

General Statement

In their revised manuscript, Bianchi et al. addressed all of my major concerns. This paper is an excellent contribution to the field and I recommend publication after considering the few minor points below.

Figure 1/Appendix A: Have the authors considered a model scenario in which nitrite oxidation can occur anaerobically? Some studies indicate the importance of NO₂⁻ oxidation in apparently anoxic waters in ODZs (Buchwald et al. 2015; Sun et al. 2021).

This is an intriguing comment. In the current formulation of NitrOMZ, we do not consider anaerobic nitrite oxidation, partly because of the large uncertainties that surround this putative process. Instead, we assume that nitrite oxidizers are obligately aerobic organisms. It is entirely possible that nitrite oxidation occurs with oxidants other than oxygen; however, the actual pathways and chemical species involved remain unclear. For example, previous work suggested the potential for iodate reduction coupled to nitrite oxidation, or even nitrite dismutation. Both pathways could be incorporated in the model, although we feel that at this stage observations are too limited to properly constrain them, and we leave this work to future studies. We note now the possibility for anaerobic oxidation pathways in the revised manuscript.

There is also the possibility that some nitrite oxidation rates from apparently anoxic waters in ODZs may reflect the presence of vanishing oxygen concentrations. Bristow et al., (2016) demonstrated that nitrite-oxidizing bacteria (NOB) have a higher affinity for O₂ compared to ammonia-oxidizing archaea (AOA). This helps explain the depth distributions of AOA vs NOB, wherein NOB are typically distributed deeper and at higher abundances into the OMZ core. Buchanan et al. (2023) recently explored this relationship further, demonstrating that NOB can thrive in OMZs due to infrequent intrusions of oxygen. Thus, NOB may be adapted to take advantage of a 'goldilocks zone' of episodic oxygenation and high nitrite supply (via nitrate reduction), since their high maximum growth rates (compared with other OMZ microorganisms) allow them to grow during O₂ pulses, offsetting losses during fully anoxic conditions.

In a complementary study where we embed NitrOMZ into an eddy-resolving simulation of the ETSP, we similarly find time-averaged nitrite oxidation rates throughout the OMZ as a result of eddy-driven O₂ injections into the OMZ, which suggests that NOB growth via alternative oxidants is a complementary, but not necessary, mechanism.

To reflect these points, we added the following text to Section 2.2 “Model tracers and processes”:

Added [line 145]: *“Other work suggests the occurrence of NO₂ oxidation in apparently O₂-deficient waters (Buchwald et al. 2015; Babbin2020, Sun et al. 2021). This may involve NO₂ oxidation coupled to iodate reduction, or NO₂ disproportionation --- two poorly characterized processes. It may also reflect the high affinity to O₂ of nitrite-oxidizing bacteria (Bristow, 2016) in regions where vanishing O₂ concentrations are maintained by infrequent lateral intrusions (Buchanan et al., 2023).”*

Note that we mention iodate reduction and NO₂ oxidation coupled to iodate reduction, or NO₂ disproportionation also in the closing paragraph of the Discussion and Conclusions, where we outline future work.

Line 80 and Appendix 4: How sensitive are the model results to the assumed stoichiometry of organic matter? The C:P of organic matter in the eastern tropical South Pacific has been hypothesized to be closer to 83:1 (Teng et al. 2014) (and if the authors disagree with Teng et al. from a modeling perspective, I'd love to hear why). Changing C:P ratio should also change N:P, since C:N remains fairly consistent (Martiny et al. 2013). I know that the assumed N:P ratio can have a significant impact on N fixation rates (Mills and Arrigo 2010), although this was a very different modeling framework. On a related note, would it affect the model results for denitrification to remineralize organic matter with a different stoichiometry from aerobic respiration? I'm thinking of the Van Mooy paper showing that the stoichiometry of remineralization via denitrification may not involve PO₄³⁻ because it's mostly amino acids (Van Mooy et al. 2002).

Indeed, we acknowledge that there is potential variability in the stoichiometry of organic matter in the ocean, as the Reviewer suggests, and that the use of a fixed stoichiometry is only an approximation (although still fairly common in ocean biogeochemical models). Stoichiometric variations should not qualitatively affect our results, and certainly different organic matter stoichiometries, or even differential remineralization rates for different elements, could be incorporated in the model.

We now clarify this point in the paper both in the “Model Rationale” Section, and in the Appendix, where we added the references suggested by the Reviewer.

Added [lines 80-81]: *“although variable stoichiometry can easily be accommodated”*

Added [lines 625-628]: *“For example, studies in the Eastern Tropical South Pacific suggest a C:N ratio closer to 83:1 (Teng et al. 2014). Furthermore, organic matter degradation reactions may also differentially remineralize C, N and P. For instance,*

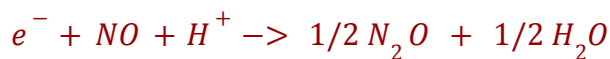
denitrification may preferentially involve degradation of amino-acids, and thus impact the N:P ratio of remineralization differently from aerobic respiration (Van Mooy et al. 2002)."

Line 343: I might be misunderstanding, but should this say "a random error of up to 20%"?

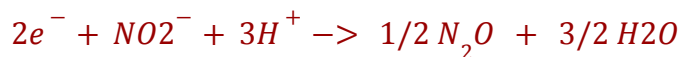
The Reviewer is correct, "up to 20%" is the correct way to describe our approach. We added this to **line 343**.

Line 627: Why only two electrons to reduce 2NO₂⁻ to N₂O?

In the stoichiometry module, we assume:



Since we do not represent NO in the current version of NitrOMZ, the summation would be represented as:



Therefore, we use 2 electrons to reduce 1NO₂⁻ to ½N₂O, which is accounted for by the sources-minus-sinks module. We added a ½ to clarify.

Added [line 637]: For denitrification, 2 electrons are required for each respective reduction step (NO₃ to NO₂⁻, NO₂⁻ to ½N₂O, and N₂O to N₂)...

References

- Buchwald, C., A. E. Santoro, R. H. R. Stanley, and K. L. Casciotti. 2015. Nitrogen cycling in the secondary nitrite maximum of the eastern tropical North Pacific off Costa Rica. *Glob. Biogeochem. Cycles* 29: 2061–2081. doi:10.1002/2015GB005187
- Martiny, A. C., J. A. Vrugt, F. W. Primeau, and M. W. Lomas. 2013. Regional variation in the particulate organic carbon to nitrogen ratio in the surface ocean. *Glob. Biogeochem. Cycles* 27: 723–731. doi:10.1002/gbc.20061

Mills, M. M., and K. R. Arrigo. 2010. Magnitude of oceanic nitrogen fixation influenced by the nutrient uptake ratio of phytoplankton. *Nat. Geosci.* 3: 412–416. doi:10.1038/ngeo856

Sun, X., C. Frey, E. Garcia-Robledo, A. Jayakumar, and B. B. Ward. 2021. Microbial niche differentiation explains nitrite oxidation in marine oxygen minimum zones. *ISME J.* 15: 1317–1329. doi:10.1038/s41396-020-00852-3

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Van Mooy, B. A. S., R. G. Keil, and A. H. Devol. 2002. Impact of suboxia on sinking particulate organic carbon: Enhanced carbon flux and preferential degradation of amino acids via denitrification. *Geochim. Cosmochim. Acta* 66: 457–465. doi:10.1016/S0016-7037(01)00787-6