

Biogeosciences Discuss., author comment AC2  
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## Reply on RC2

Nicole M. Travis et al.

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Author comment on "Nitrite cycling in the primary nitrite maxima of the eastern tropical North Pacific" by Nicole M. Travis et al., Biogeosciences Discuss.,  
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*This study by Travis et al., entitled 'Nitrite Cycling in the Primary Nitrite Maxima of the Eastern Tropical North Pacific' investigates the roles of four major processes that affect the depth and maximum concentration of NO<sub>2</sub><sup>-</sup> in the primary nitrite maximum (PNM). A suite of experimental and modelling techniques are applied for several cruises in the ETNP, and show that the depth of the PNM is correlated with water column parameters such as the chlorophyll, oxycline and nitricline depths, while the concentration of NO<sub>2</sub><sup>-</sup> at the PNM is weakly correlated. This study confirms many prior studies in other ocean locations and adds to the field by addressing characteristics of coastal/upwelling PNM formation.*

*Overall the manuscript is thorough and uses a robust combination of approaches to tease out oceanographic processes that affect PNM formation in the ETNP. The study is also well-framed and the literature review is used to give a clear context to the results. The manuscript becomes redundant at times, and the authors might be able to streamline these parts in the interest of space.*

We appreciate the constructive comments and agree with the need to streamline parts of the manuscript. We will especially focus on shortening the multiple linear regression modeling sections and moving details to the supplement.

*I found the coastal/oceanic comparison interesting and novel, but was unsettled by the lack of clear defining features used to classify each site. The authors state that coastal sites were selected based on "presence of shallow nitraclines and shallow chlorophyll maxima depths, as well as larger chlorophyll maxima and nitrite maxima. [...and] had the steepest density gradients near the observed larger PNM." While these criteria are logical, their major weakness is that they rely on the measured data in order to group the sites, and then the same sites are modeled against the same data set, making it somewhat circular. The sites should be grouped based on other criteria that are independent of the measured parameters (e.g. isobath or distance from shore).*

In the first version of our multiple linear regression model we included all the station data possible and assessed the best fit model for our entire ETNP dataset (full variable - all station model, Fig 6). This first modeling effort was unable to accurately predict nitrite profiles for all the stations, with some of the largest size errors occurring in nitrite maxima occurring at Stations 5-9, effectively the coastal stations (Table S5). We hypothesized that the controls on nitrite accumulation in the PNM might be different across stations and between regions within the ETNP, especially for stations within a productive upwelling

coastal zone.

Our coastal vs offshore categories were a result of inspecting the CTD station data and noticing patterns in the hydrography and chemical gradients of stations with the largest nitrite maxima. Using the measured data we refined the spatially-defined coastal stations into a smaller 4-station subset based on the size of the nitrite maximum, its depth, the size of the chlorophyll maximum and the depth of the nitracline. The selection criteria for "coastal" stations used in MLR construction were: nitrite maximum > 800 nM, top of the PNM feature beginning <40m depth, and chlorophyll maximum larger than 9.5 mg m<sup>-3</sup>. To build our models, we employed a k-fold cross validation method to avoid having to withhold large subsets of data for training and validation prior to testing the model against a small remaining set of field observations. In this way we have avoided the common modeling pitfall of predicting data that were used to build the model in the first place.

A general finding across all models was that the depth errors were small, while size of the nitrite maximum was not as accurately predicted. Predicting nitrite profiles at the coastal stations using the 'coastal' model is acceptable due to the k-fold cross validation methods used, and we did find that both regional models performed better than 'all station' models. However, the goal of this modeling effort was not simply to predict nitrite. We aimed to compare the resulting coefficients from a model that reflected the environmental conditions associated with larger nitrite peaks near the coast vs the conditions of an offshore station (Discussion 4.3). This comparison helped to investigate the underlying controls on the accumulation of nitrite at the different station groupings. From the relative importance calculations for each coefficient, we discerned that the coastal model nitrite predictions were influenced by nitrate and light, while the offshore model nitrite predictions were still dependent on nitrate concentrations but had more influence from chlorophyll concentration. Thus, we believe that the finding of nitrite at the 'coastal' and 'offshore' stations having different relationships to the environmental parameters is distinct from using the patterns to group the stations to begin with.

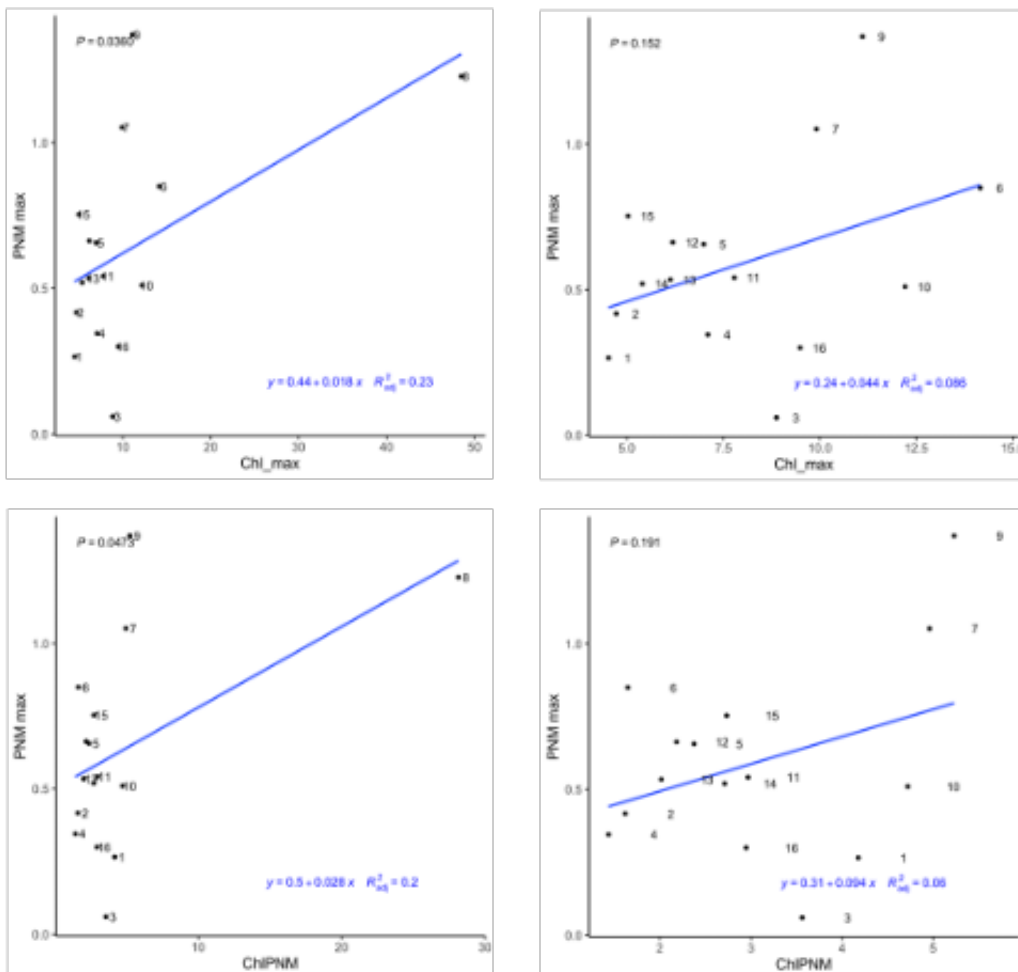
*Lines 515-519: The ability of the model to predict the formation of double PNM peaks is intriguing – the model predicted the feature at 5 sites, of which 3 showed the double peak and two did not in the field data. The manuscript would really benefit from some discussion as to the possible disconnect observed here because it could elucidate important timing or hydrographic factors that influence PNM formation. For example, do the authors believe the double peak is due to NO<sub>2</sub>- formation/consumption rates changing rapidly with depth such that the feature is too transient to consistently observe in field profiles? Is there any evidence to suggest whether the smaller double peak is a remnant of a prior peak that is degrading, or a new peak that is "growing in", (or both)? Do the authors think this is due to physical factors, like shoaling or mixing, or chemical factors that influence the biota, such as upwelling?*

Thank you for your comments. The double-peaked PNM is an interesting occurrence that is not well studied, likely because it is less commonly observed. The high resolution PPS nitrite profiles allowed us to observe small additional nitrite peaks at 4 of the 16 stations occupied in 2016 - two on the underslope side and two on the upper slope side. Prediction of additional nitrite peaks by the MLR models occurs fairly frequently, and most often erroneously, and likely reflects the fact that the models don't have enough information to predict small changes in nitrite - thus the large size errors compared to depth errors. However, double peaked nitrite profiles suggest that the environmental parameters included in the model contain enough variation across depth to introduce extra peaks in the nitrite profiles. As the reviewer suggested, the microbial rates (mediating the connection between environmental conditions and observed nitrite accumulation) may lag behind changes in water column characteristics, thus creating a disconnect with observed nitrite.

Lomas and Lipschultz (2006) initially hypothesized that rapid changes in the relative availability of light and nitrate in the surface ocean causes transition periods where double-peaked PNM temporarily exist. One peak would be the newly forming peak, and one would be degrading. We can try to investigate water column characteristics such as distance between nitrite maxima and top of nitracline, and concentration of nitrate at the nitrite maxima, to get a sense of whether the nitrite accumulation is newly forming or a remnant feature, but the age of the PNM remains difficult to discern using only this information. It is also difficult to tell if there is an age difference between the main peak and the smaller peak, and our rate measurement data is not high enough resolution to capture the smaller peak adequately. Perhaps our future investigations of nitrite age near the PNM using natural abundance isotopes will be able to provide more insight.

*The chlorophyll correlations in Figure 2a,b appear to be strongly influenced by a single extremely high data point. I wonder how the interpretation would change if the data were fit without this one point; it looks as though the slope would be quite a bit higher while still (maybe) being significant. It would be appropriate to check this and discuss model sensitivity.*

Yes, the Station 8 chlorophyll maximum was much larger than the other stations. The significance of the relationship declines when Station 8 is removed from the regression (see figure below), and it is worth adding a note in the revised manuscript to discuss the sensitivity of the results to the inclusion of Station 8, which has a disproportionately large impact on the regression line. Therefore, care needs to be taken when interpreting the importance of chlorophyll concentrations on formation of nitrite maximum as suggested by the reviewer.



*The last paragraph of the Conclusion section brings up nitrous oxide formation, yet it is not mentioned anywhere else in the manuscript. Information in the conclusion should wrap up the findings of the paper, not introduce new ideas. If desired, the authors could add a "forward looking"/"future work" paragraph at the end of the discussion that briefly fleshes out the ideas presented in the conclusion in light of their own data set, but I do not think it should be in the conclusion because it is not actually a conclusion of the work presented in this study.*

Thank you for your comment. We agree, the discussion of nitrous oxide would be more appropriate in the Discussion section. The conclusions will be revised to focus on data/results directly presented in this manuscript.