Comment on bg-2022-139

Christopher Somes (Referee)

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-RC2, 2022

This study by Zakem et al. uses a global microbial ecosystem model to estimate controls, rates, and abundances of nitrifying microbes (AOA and NOB) in the ocean. Their microbial ecosystem model is based on characteristics of known AOA and NOB communities, which allows predictions of their abundances and rates to emerge in a dynamically consistent way without having to prescribe simple rate functions like most global biogeochemical models. There still seems to be considerable uncertainty in some parameters, which was addressed with an ensemble of model simulations. They use measurements on rates and yields to distinguish the different parameters between AOA and NOB functional types to best estimate their abundances and rates, using three approaches starting with a steady-state 0D model to validate the core microbial model, then with a vertical water column, and finally with a global 3D model. They find that the NH3 and NO2 oxidation rates are mostly consistent in the deep, oxygenated ocean and primarily driven by the export of organic matter to the local system. Global NO2 oxidation rates are slightly lower than NH4 oxidation due to their model predicting NOB are less competitive against phytoplankton relative to AOA. An important finding is that AOA fixes about twice as much carbon mainly due to their higher yield compared to NOB. Their model estimates a global carbon fixation rate of 0.2-0.5 Pg C yr-1 which is a small fraction of global net primary productivity.

Overall I find this to be an important and informative study on global nitrifying microbial communities and their associated rates. I think it was very well written with an ideal balance between a concise technical description and understandable results. The model results and caveats are fairly addressed and discussed. My only minor criticism is that some additional insights and discussion could be provided in the paper (see minor comments below).

Best regards,

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Minor Comments:

Figure 4: NPP and Export patterns

It is interesting to me that your global NPP rates are consistent with most estimates whereas the export is on the very high-end of the estimates. I wonder if that has something to do with relatively high nitrification occurring in the euphotic zone.

I'm surprised to see highest NPP and export rates in the Southern Ocean on the annual average, is that consistent with other estimates? If export is overestimated in the Southern Ocean, would that imply NPP might be underestimated in the low latitudes? Does export efficiency (including through the twilight zone) change significantly between low and high latitudes which could alter vertical profile total nitrification rates in different regions?

Lines 317-318: 10-30% of global total

It is intriguing to me that your analysis suggest up to 30% of global nitrification may occur in the euphotic zone. In Figure 3b, it even appears your model is significantly underestimating NO2 oxidation at the base of the euphotic zone. I'm curious about this uncertainty range as I see very little shading around the model lines in Figure 3b.

Lines 271-272: "NOB … are higher than AOA … due to anaerobic NO3 reduction"

I find it interesting that the highest NO2 oxidation rates in the global ocean occur near oxygen deficient zones. I wonder how well ODZs are reproduced and how that factors into the uncertainty given the very high rates (I think you mean Fig. 4 c and d instead of Fig. 2 here since I don't see any indication of oxygen in Fig. 2). For example, I don't see any hot spot in the Arabian Sea ODZ and there appears to be a hot spot off the North African Eastern Boundary Upwelling System that is not related to export which is typically not anaerobic.

Section 4.2:

Most global biogeochemical models estimate nitrification based on the amount of particulate organic matter (from export) that remineralizes in each location, which you also acknowledge (lines 147-148) is the main driver of nitrification rates in your model. Thus, I am not completely convinced that global biogeochemical models that do resolve microbial ecosystem functional types cannot provide reliable estimates on global nitrification rates, so perhaps you can be more specific about what you mean by "biogeochemical models that parameterize nitrification using a bulk rate constant do not provide the framework necessary for directly linking laboratory measurements to global-scale dynamics".

One important exception is nitrification occurring in the euphotic zone. If possible, perhaps you can provide some insights or recommendations about how global biogeochemical models unable to explicitly resolve microbial functional types could best parameterize this process?

Section 4.4: "first" (lines 342-344) and "third" (lines 347-350) reasons

These appear to be processes that are more realistically accounted for in your model estimate compared to previous ones. For example, you apply higher yields, but these are supported by recent observations. In my opinion, due to these two processes, this

suggests these previous estimates should be considered underestimates or a lower bound more than your estimate here is an overestimate or an upper bound.

Lines 345-347: modeled export flux is larger than previous estimates

It is still unclear to me how this error is accounted for in the uncertainty range. Earlier (e.g. line 281) you show that export production occurs between 11-12 Pg C yr-1 in your model. Is it right that your low-end of your uncertainty range for nitrification rates is driven by a model with export production at 11 Pg C yr-1? Or are the low-end rates reduced in some way to explicitly account for the fact the export production is likely too high? Since this is the clear process why your model estimate is providing an upper bound for global nitrification, I think exactly how you account for likely overestimated export production in your uncertainty range should be explicitly described in the main text. On line 149, you state this will be described in section 3.3.4, but I don't find an explicit description of this other than mentioning that export production is larger than other estimates.

Lines 353-358: comparison with Baltar and Herndl (2019) estimate

It seems to me that comparing your nitrification only estimate with a total deep ocean carbon fixation is a little like comparing "apples to oranges". I'm not familiar with that Baltar and Herndl study, which apparently provides a very large range, so I'm wondering if it is possible to infer a first-order estimate of the nitrification contribution from that study. If you believe that nitrification only accounts for a small fraction of total deep carbon fixation, is there any other specific metabolism you think may be most important to explore next?