the literature, but do not provide references in Table 3 or elsewhere. The ms also does not say how it was determined that the parameters were unconstrained by the data. This seems inappropriate to me, since this is specifically a model study about diazotrophy, so I expect that great care is taken to select appropriate parameter settings. The fact that the diazotroph parameters are unconstrained by the data makes the choice of data appear questionable to me. In my view, the data should be able to constrain the most important aspects of a model's performance, and if this is not the case, one should try to either find better data or develop a better cost function (see below). The problem is that the inability to constrain the model parameters with the data implies that the associated processes are actually irrelevant. The simple fact that the authors observe better model performance when including diazotrophs implies that the associated parameters must have an effect, so I expect that a better cost function can in fact be designed which is capable of constraining those paramenters.

3. Model evaluation. The authors report that they performed sensitivity analyses to

obtain information of sensitive model parameters but they do not say how the sensitivity was quantified nor present any results from the sensitivity analyses. This could well be done in the supplement, but it es important for those who want to work with the model later.

The authors mention that they considered the first year of the model simulations as spinup but do not say how the model was initialised (from observations? what about the non-observed variables?). From my own experience with 1D modelling, one year is a rather short period for a spinup. Did the authors try longer spinups in order to find out whether the model is sufficiently close to a quasi-steady-state after one year? This should be discussed as well. It is this kind of omission, together with missing entries in the list of references (e.g., Fernandez 2011 and Smith 1936), that leaves an impression of sloppiness.

4. Parameter estimation. The authors apply RMSEs of absolute concentrations to obtain a measure of model-data misfit. This cost function will not be sensitive to large relative deviations if the absolute concentrations are low. Thus, it is only logical that the inability of the model to reproduce the negative N* in the surface waters "is not a source of large data-model discrepancies" (l. 8, p. 12). Introducing relative-error information or local scaling into the cost function could help here. The most important shortcoming of the authors' cost function, however, is that it neglects error correlations, see, e.g., Schartau et al., Biogeosci. 14:1647 (2017).

5. Figures. The use of log-scales in Fig. 9 makes it impossible to see the differences among models and between models and data. Please use a linear scale.

6. Conclusions. As it stands, the conclusions are not sufficiently supported be the model analysis described. In particular, the conclusions about aphotic N2 fixation are compromised by the choice of unrealistic parameter values constraining autotrophic diazotrophy to the very surface. If inferences about heterotrophic diazotrophy are to be drawn, at least the parameters determining the depth distribution of autotrophic

diazotrophy must be analysed with a detailed sensitivity analysis. The current analysis cannot say whether the deep N signal is really due to aphotic N2 fixation or exported material from the surface.

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