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Comment on bg-2022-137

Anonymous Referee #1

Referee comment on "Nitrite cycling in the primary nitrite maxima of the eastern tropical North Pacific" by Nicole M. Travis et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-137-RC1, 2022

Reviewer report

Comments and review for the manuscript "**Nitrite Cycling in the Primary Nitrite Maxima of the Eastern Tropical North Pacific**"

- **Overview and major comments**

1.1 Overview

The manuscript 'Nitrite Cycling in the Primary Nitrite Maxima of the Eastern Tropical North Pacific' by Travis et al. investigated the distribution and cycling rate of NO2- in the upper ETNP. Statistical analysis and modeling approaches also provide valuable information on understanding the depth and magnitude of PNM. The authors found that the depth of PNM can be well predicted, while the magnitude of NO2- accumulation was less well correlated with any of the measured biological parameters and remained hard to reconcile; instead, several potential reasons were proposed for explaining the varied NO2- maxima.

Overall, I feel the present study is well designed and executed; the main results and findings improve the mechanistic understanding of the formation, distribution, and cycling of NO2- in the upper layer of this global relevant oceanic regime, albeit the reasons responsible for the varied magnitude of NO2- at the PNM remains unresolved. The

manuscript is well organized and written despite some parts of the results and discussion appearing to be long and redundant that can be improved.

I have a few concerns and questions regarding the data processing, interpretation, and discussion, and I would like to see the authors' response to these comments (see below).

1.2 Major comments

1) The paired light-dark incubation is one of the strengths of the present study. Given that most previous studies used dark incubation for nitrification rate measurement and light incubation to quantify phytoplankton-associated processes rates, the paired light-dark incubation should inform more comprehensive and accurate rates by integrating the rates derived in both conditions. The present manuscript is unclear how the daily rate is derived and whether the results from both light-dark incubations have been incorporated? Meanwhile, comparing the light and dark incubation rates would also help assess the diel rhythm of NO_2^- cycling in the sunlit ocean.

2) I have concerns about using the gross rates derived from the high tracer enrichment (i.e., 200 nM). Because the NO_2^- concentration drops sharply outsize the PNM, and the NH_4^+ concentration appears to be low in most of your incubation depths (Table S1), the rates reported here should be attributed to 'potential rates.' While I acknowledge these potential rates are still very valuable, care should be taken in interpreting these results. For example, the authors measured some conspicuous high rates under substrate depleted samples, such as those high NO_2^- uptake rates (> 100nM/d) above the PNM where ambient NO_2^- is low. It tells the high potential for the phytoplankton to control the cycling and distribution of NO_2^-, but is it meaningful to use those potential rates to calculate the residence time?

3) The prominent accumulation of NO_2^- (i.e., >1μM) in the coastal stations is not surprising. It is interesting to see the absence of higher ammonia oxidation rates in these more eutrophic systems, as is frequently observed in other studies. On the other hand, the authors observed a mild increase of NO_2^- released by the phytoplankton, but I expect a long time is still required to get the high NO_2^- concentration observed here. The question is, can you find evidence of such a long residence time of the water mass here,

as I expect a short residence time of these shallow, high dynamic coastal waters? I am also very interested to know if the main findings (both the contributing processes and time required for PNM formation) derived from the tracer experiment are consistent with dual NO_2^- isotope (i.e., 15N-18O natural abundance) in this region (if any). To my knowledge, the combined use of the natural abundance and isotope labeling approaches in exploring the PNM remains lacking, so any comparisons between these two independent methods would be very helpful.

2 Specific comments:

Line 41: Not sure how the authors derive the concentration of 300nM? Is it based on any specific statistics? As NO_2^- concentration at the PNM varied over space and time (as highlighted in the manuscript) and frequently fell below 300nM (e.g., in the subtropical gyres), justification is needed to better clarify the number.

Line 56: I suggest revising the description of the three microbial groups to ammonia oxidizers, nitrite oxidizers, and phytoplankton.

Line 57: Bacterial ammonia oxidizers should not be excluded as they might also play a role in some regions, such as the coastal zone.

Lines 138-154: Please clarify the detection limit and accuracy of the methods used for on-board and onshore nutrients measurement.

Line 151: The acronym of PPS has been defined in line 136.

Lines 180-181: I have concerns about using the HDPE Nalgene bottles for light incubation due to the screen of light by the HDPE bottle. PC bottles are more widely used for simulated light incubation. Has the light intensity been measured inside the bottles? How accurate was the actual light intensity compared to the in-situ light intensity?

Line 213: For the samples with low in-situ NO_2^- concentration and high NO_2^- consumption rate (either by NO_2^- oxidation or assimilation), the newly produced $^{15}NO_2^-$ via $^{15}NO_3^-$ reduction might have been utilized by NOB or phytoplankton. The observed NO_3^- reduction rate thus might represent a conservative estimate.

Line 233: I have two questions/ concerns regarding the rate calculation: 1) the equation is used to derive the gross (i.e., in-situ NH_4^+ plus $^{15}NH_4^+$) oxidation rate (potential rate), taking the different f15N for samples from various depths and stations, the enhancement of the rate due to tracer enrichment would also be varied. 2) it is unclear to me how the daily rate was calculated, have the rates from light and dark incubation been considered? These issues should be clarified.

Lines 311-312: The criteria for defining coastal and offshore stations should be clarified, e.g., by using the isobath or distance to the shore? Mark the boundary of the 'coastal zone' in Fig. 1a should also be helpful.

Line 349 and line 373: Fig. 2 and 3 contain a lot of statistical information that is very useful. But even though the regression of NO_2^- maxima /PNM depth against the parameters (listed in Table 1) was not fully shown in Fig. 2 and 3. Would you consider

plotting a heatmap showing the result of Pearson correlation analysis (or other statistical information) between NO_2^- maxima /PNM depth and the collected parameters? That would provide more comprehensive information and save some space at the same time. In addition, as you have measured NO_2^- uptake rate, I assume you have also measured the PN concentration, which is a better indicator of organic matter stock and biological productivity than the Chl-a; thus should be included in the main results, and the statistical analysis.

Lines 434-435: These results suggest either a quasi-homeostatic or physical dispersion plays a role in determining the magnitude of PNM.

Section 3.6 and 3.7: These sections are long and read dense; many results are similar between the sections. There are also a lot of similar descriptions in section 4.3. I suggest reducing these parts and focusing on the main findings.

Lines 609-610: I concur that a net production rate is required for the formation and maintenance of NO_2^- accumulation at the PNM. On the other hand, negative feedback, either physical dispersion and/ or enhanced biological NO_2^- consumption, must be involved to restrict the magnitude of PNM to a certain degree. That saying, NO_2^- accumulation at PNM should be in the quasi-steady state that varies over the diel cycle, seasonal cycle, or some event disruption, making the concentration of NO_2^- at PNM less predictable.

Line 650: See my comment on sections 3.6 and 3.7 above. Please consider reducing these sections to avoid redundancy.

Lines 710-711: Check the format.

Lines 749-751: Again, these results indicate a dynamic feature of NO_2^- concentration at the PNM, which can't be fully explained by a snapshot of the measured rate.

Line 829: It looks to be the only difference between panels a and b is the role of phytoplankton in releasing NO_2^-, with a higher rate in the more eutrophic coastal upwelling zone. At the same time, the role of nitrifiers remains unchanged. The question is: what is the potential cause for the absence of stimulation on nitrification rate to the enhanced productivity (and thus organic regeneration and substrate supply)?

Lines 859-861: On the other hand, won't the upwelling reduce the residence time of the water mass at PNM due to the replacement of water by the upwelling? Also, can you see any clues of NO_2^- supply from SNM as I expect significantly higher NO_2^- can be seen in the oxygen-depleted subsurface water in at least part of your stations?

Supplementary Line 70: What does this panel show?