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Interactive comment on "Water column biogeochemistry of oxygen minimum zones in the eastern tropical North Atlantic and eastern tropical South Pacific Oceans" *by* C. R. Löscher et al.

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The authors thank Referee #1 for regarding this manuscript interesting. We addressed the comments and suggestions that were made, which we considered very helpful and informative. Please find our detailed point-by-point answers in the following, changes were highlighted in the text:

p. 4497, line 6. "among" vs "between"?

We changed between to among.

p. 4498, lines 11-13. You switch between uM and umol L-1. Pick one.

We changed to μmol L-1 throughout the manuscript.

p. 4499, lines 6-7. Something missing here. Reword. line 21. "among" vs "between"

We changed the sentence, it now reads:

'The effects of O2-dependent nutrient cycling processes occurring in these relatively small regions (Codispoti, 2010) are conveyed to the rest of the ocean (see e.g. Deutsch et al. (2007)). ' We changed between to among.

p. 4501, line 29 "during daytime" or "at daybreak"? p. 4502, line 5. "during daytime"

We now wrote 'during daytime'.

p. 4506, lines 2-6. Sounds overly dramatic. Temperature shifts will be gradual- not likely a step function which would induce massive lysogeny.

This is true. It may be worth to think about certain thresholds with regard to temperature dependent lysogeny. However, reviewer II suggested restructuring the whole paragraph on viruses in OMZs, which we did, and the respective sentence now reads:

'If lysogeny is the prevailing mode of existence in OMZ core viruses, one would expect changing environmental conditions such as temperature shifts (Bertani and Nice, 1954;Seeley and Primrose, 1980) to induce lysis of host cells. This would consequently lead to shifts in water column nutrient budgets that cannot be accounted for in biogeochemical models by microbial processes alone.'

p. 4507, line 23. Delete "are" in this sentence.

'Are' has been deleted.

p. 4509, line 8. How is BATS a "less intense OMZ area"?

We rephrased this, it now reads:

'In a much less intense OMZ area (e. g. in the tropical Atlantic around the Bermuda Atlantic Time Series Station), DVM-related transport was found to account for 30% of

C and 57% of N export from the euphotic zone, relative to trap particulate C and N (Steinberg et al., 2002).'

p. 4512, line 14-15. Arabian Sea- what about Ward et al. 2009 who contend denitrification is dominant in the Arabian Sea? Nature 461, 78-81 doi:10.1038/nature08276

This is true! We included the finding of Ward et al. and referenced it as follows: Moreover, N is (i) lost by denitrification (the 4-step reduction of NO3- to N2 (Devol, 2008)), which has been identified as the dominant N loss process in the Arabian Sea OMZ (Ward et al., 2009), or (ii) recycled by both DNRA (the dissimilatory nitrate reduction to ammonia, as hypothesized by (Lam et al., 2009)) and nitrification (the aerobic oxidation of ammonia via NO2- to NO3- under oxic to suboxic conditions (Ward, 2008)).'

lines 15-18. Could be better worded- ": : : lost by denitrification or anammox or recycled by DNRA or nitrification".

We agree and rephrased this sentence, it now reads: 'Moreover, N is (i) lost by denitrification (the 4-step reduction of NO3- to N2 (Devol, 2008)), which has been identified as the dominant N loss process in the Arabian Sea OMZ (Ward et al., 2009), or (ii) recycled by both DNRA (the dissimilatory nitrate reduction to ammonia, as hypothesized by (Lam et al., 2009)) and nitrification (the aerobic oxidation of ammonia via NO2- to NO3- under oxic to suboxic conditions (Ward, 2008)).'

p. 4513, line 24-25. Previously you quote an upper boundary of 20 uM (p. 4501, lines 16-17). In the next paragraph on p. 4514 you use 25uM as the upper limit.

In order to unify the borders and definitions of oxic, suboxic and anoxic and the ranges that were interpreted limiting for biogeochemical processes from our combined results, we included a table into the introduction. Based on this, we unified the definitions throughout the text.

Lines 18-19. You said this on the last page (4512) . The statement on N turnover in the ETNA has been removed, here.

p. 4514, line 14-16. How can "ratios" be a source of a nutrient?

We agree, this of course can't be, we changed it from 'N:P ratios' to 'excess phosphorous'.

p. 4515, lines 2-3. Please note: Dekaezemacker, J. et al. (2013); Bonnet, S., et al. (2013) and Turk-Kubo, K. A., et al. (2013).

Thanks for this input- we added all three references to the text.

Line 3. Whose unpublished data?

We added the missing information: Joshi and Löscher, unpublished.

Lines 15-21. Also should mention the recent kerfuffle regarding contaminated N2 stocks Dabundo, R., et al. (2014).

Yes, this is true and an important topic when talking about diazotrophy in terms of rates. Overall, all of us that have used contaminated or potentially contaminated gas stocks may have misinterpreted their data. Thus we added the following explanation:

'A very recent study however demonstrated, that N2 fixation rates may have largely been misinterpreted as the applied gas stocks were to different degrees contaminated with other 15N compounds, such as nitrate or ammonia (Dabundo et al., 2014). This study raised concern about all previously generated N2 fixation rates.'

p. 4516, lines 27-28. Not sure I fully get this. For both anammox and nitrification through regeneration of NH4+?

This may indeed happen at certain oxygen ranges; Kalvelage et al., 2011, showed a strong overlap of both processes at O2 ranges between $\sim$5 and 20μmol kg-1. The presence of organic matter in particulate form has very recently been shown, by Ganesh et, 2015 in ISMEj, to promote both processes, and it has been suggested that ammonia availability is the reason for this. We added this reference to the text:

‘Additionally, the strong correlation between nitrification and anammox activity to the modeled export production rates (Kalvelage et al., 2011) indicates an impact of organic matter supply also for autotrophic N-cycling processes, which has been suggested to result from ammonia availability (Ganesh et al., 2015).’

p. 4517, line 16. “Oxygenic” means O2 producing. Perhaps use “oxic” or “aerobic”?

We changed this to ‘aerobic’.

lines 24-26 and then some on the next page. Seems a redundant passage to earlier discussions.

We shortened the repetitive statements and restructured the section.

p. 4518, lines 7-10. Needs to be explained more fully.

We now introduced the feedback effects as proposed by Landolfi and by Canfield and discuss positive vs. negative feedbacks in this section:

‘Model studies, however, show that denitrification of N2 fixation-derived organic matter may lead to a net N loss that further stimulates N2 fixation, because 120 moles of nitrate per mole of phosphorus are used to remineralize Redfield organic matter via denitrification (Landolfi et al., 2013). In contrast, N2 fixation fixes only 16 moles N (per mole P). Because of those stoichiometric constraints, denitrification of newly fixed N would lead to a net loss of N, which would then enhance the N deficit, promoting further N2 fixation, a cycle that ultimately leads to a runaway N loss (Landolfi et al., 2013). Only by decoupling N2 fixation and N loss, e.g. by iron limitation or dissolved organic matter cycling, the N inventory may stabilize, otherwise the OMZ would become completely void of ïňĄxed inorganic N and the OMZ sulïňĄdic conditions would potentially evolve.’

Lines 18-20. Should clarify – from an “N” cycle perspective, or N:P ratios, N2 fixation should be a negative (stabilizing) feedback compensating for N losses as proposed by Deutsch et al. Indeed, the N2 fixation may be a positive feedback within the OMZ with respect to O2.

This is a very interesting topic to me; I included some additional discussion on feedback effects (see also comments above) and discussed this topic a bit more detailed from an N cycle perspective.

p. 4520, lines 6-9. See also Dekaezemacker et al. (above) for their 10oS transect to 100oW We included this reference and their results on Fe and inorganic carbon stimulation of N2 fixation.

Lines 16-19. Perhaps something else is constraining diazotrophic cyanobacteria here (e.g. Fe). See also Turk-Kubo et al. and Bonnet et al. (above)

For sure, but comparing our bioassay experiments to the ones of J.Dekaezemacker, we also found large variability. It could be speculated that it is a combination of factors, such as Fe availability along with organic matter and/or P. We included the references and their results on Fe-dependent N2 fixation.

p. 4521, lines 8-9. This point here is unclear to me. Clarify.

We rephrased this sentence, it now reads:

‘However, one of the predictions of the optimality-based model of N2 ïňĄxation by Pahlow et al. (2013), which is based on the assumption that natural selection should tend to produce organisms optimally adapted to their environment, is that the competitive advantage of diazotrophs is most pronounced under conditions of low DIN and increased DIP availability (Houlton et al., 2008). The ability to compete for DIP is less important at high DIP; based on this, high phosphate concentrations above the ETSP OMZ might actually reduce the selective advantage of diazotrophs compared to ordinary phytoplankton.’

p. 4522, lines 14-15. Perhaps cite the early Dugdale et al. 1977 observation here?

We included this reference as follows, again thanks for this hint!

‘An early observation from the Peruvian OMZ brought the development of H2S into

context with full denitrification (Dugdale et al., 1977).'

Line 23. "Diffusive" vs "diffuse"?

We changed 'diffuse' to 'diffusive'.

p. 4525,line 16. To "increase" rather than to "be increasing"?

This has been changed, accordingly.

line 18. Do you mean ">" rather than "<" here? How is this level of O2 "classical"? A reference might help. Line 23. ": remains to be proven". Is there a suggestion that anammox and/ or DNRA produces N2O? References. Or change to "assessed" or "demonstrated".

Thanks for this hint, we modified the sentence and added the following references:

'The production of N2O by archaea (and bacteria) depends on dissolved O2 concentrations and is increasing with decreasing O2 concentrations (Frame and Casciotti, 2010;Löscher et al., 2012). Denitrifying bacteria do not produce N2O in the presence of O2 (> 10 μmol L-1); however, when O2 concentrations are approaching 0 μmol L-1, N2O is consumed during denitrification. There is no N2O production under anoxic conditions. The significance of N2O production during anammox (Kartal et al., 2007) and DNRA (Giblin et al., 2013) in OMZ (see section 5) remains to be proven.'

p. 4526. This observation goes pretty far back- Firestone's work in soil. Also see Cohen, Y. (1978), Consumption of dissolved nitrous oxide in an anoxic basis, Saanich Inlet, British Columbia., Nature, 272, 235-237

We are aware of Firestones work but we considered the Cohen reference on the Saanich Inlet more adequate and included into the text - thanks for this hint.

p. 4529. Line 17. "Classical" is an odd word to use with Anammox.

Probably true, we removed 'classical'.

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