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Interactive comment on "No nitrogen fixation in the Bay of Bengal?" *by* Carolin R. Löscher et al.

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Dear reviewer, Thank you for considering our manuscript important and valuable for publication in Biogeosciences. We are grateful for your comments and suggestions, which we addressed in the revised version, we believe they largely helped to improve our manuscript.

Response to reviewer comments:

Their hypothesis is that surface nutrient limitation restricts PP, the ensuing flux of sinking organic matter, and thereby oxygen removal at depth. However, the sampling focus is on N2 fixation at greater depths than the euphotic zone, which will accordingly not fuel surface PP without upwelling. This is generally prohibited in this strongly stratified regime. It is the level of N2 fixation in surface waters, not the OMZ, that will have the greatest impact on PP and therefore (potentially) organic matter supply and respiration

in the OMZ. As cited in the manuscript there are some observations indicating likely high surface nitrogen fixation by Trichodesmium in the Bay of Bengal (e.g. Sahu et al. 2017). I think this discrepancy between the hypothesis and the observations should be made explicit through the paper, including the abstract. However, I do agree with the authors that the nitrate isotope data do seem to argue against this in the case of their observational time period.

Response: This is a generally agreeable point, and the observation of Trichodesmium during other cruises is not contradictory but rather in line with our hypothesis of a water mass dynamic-dependent on-set of N2 fixation. For our cruise we were somewhat limited with regard to our coverage of the water column and thus we included isotope data which we read as an argument against N2 fixation during the time of the cruise in surface waters. The manuscript of Sahu et al is now more thoroughly discussed in our revised version of the manuscript.

The authors summarize some of the very high variability in N2 fixation in the Peruvian OMZ (i.e., detection limit to 840 umol N m-2 d-1); I think it could be useful to comment directly how this measured variability could provide relevant context to the BoB observations. i.e., how would this change the manuscripts conclusions? Indeed, that genomic signatures of diazotrophs were found suggests N2 fixation does occur in this system at some time points, which would be in line with sediment trap isotopic compositions.

Response: The observations from the OMZ off Peru are in so far interesting to compare to the BoB as they demonstrate first how N2 fixation in OMZs can vary, but also they show a potential of a very similar community of N2 fixers to actively fix N2 (as opposed to them being largely inactive). If we would assume rates in the same range for the BoB, an additional N input would be provided which would, according to our model, be enough to cause full anoxia. This is a helpful point and has now been included into the revised version of the manuscript.

I found a mixture of decimal points and commas (representing decimal points) in the text

Response: This has been cleaned up.

Lines 179-180: Please provide some value ranges for SST and salinity

Response: Ranges have been added.

Please provide a description in the Methods section about the remote sensing images: where did these data come from (sensor, database), and what exact dates were used to produce the images in Fig. 1 (can also go in figure caption if preferred). This also applies to the satellite-derived data in the SI, which includes phytoplankton types – it is important to know where this came from and the algorithms that were used to generate these.

Response: The information has been added to the figure captions as suggested.

Line 211: PP is a function of phytoplankton biomass and the biomass-normalized photosynthetic rate (depending on light availability, temperature etc), therefore lower POC biomass does not necessarily mean lower PP (as implied in the statement)

Response: I have some difficulties with this comment because it reads tome as some disconnect between primary production and POC. But I overall agree that POC is not necessarily an indicator for primary production. This point has been presented right above from line 202 on. I revised the sentence in question to make it clearer, it now reads 'While our POC concentrations from DCM are one order of magnitude higher than the satellite-derived POC estimates (Fig. S2) from surface waters indicating that POC and primary production in surface waters was not higher than in the DCM, it must be noted that our measurements did not cover the entire mixed layer and are thus likely a rather conservative minimum estimate.'

Line 217-218: But the N2 fixation measurements were not performed at the surface, they were performed at depth (>=60 m), where fixed nitrogen concentrations were

presumably higher and therefore potentially removing this niche for N2 fixers.

Response: This is true, and this is also the reason for the parallel presentation of nutrient data showing N depletion in the surface and of δ15N data showing no N2 fixation signal, both of which are thus supporting this claim.

Paragraph starting line 252 and Figure 5: Would it not be meaningful to include one or more non-OMZ sites in this analysis? i.e., to both indicate if (i) the OMZ sites have similarity between each other and (ii) are unique from non-OMZ sites?

Response: It would indeed be meaningful and relevant for a global assessment; however, the limit of the Venn diagram has been reached by including six datasets. The purpose of this plot was to show that the non-cyanobacterial clades in different OMZs are similar, non-OMZ areas would show cyanobacterial N2 fixers in combination with heterotrophs, however, it has been pointed out previously that those heterotrophs re not active in non-OMZ areas (e.g. Turk-Kubo et al. (2014)).

Lines 324-327: I do not understand this sentence – if the model includes the potential for Fe limitation of N2 fixation, but the prescribed Fe concentrations are high, N2 fixation should not be effected by the Fe limitation term?

Response: This is correct, I intended to say that the model only includes Fe as limiting nutrient and doesn't have any other possibly limiting nutrients such as molybdenum of Vitamin B12 included. The sentence has been revised.

Model equations lines 8–10 of the SI: equations do not balance? E.g. I count 42 oxygens on the left hand side of the second equation and 51 oxygens on the right? Please check this and other equations!

Response: Yes, a number got lost, here. Thanks for the hint.

Some more details on the model would be useful – i.e., does the model have any time iteration, is it ran until steady state for each upwelling value?

Response: The model does not have any time iteration, which is certainly a weakness given different elemental recycling times and is run into steady state for upwelling rates. The information has been added to the supplementary material.

Figure 6: It is quite hard to see the different lines in the plot, e.g., I cannot see the blue oxygen line and whether it always stays at 0 for all upwelling values

Response: We adjusted the lines in figure 6.

Line 712: I don't understand the sentence: '. . .export to the productive surface if stratification becomes weaker'. Do you mean upwelling of ammonia to the surface, which then fuels more PP?

Response: It meant to say that a stock of nitrogen would be build up, which could eventually be upwelled into surface waters and fuel primary production, there. The Figure caption has been rephrased for better readability.

Lines 339-341: Regarding the sentence: 'However, the fact that N2 fixation is limited by phosphorous supply via recycling in addition to upwelling and diffusive fluxes imposes an upper limit to O2 depletion.' But this cannot be the case in the actual BoB, as phosphate concentrations are in excess (not reported, but indicated by the negative intercept on the nitrate versus phosphate plot), yet there is still O2 present in the OMZ? Therefore, the field data indicate P availability in surface waters is not the cap on O2 depletion in the OMZ as suggested by the model?

Response: This is actually very true; the statement made no sense in the context it was presented and has been removed.

It would be useful to briefly comment on how the physics of increased upwelling (or reduced stratification) might or might not increase ventilation of the OMZ? Are they completely decoupled?

Response: This would largely depend on the source of upwelled water. Our model distinguishes between deep water upwelling (ventilated) and intermediate OMZ water

upwelling (not ventilated). We added a statement on the source of upwelled water to line 340.

References:

Turk-Kubo, K. A., M. Karamchandani, D. G. Capone, and J. P. Zehr. 2014. 'The paradox of marine heterotrophic nitrogen fixation: abundances of heterotrophic diazotrophs do not account for nitrogen fixation rates in the Eastern Tropical South Pacific', Environ Microbiol, 16: 3095–114.

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