

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2022-139

Anonymous Referee #1

Referee comment on "Controls on the relative abundances and rates of nitrifying microorganisms in the ocean" by Emily J. Zakem et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-139-RC1>, 2022

Zakem et al set out to evaluate the global contribution of nitrification to global N and C cycles. The approach is to apply a previously developed ecosystem model (Zakem et al. 2018) that resolves growth, respiration and loss rates of ammonia- and nitrite-oxidizers (AOA and NOB), as well as several other important biological and inorganic nutrient components. The new addition to parameterization of the model is the recently published (Bayer et al. 2022) information on cellular C and N quotas and yields for AOA and NOB.

Nitrification rates in the model are driven by the release of NH_4 from remineralization of organic matter. It is stated that the remineralization flux in this model is larger than that produced by other models (L149), without explaining why that is so. It may be explained in the previous paper (does it result from the heterotrophic parameterization of Zakem et al 2018 and if so, how?), but it would be good to explain that briefly here, as this dependence on remineralization is fundamental to the outcome of the exercise.

Despite this larger remineralization flux, it is found that total nitrification is on the low end of estimates obtained from other sorts of models. The authors argue that their numbers are reasonable and better, because not only are the other outputs from their model reasonable, but the new quota and yield parameterizations are both realistic and data based. Their higher remineralization flux would have had the opposite effect, implying that real physiology of the microbes is responsible. How much lower would the nitrification rates have been at the lower remineralization rates of other models?

The underlying model has been published before and my expertise does not equip me to critique it carefully, so I will take it as acceptable and go from there to comment on a few other aspects of the work. I found the paper very clearly written and very readable, logically developed without redundancy. The main points were clear and generally well supported and linked directly to the calculations.

The authors emphasize some of the major outcomes of their model, which I agree are interesting and important, but perhaps not quite as novel as they imply.

-The finding that nitrification in the euphotic zone comprises up to 30% of the global total: It would be good to mention and cite Yool et al (2007) as an earlier model (which was based on a lot of actual rate measurements) that did indeed consider nitrification in the euphotic zone and found that it was very significant, providing substantial recycled NO₃ to support primary production.

-Uptake kinetic parameters are not important in determining abundances or rates in the deep ocean: That is an interesting finding, but the inverse, which they state, is even more interesting – that kinetics are important in more dynamic settings. Since the upper ocean (bottom of the photic zone) is where nitrification rates are highest, and kinetics are important there, then kinetics are important in the overall picture. Others have published plenty of data showing lack of relationship between in situ substrate concentrations and measured rates (which implies that substrate concentration is not the controlling factor). One small data set which directly supports the contention of Zakem et al here is the paper on nitrite oxidation by Sun et al (2017). They measured substrate kinetics and found that correcting for substrate affinity did not affect apparent rates below the surface layer.

L 279: I suggest actually citing a paper for the previous estimates of Global NPP. Maybe something like Anav et al. 2013 (J Climate), which has a figure showing a lot of different model estimates.

Several places in the text: I think they have the wrong Ward (2008) citation in the reference list. I don't know why they would be citing a paper about copper limitation of denitrification here.