

Interactive comment on “High bacterial organic carbon uptake in the Eastern Tropical South Pacific oxygen minimum zone” by Marie Maßmig et al.

Anonymous Referee #1

Received and published: 24 July 2019

General Comments.

The manuscript by Maßmig et al. shows interesting results from two cruises in the ETSP OMZ off Peru. The combination of DOC, TDN, DHAA and DCHO with bacterial production and extracellular enzyme rates provides a nice overview of the microbial activity in general terms. Authors also show diapycnal fluxes for oxygen and DOC, including the potential role of microbial processes into those total fluxes. A similar manuscript has been recently published by the same authors (Loginova et al. 2019 Biogeosciences, 16). DOC, TDN, DON, DHAA, DCHO and diapycnal DOC and oxygen fluxes were also measured/estimated in a previous cruise in the same area. It is clear

C1

that the present study includes other data but discussion lacks a comparison between both studies and some results/conclusions seems to be repeated. For instance, the 33% of oxygen loss over depth attributed to bacterial oxygen demand is quite similar than in the previous study (38%). Please extend the discussion and comparison with the previous manuscript.

The stations were sampled in two cruises (April and June) and distributed in three transects perpendicular to the coast: Lima, Paracas and Puerto Caballas (approx.). Spatial and temporal variability is however not considered in the manuscript. Some data correspond to some transects and cruise and other data correspond to other but no clear differentiation is included. Substances concentrations and fluxes were measured in Lima and Paracas transects in April, but enzymatic activity was measured in Paracas and Puerto Caballas in June. These data are however pooled and used for all the later estimations without any further consideration of spatiotemporal differences. Only one transect (Lima) is shown in Figures 2-3, are the conditions equal in the other transects (Temperature, Oxygen, Chlorophyl. . .)?

Specific Comments.

Title: It does not reflect the measurements performed in the study. “Bacterial organic carbon uptake” was not measured.

L19: Bacterial growth efficiency was taken from Rivkin and Legrende (2001) as a simple function of temperature. It should not be considered as a result from the present study.

L25: Gruber et al. is a good reference for global scale processes and future conditions, however, a better reference for the measurement of anoxic conditions in the ETNP OMZs would be: Tiano et al. 2014. Deep-Sea Res. Part I. 94, 173-183.

L28: One classical reference dealing with the extention and volumens of the different OMZs is Paulmier & Ruiz-Pino 2009. Progress in Oceanography 80, 113-128.

C2

L36-37: DNRA might result in lower metabolic energy yield, but it is not a major pathway in OMZs. Although it has been found in the ETSP, it showed sporadic and low rates (Kalvelage et al. 2013). On the other hand, denitrification might be considered one of the main anaerobic heterotrophic processes but it is yielding 99% of the energy compared to aerobic respiration, i.e. it is almost equally efficient. This paragraph seems to be biased to introduce the idea of inefficient anaerobic metabolism, but it is not proved.

L51-58: The effect of oxygen concentration on bacterial production and extracellular enzymes activity was ambiguous before the comment of G. Taylor. When the differential particulate organic matter was considered, hydrolytic rates were similar. This paragraph needs then some rewording because the study is not clearly justified now.

L61-62: Again, I disagree with the "lower efficiency of anaerobic respiration" (unless other processes different than denitrification are proved to be relevant).

L86-87: It is not clear for me if the filter or the ampule were rinsed with the sample.

L96-106: To be consistent, what is the detection limit and precision of the DHAA and DCHO analysis?

L116: Fig. 5 is cited before Fig. 2.

L131: Bacterial Production was measured at 13°C for all samples. Considering the range of temperatures found along the water column (7-24 °C), incubation temperature was up to 12°C off the in situ temperature. There was no compensation for the temperature variation, probably leading to significant deviation from in situ estimates. Considering the relevance of these results for the discussion, authors should correct measured rates with in situ temperature.

L154: Enzymatic rates were also measured at a fixed temperature of 13°C. Could in situ temperature be taken into account?

L159-160: Please improve the description of the "Gas tight incubator". Considering the oxygen concentration values in your "low oxygen" incubations (8-40 $\mu\text{mol/kg}$), how

C3

realistic are the conclusions applied to the anoxic core from these incubations? Oxygen concentrations of 8 μM are way above the K_m for microbial processes such as Oxygen respiration, ammonium and nitrite oxidation, for instance, and above the inhibition values for anammox and denitrification. Please, include in the discussion the possible limitation of the measurements considering the high oxygen values achieved in the incubations.

L201 (and L314): TDN includes the inorganic fraction. Nitrate in OMZs increases with depth, and might reach values up to 30-40 μM (example: Lam et al. 2009. Proceedings of the National Academy of Sciences 106, 4752-4757), which might represent 80-100% of the measured TDN. Could the authors include inorganic nutrients and use DON instead?

L261-270: It is not clear how the parameters (DOC loss) have been calculated, only ranges are shown and it feels like the ranges have been subtracted without including the apparent heterogeneity of the different stations. Based on the data shown in Fig. 5, the large differences in the oxyclines must result in large differences in diapycnal oxygen fluxes. Some separation in the data shown in Fig. 5 would be advisable. Anoxic conditions are reached at depths varying from 20 to 100 m, probably with very different values for the measured variables (Chl a, DOC...) too. Contrary, DOC values change quickly in the first 10 m, but seems to be relatively constant below. Dots are not connected with lines so it seems to be a pool of data without a clear pattern. All the stations seem to be the same.

L275: DNRA might have lower energy yield, it is not so low for denitrification.

L291-292: I would delete "nitrous oxide" otherwise further explanation is needed as the contribution from anammox to N₂O production is quite reduced.

L296-297: Remove "respiratory" from "autotrophic anaerobic respiratory pathway". Babbitt et al (2014, Science 344, 406-408) and Kalvelage et al. (2013. Nature Geosciences 6, 228-234) are also appropriate references for that quote. In addition, I would

C4

delete the sentence in L298-299, denitrification+anammox are included in the global estimations for N losses.

L301-307: This section exceed the results obtained in the present manuscript. A possible link to N cycle could be pointed, but the connection between hydrolysis and coupled denitrification-anammox is not supported.

L314-316: Inorganic nitrogen might be the mayor fraction of TDN. This fact must be taken into account, especially if any stimulation of metabolism is considered.

L317-322 and L323-331: These paragraphs seem to be not finished. There are no clear conclusion for the discussion of these results.

L346-347: According to M&M, BGE followed the established temperature dependence. If no other parameter was used for its calculation, I cannot see how the results of this manuscript for this calculated (but not measured) parameter suggest that oxygen availability control bacterial growth efficiency.

L365-367: Well, this study provides estimations, but does not provide measurements for carbon and oxygen losses.

L378: Why a BGE of 20% is now assumed? BGE was estimated based on in situ temperature before.

L383-390: The presented data for bacterial production can not be directly attributed to denitrification as it was not directly measured and the high oxygen levels during the BP measurements could have inhibited denitrification. The last and conclusive sentence seems to be pretentious.

L392-400: Conclusions should be more attached to the obtained and proved results of the measurements. The measurements of bacterial production do not allow to prove the dominance of individual pathways and even less to link it with the production of nitrous oxide.

C5

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2019-237>, 2019.

C6