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 NH4 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.

Good points, thank you. In the revised version, we will explain how the upper estimate of the remineralization flux at depth is due to the export flux, which yes, is a consequence of the heterotrophic parameterization originating from the 2018 publication. The parameters that dictate the rate of organic matter uptake by heterotrophic microbes combined with the sinking rate of the POM control the export flux. This parameterization is (necessarily) simple in our model, and the parameter values are uncertain, and so we chose values that gave this upper estimate of export. The magnitude matters too: there is a very wide range in the data-based and model-based estimates of the export flux (from 5-11 PgC/yr), and ours is 12-13 PgC/yr. We intentionally embraced this overestimate so that we could robustly conclude an upper estimate on global nitrification and C fixation rates. However, as you point out, this needs to be thoroughly explained in the text.

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?

Thank you: the answers to this question will be helpful to flesh out in the text. To first order, the nitrification rates at depth correlate with the exported remineralization flux in a linear fashion. Nitrification within the euphotic zone then adds to give total nitrification. In Table 3, subtracting the euphotic zone nitrification from the total matches the N-based export shows this relationship (though some nonlinear interactions between the dynamic biomass populations make it not precisely so). We will make this more explicit in Table 3 by adding lines with “Below euphotic zone” totals for NH3 and NO2 oxidation, which will roughly match the N-based organic carbon flux total above. Also, we will explain within the text that should the export flux be different, the “Below euphotic zone” nitrification totals will scale with this export, and so the total nitrification rates can be anticipated in this way. For example, if our model had a C-based export flux of 10 PgC/yr (equating to roughly 1.5 PgN/yr) then our below-euphotic zone nitrification rates would be roughly 1.5 PgN/yr instead of the 2.0-2.5 that they are now. If our C-based export was 5 PgC/yr (equating to roughly 0.75 PgN/yr) then our below-euphotic zone nitrification rates would be roughly 0.75 PgN/yr

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.

Yes, we are still struggling to highlight what we think is actually new and helpful to the community: how the differences in AOA vs NOB physiology translate into observed differences in the water column and differences in global totals. For example, the relative loss rates of AOA vs NOB have been inferred differently by different recent papers (Zhang et al 2020 and Kitzinger et al 2020). Kitzinger et al. (2020) inferred that NOB loss rates must be higher than those of AOA to explain their relative abundances. We show that because of the control by the other parameters, we don't need to invoke a difference in loss rates. The reconciliation lies in the delineation of energetic vs. DIN yield (which we explain in our paragraph beginning on line 320). Additionally, rather than just providing a number for global nitrification and associated C fixation, our analysis allows us to interpret those results and robustly explain *why* the model produces the numbers that it does: AOA C fixation exceeds NOB C fixation mainly because of the differences in DIN yield. We will try to highlight these contributions more clearly in the revised version.

-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.

Yes, thank you. It was an oversight to not reference Yool et al 2007.

-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.

Yes, the inverse is indeed interesting. One implication is that the nitrification rates in the euphotic zone are less certain than the below surface rates. Thank you for making this point. We will include this in the revised version of the paper, and also cite Sun et al 2017.

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.

Thank you for the reference. We will cite this and other estimates of global NPP.

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.

Yes, thank you, we meant a different Ward 2008. We will replace it.