

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2021-280

Anonymous Referee #2

Referee comment on "Controls on nitrite oxidation in the upper Southern Ocean: insights from winter kinetics experiments in the Indian sector" by Mhlangabezi Mduyana et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-280-RC2>, 2021

In the manuscript *Controls on nitrite oxidation in the upper Southern Ocean: insights from winter kinetics experiments in the Indian sector*, Mduyana and colleagues present strong evidence for nitrite oxidizing bacteria requiring a threshold nitrite concentration to produce nitrate in the mixed layer of the Southern Ocean in winter. Overall, the manuscript is well written, with a great set of figures, and the key findings and any associated limitations / caveats are clearly presented and thought through. Prior to publication I just have a few comments to enhance the clarity of the presentation in places.

Line 31 to 33: ending the abstract on a hypothesis / speculation seems out of place, I would suggest the authors consider instead a sentence focusing on the broader perspective of their work.

Line 39 to 41: the superscript on CO₂ needs to be deleted.

Line 96 to 98: for clarity I think it would be important to clearly distinguish between the base of the euphotic zone and the mixed layer here, it is my understanding from Lomas and Lipschultz, 2006 (and other studies) that they have found the PNM at the base of the euphotic zone which sets it apart from your work.

Line 150: Nutrient collection is not discussed in this section, so suggest you update the subheading

Line 188: the custom built on-deck incubator use for the nitrite oxidation experiments, were these fitted with the neutral density screens mentioned for the nitrate update experiments, or were they carried out in the dark – this has important implications for your findings.

Line 180 to 205: it would be beneficial for the authors to comment on (here or in the discussion) the potential limitation of only having T_{zero} and T_{final} samplings for their rate experiments, and thereby assuming a linear relationship over the incubation period (potentially missing any time lags, flattening off, or exponential activity). Also, it would be worth mentioning the reasoning behind running the NO_2^- oxidation and NO_3^- uptake experiments for very different incubation periods?

Line 237: did you directly determine the fraction of the nitrite pool labelled with ^{15}N i.e. concentration measurements before / after addition – this is not clear in the methods as currently written.

Line 362: directly related to my comment above, were these nitrite concentrations measured or assumed? This needs to be clearly documented in the methods section.

Line 440 to 444: this is a nice point, but it belongs in the discussion.

Line 486: 'low ambient NO_2^- ' can you be quantitative here? Also, how applicable are these 'low' concentrations to the rest of the ocean.

Line 598: I would add in here as you have in the figure caption that this relationship is only for the euphotic zone. With the kinetics experiments only be conducted with surface waters the question remains, how applicable are these numbers / thresholds to deeper waters in the euphotic zone (where the community might be different, different light conditions etc), and while the authors do point this out in the manuscript, I think it also needs be articulated in this section as well and the potential impacts on the conclusions discussed.

Line 612 / Section 4.3: I largely enjoy this section, there are some really nice discussion points, but in a few places this section becomes a little like a literature review and could benefit from some streamlining to focus on your findings.

Line 666 (Figure 6): is it the revised rates that are shown? In panel b are the error bars smaller than the symbols?

Line 694: why particularly NOB?

Line 744: It is not clear how this line of discussion on life strategies links to a potential explanation for the decoupling observed.