

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2022-137

Anonymous Referee #2

Referee 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 NO₂⁻ 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 NO₂⁻ 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.

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).

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 NO₂⁻ 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?

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.

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.