

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

**Anonymous Referee #1**

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The manuscript by Löscher et al. addresses the important issue of assessing N<sub>2</sub> fixation rates in the BoB OMZ. Few rate measurements and characterization of potential diazotrophic communities have been conducted in this region; therefore these observations are a valuable addition to the literature. I also appreciated the attempt to link controls on N<sub>2</sub> fixation with the extent of oxygen depletion in the OMZ. I however I have a number of comments that should be considered prior to publication in Biogeosciences. These are listed below in the order I encountered them (not in order of importance).

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 N<sub>2</sub> 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 N<sub>2</sub> fixation in surface waters, not the OMZ, that will have the

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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.

The authors summarize some of the very high variability in N<sub>2</sub> fixation in the Peruvian OMZ (i.e., detection limit to 840  $\mu\text{mol N m}^{-2} \text{ d}^{-1}$ ); 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 N<sub>2</sub> fixation does occur in this system at some time points, which would be in line with sediment trap isotopic compositions.

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

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

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.

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)

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Line 217-218: But the N<sub>2</sub> fixation measurements were not performed at the surface, they were performed at depth ( $\geq 60$  m), where fixed nitrogen concentrations were presumably higher and therefore potentially removing this niche for N<sub>2</sub> fixers.

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?

Lines 324-327: I do not understand this sentence – if the model includes the potential for Fe limitation of N<sub>2</sub> fixation, but the prescribed Fe concentrations are high, N<sub>2</sub> fixation should not be effected by the Fe limitation term?

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!

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?

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

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?

Lines 339-341: Regarding the sentence:

'However, the fact that N<sub>2</sub> fixation is limited by phosphorous supply via recycling in addition to upwelling and diffusive fluxes imposes an upper limit to O<sub>2</sub> 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 O<sub>2</sub> present in the OMZ? Therefore the field data indicate P

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availability in surface waters is not the cap on O<sub>2</sub> depletion in the OMZ as suggested by the model?

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?

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