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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 #2

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GENERIC COMMENT:

This paper presents a model for heterotrophic nitrogen (N) fixation, implements it in a 1D context in the Arabian Sea and uses the model to test hypotheses on the relative contributions to N fixation by the different organisms. The subject fits perfectly in the Journal remits, and the work is highly relevant and it will be an important contribution to the topic of N fixation. I particularly liked the use of genetic algorithm for calibration and use of the model to test hypotheses. Authors set up a generally good framework to perform those test, unfortunately I believe that some further tests are needed in order to properly attribute the changes in the model outputs to the N fixation trait (see main comments).

MAIN COMMENTS: First of all, I would strongly encourage authors to be more com-

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Discussion paper

prehensive in the model description the supplement, because some key details are not clear, particularly on how the equations change in the different model set-up. In particular, in model H2 how the equation for zooplankton growth changes? Does Zooplankton see a single pool of phytoplankton formed by non-fixers and unicellular fixers, or does it graze separately on both? Given the non-linearity of the limitation function, the two options are very different. I would suggest writing explicitly all equations of H1, H2 and H3 that differ from H0 instead of summarising with sentences like "All other state variable equations are modified accordingly" The main concern is that authors directly compare models with very different structure and then attribute all changes observed in the results to the process without separating the impact of the biogeochemical process from the impact of the different model structure. For instance, in H3 authors added the heterotrophic nitrogen fixers, by adding to the implicit first-order mineralisation scheme of the small detritus, a more dynamic one that includes an explicit heterotrophic group (Hf). Such a big change in the model structure is bound to profoundly impact the model results, regardless of the N fixation ability of the heterotrophic group, because the whole dynamic of mineralisation is changed. I would recommend the authors to implement a H3' model where a non-fixer group of heterotrophic organisms uses organic and inorganic for of both N and P is used as Hf. The comparison between H0 and H3' would enable to understand how much of the mismatch between simulated and observed bottom waters N and O2 is due to an underestimation of mineralisation, while comparing H3' with H3 will allow to assess how the N fixation trait influence those dynamics. The comparison of H0 and H3' is much more important because the mineralisation rates have not been calibrated, and therefore could be affected by an initial bias. Similarly, when comparing model with 1 phytoplankton group (H0) with models with multiple PFT, all trophic dynamics can change, due to non-linearity in the grazing. Since Zooplankton dynamics are not shown, nor detailed equation for grazing and zooplankton growth in the different models, it is impossible to me to assess if the implementation of H1' and H2' similar to H3' are to be recommended or not.

Another main comment is related to the Redfieldian assumption. While I fully acknowledge the long tradition of Redfield ratio based models and data analyses, their power and their advantage, I'm always a bit concerned when these are used to draw conclusions on nutrient ratio dynamics, particularly in the short temporal and spatial scales. Phytoplankton internal nutrient ration and nutrient uptake are far from being constant and fixed to the Redfield ratio and they also varies a lot from species to species (e.g. Geider and La Roche, European Journal of Phycology, 2011, http://dx.doi.org/10.1017/S0967026201003456). I appreciate that this complexity is impossible to fully reproduce in biogeochemical model and therefore the Redfield assumption can still be used as first order approximation in simple biogeochemical model, however I would not use those model to analyse the instantaneous dynamic of nutrient ratios because this will be strongly affected by the huge assumption of fixed stoichiometry. Figure 6 itself shows how the model is not able to capture the wide variability of DIN:DIP ratio. For this reason, I would suggest to cut the part related to N*, or alternatively, repeat the analysis using annual means of DIN and DIP and include a discussion on the importance of non-Redfieldian dynamics.

SPECIFIC COMMENTS: Page 5, lines 19-20: in H0' N fixation and denitrification are balanced: where did denitrification occur? In the benthos? I recommend adding some detail to better interpret the vertical dynamics simulated by this model implementation Section 4.2.2.: Top left panel of figure 5 shows that model H2 and H3 are significantly overestimating surface nitrogen in the last 4 years of calibration, with the exception of the deep mixing events in winter 2007/2008. H3 largely overestimates surface nitrogen also in the validation period (figure 8). This important dynamic is not discussed in the paper. Section 4.2.4.: while I agree that H3 better compares with observed deep values, in the last couple of years a significant trend in deep nitrogen appears in the simulation and it's not in the data. I suggest authors to comment on that.

TECHNICAL COMMENT: Page 12, line 18: in 10b, the dot corresponding to Capone and Carpenter 1982 shows a N fixation equal or close to 0, that is quite different from the values simulated by the different flavour of H3 Figures 2,4,7: I recommend the authors to redraw the picture using a perceptually uniform and colour-blind friendly colourmap like viridis, inferno, magma or plasma in Python or Parula in Matlab. More details on the importance of this in the following video https://www.youtube.com/watch?v=xAoljeRJ3lU

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