

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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As several points were criticized concerning the structure and readability of this manuscript, we restructured it. Major changes were made by merging the section on remineralization with the N cycle section; in addition, the manuscript was shortened considerably. Of course, this is a product of different authors, however, we now tried to integrate the different sections better, in order to make it more concise and connect the different parts better. It has further been remarked that a certain bias may be present in this study, as it synthesizes the results of a large scale project from GEOMAR and Kiel University. We now tried to compare our studies to other ones more obviously, to

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reduce the impression of biased opinions.

A table of content, two additional figures and one table have been added.

P4500: l.14-15 Can Fe and Mn and SO₄ reduction really be distinguished based on O₂ levels?

No, a certain overlap has e.g. been described in Canfield and Thamdrup, 2009. We removed the sentence on O₂ levels as it was confusing.

4501, l.10 connect sentence.

The sentences were connected

4501, l.24-25: Explain how? In a review such a sentence sounds odd and offers nothing.

We decided to remove the sentence as we agree that there is no additional insight provided by it.

4502: l.2 leave out visual.

‘Visual’ was removed.

4502, l.7 leave out brackets.

The brackets were removed.

Overall comments to section 2.2: This section lacks a distinct thought structure. The reader is exposed to a lot of information that does not seem to have a particular logic or goal. In addition, this section does not arrive at a conclusion that is used at the detailed level that is provided here. Please reduce to the essential that is needed to support your major line of argument.

We restructured this section and merged it with section 4.2 in order to clarify the importance of diel vertical migrating species for the OMZ biogeochemistry.

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4503, l.25 "to move forward.." The conclusion comes at an odd place and is phrased oddly. Move this together with your other conclusions to the end and synthesize better with the rest of the text.

We removed this sentence, as well as the other future objectives in the text, and included them into the summary (former section 10).

Section 3: This section is very interesting, but as for the other section, it is poorly integrated. The information density is high, but what is critical for the reader to know? A hierarchy is needed. Better to be guided by a particular argument one wants to put forward than to present everything that is known even it is very interesting. What is known about the temperature dependence of viral activity in the ocean?

We restructured this paragraph and set the focus on the meaning of viruses in OMZ waters- which are to date a rather understudied topic.

4504, l.25: none-particle bound community: odd term.

As the chapter was restructured, this sentence changed, too. Thus this term got removed.

4506. l. 27: Do you imply that microbes are protected in aggregates? Is the more direct evidence for this claim?

Yes, this has been demonstrated, before, we added the respective references to the sentence:

By attachment to particles such as marine snow and fecal pellets, microbes get direct access to the nutrients stored therein. Moreover, they can create protective microenvironments via biofilm formation (Hall-Stoodley et al., 2004 and references therein).

4507, l.1: Again the term " move forward " is used in the middle of the summary. Such a sentence is not needed in this summary.

We removed this sentence and included it into the summary (section 10).

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4507, l.1 'lead' instead of 'led':

As the chapter was restructured, this sentence changed, too. Thus this term got removed.

Section 4:

4507, l.23 leave out 'are'

We removed that.

4508, l.2-5. Again, recommendations for further research are made. If you find this so important to include, why don't you move your recommendations to a separate section rather than repeatedly distracting from the flow of the text with these inserts? It would be more useful if the sections are tied together better.

We removed the recommendations and included them into the summary (section 10).

4508, l.11, who is 'they'?

This refers to pelagic species conducting DVM, we added this information to the sentence.

4508: l.20 Is it necessary to use present the 5kPa partial pressure unit instead of one commonly used unit. This adds just unnecessary extra info.

It is probably unnecessary and confusing, as well. Thus, we removed it.

4508, l.28: Move up verb in this sentence. Very hard to understand meaning. We rephrased the sentence:

'The expansion and intensification of OMZs may thus reduce zooplankton and nekton mediated fluxes by decreasing DVM.'

4508: Section 4.2 fits better with section 2.

We included section 4.2 to section 2 and re-structured section 2, accordingly.

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4509, l.2: replace 'to the' with 'in'

This has been replaced.

4510, , l.1 'pronounced'. Do you mean 'higher'?

Yes, this has been changed.

4510, l.3-5. I don't understand this sentence very well. Can vertical velocities drive vertical tracer fluxes, or aren't vertical velocities derived from vertical tracer fluxes?

Yes, vertical velocities indeed drive vertical fluxes of dissolved tracers such as DOC or oxygen. We recommend in this case the review paper of Levy et al. (2012) 'Bringing physics to life at the submesoscale' in GRL, where a comprehensive overview of the role of submesoscale processes for marine life is given. The mathematical definition of a turbulent tracer flux is $F = \text{mean}(w \cdot T)$, where w is the vertical velocity fluctuation and T the tracer fluctuation. However, it is also true that vertical tracer fluxes can be used to quantify vertical velocities, as it is very difficult to measure the vertical velocities directly. However in this case we talk about the general role of vertical velocities and not about observational approaches how to measure them. We guess that the word 'tracer' might have caused confusion in this case as and we now use the word 'solutes'.

Section 5: Needs some restructuring. Again, the level of detail needs to be funneled such that the reader does not get completely confused by the sheer amount of information.

We restructured this section in order to avoid repetitions, major changes made to this section are that we merged it with parts of the previous section 6 and that we structured it in 4 subsections:

1. O₂ - a major control N cycle processes in two contrasting OMZ regions
2. The role of nutrient stoichiometry for primary production and N turnover in OMZ waters

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3. N₂ fixation- an underestimated term of the N budget in OMZs

4. Feedback controls of the N cycle in OMZ regions

4512, l.21. Complicated sentence. Shouldn't this sentence be moved up? The key new paradigm is the spatial connection of N₂ fixation and N₂ loss. Make this more obvious.

This has now been moved to the beginning of the paragraph and explained in more detail.

4513, l.3. replace 'give'. The word give implies a causal relationship, whereby the N regime is a cause of the O₂ regime. There is, however, a mutual effect.

Due to the restructuring of this section, this sentence was removed.

4513, l.17: Nitrification cannot be only N turnover process. In l.11 you say that dissimilar processes occur.

The statement on dissimilar N cycle processes seems to be confusing, here, we changed it to :

'This difference between the ETNA and ETSP OMZs is mirrored by a diverging d¹⁵N-nitrate signal, which is strongly positive in the ETSP but has negative values in nitrate depleted surface waters of the ETNA (Ryabenko et al., 2012), indicating different N turnover processes characteristic for these two regions.'

4513 l.27 - 4514: l.10 The extrapolation of the occurrence and significance of a process merely based on O₂ tolerance levels is very tentative and should be treated as such.

It is very tentative, thus the statement of the increased N loss area has been removed.

4514, l.12 leave out 'we could identify'. In essence N limitation was implied from experiments.

Changed as suggested:

'Despite the fundamental differences between the OMZs of the ETNA and ETSP with

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regard to N loss, the results of short-term mesocosm experiments implied N limitation of surface plankton communities in both areas (Franz et al., 2012a; Franz et al., 2012b).'

4514, l.11 to l.21. This is a very important section and strangely not given very much space or depth altogether compared to other detailed sections in the text. DOP discussion: Different paradigms or current views should be juxtaposed more clearly. What about Poly-P storage? Is the assumption of a Redfield stoichiometry justified? There is altogether a need for a separate P section or a section on CNPO stoichiometry in this paper.

By restructuring this section, (we now included a subsection 'The role of nutrient stoichiometry in OMZ waters') we added a discussion of the role of P including DOP. 'Part of the excess phosphorus was consumed by non-Redfield production, predominantly by diatoms. Release of dissolved organic phosphorus (DOP) by phytoplankton further diminished excess P. N:P of the accumulated biomass generally exceeded the supply ratio (Franz et al., 2012b). These results demonstrate that low nutrient N:P conditions in upwelling areas overlying O₂-deficient waters represent a net source for DOP. Whether accumulated DOP stimulates growth of diazotrophic phytoplankton is presently unknown. However, a very recent model suggested an important role of DOP for stimulating growth of N₂ fixing organisms based on large-scale surface data sets of global DON and Atlantic Ocean DOP (Somes and Oschlies, 2015). This model indicated that the marine N- budget seems to be sensitive to DOP and that access to the relatively labile DOP pool expands the ecological niche for diazotrophs. First direct evidence for a stimulating effect of DOP on N₂ fixation was obtained from a mesocosm experiment, the authors of the study noted that a general stimulating effect of DOP on N₂ fixation has been observed (Meyer et al., 2015). :

4515, l.11. I saw a paper in ISME on N cycling on cyanobacterial aggregates by Klawonn et al. (2015). This might be a very useful reference to demonstrate co-existing N cycle processes in a cyanobacterial aggregate.

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Thanks for this hint; we included the information on co-occurring N turnover processes in Baltic Sea cyanobacterial aggregates and the Klawonn reference to the text.

4516, l.1-2. Last sentence provides nothing.

We removed this sentence.

Section 5.4. The title promises more than the text provides. This paragraph is more about the connection between Anammox and export fluxes than nutrient regeneration and primary production. Consider changing the title or do better justice to the title by including relevant information on nutrient regeneration and primary production. The complete section has been restructured; the content has been integrated into the subsection on Feedback controls of the N cycle in OMZ waters, see also comments to reviewer I.

Section 6

Section 6.1 can be tied with the previous section 5.4. The section on stoichiometry comes too late. It would tie the different arguments together.

See comments above; we merged this section with section 5 and moved the section on nutrient stoichiometry up.

Section 6.2: The information here implies a major paradigm shift. I doubt many readers will understand this. Broaden this section, bring out its true significance and explain better. Section 6.2 is a very critical section, but unfortunately not explained well at all. It also ends very abruptly. Consider rewriting the whole paragraph. It is based on the argument put forward in Landolfi et al. (2013). Emphasize that this is a paradigm, but that the paradigm doesn't work too well for different reasons. The problem is that the section is quite confusing for a reader to figure out. I have difficulties understanding the meaning of the sentence 'Denitrification partly reverses the role of remineralisation in the nitrogen cycle of OMZ and acts to transform them into net sinks of fixed N, because.....' It's a huge sentence, very convoluted in style and very dense in information.

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The flow of the argument is interrupted by section 6.3 and then picks up in section 6.4. Restructuring is needed.

See comments above; we merged this section with section 5, re-structured it and put more emphasis on the different modes of feedback regulation as discussed by Landolfi et al..

4517, l.16 Oxygenic respiration is the wrong term. Use Oxygen respiration. Biochemically the first sentence is misleading. A respiratory process does not regenerate an inorganic form of nitrogen.

We used the term 'aerobic' instead.

4517,l.24 '... OMZs harbor diverse bacteria of the N cycle'.

According to the comments of reviewer I, this sentence has been restructured, thus this expression has been removed.

4517, l.26, there are no nitrates, use singular.

This has been changed.

Section 6.3 conveys a confusing message. Back to zooplankton, but very briefly. Is this section necessary or can this information be included elsewhere?

The information has now been included into section 2.

Section 6.4. This is a very informative section, but again, consider which key arguments you intend to put forward and focus on these. Introduce the optimality-based model. The term is not introduced prior.

This section has been moved to section 5 and has been re-written, we added an explanation of the optimality-based model to it:

'However, one of the predictions of the optimality-based model of N₂ fixation by Pahlow et al. (2013), which is based on the assumption that natural selection should

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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.'

Section 7: Section 7 should be tied with the previous section.

We do not agree on that point, the particular importance of sulfidic events with regard to climate change and future development of OMZ waters requires an own section. We however tried to better connect it to the previous and the following sections.

Section 8:

Retain style by providing an introduction paragraph

We included an introduction to this chapter.

4525, l.25, remove 'indeed'

Has been removed.

4526, l.25: What is exactly the recent hypothesis by Canfield et al. (2010)? This is written as if every reader should know this.

An explanation has been added to the sentence:

'This is in line with the recent findings from the anoxic event off Peru by Schunck et al. (2013) and similar to the recent suggestion of a cryptic sulfur cycle where sulfate reduction is coupled to rapid H₂S oxidation by NO₃- proposed for the OMZ off Chile by Canfield et al. (2010).'

4527, l.21. 'implies'

This has been changed.

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Section 9:

4529, l.13: core community: State how you define a core community and what this means for OMZ.

We changed the sentence by adding information on what is special about the microbes in the OMZ- namely that most of them are there, wherever you are :

'In accordance with several previous studies (Stevens and Ulloa, 2008;Stewart et al., 2012) a large part of the microbial community has been identified to be phylogenetically similar throughout the OMZ.'

Section 10: Cut out all your open questions from the text and paste them into this section.

We included all open questions in the text into section 10.

4533, l.5. 'In detail, we aim....' . Who is 'we'? Shouldn't this be a general recommendation for further work

This has been rephrased:

'In detail, future studies may address the following key questions'

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