

Interactive comment on “Quantification of multiple simultaneously occurring nitrogen flows in the euphotic ocean” by Min Nina Xu et al.

Anonymous Referee #1

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The authors present a ms that utilizes sets of linear equations (as a matrix model) to describe nitrogen transformations in seawater from 25 meter depth. The authors argue that ‘conventional methods’ for calculating rates, including nitrification, do not consider that multiple nitrogen processes are occurring simultaneously. The authors present the model, and then illustrate 2 manipulations where enriched $^{15}\text{NH}_4^+$ was added to determine what nitrogen pools it ends up in. They use the program STELLA to estimate parameters of their model. They conclude that NH_4 regeneration is likely an important process through isotope dilution, that their model can give differing results from the ‘traditional model’, and that DON is likely very important. They were able to solve for multiply occurring processes. This is an interesting ms and the community will be interested in the approach. The authors, however, need to address a number of comments to make this a more significant ms.

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The recognition that there are multiple nitrogen transformations is an important one, and the coupling of the model to an enrichment assay is a strong approach. Although I appreciate what the authors are doing here, the statement that they are the first to do needs to be amended, given the recent publication of Pfister et al. in BGS. Biogeosciences, 13, 3519-3531, 2016, <http://www.biogeosciences.net/13/3519/2016/> (“To our knowledge, this is the first and most convenient method designed to quantitatively and simultaneously resolve complicated nitrogen transformation rates, albeit with some uncertainties.”). Thus, throughout the discussion it would be appropriate to see their model compared with the differential equation model used by Pfister et al. to model multiple nitrogen transformations. The authors also need to compare their conclusions with the above study. For example, I note that this study follows processes in seawater only, while Pfister et al. includes benthic species. It would be useful for the authors to comment on the comparisons.

The ms would benefit from more direct discussion about the comparison of the models presented in this paper and other models and approaches. Nitrogen processing rates don't seem to differ much based on methodology (Table 3), with values at least being within a similar range. I find this surprising, especially given the authors' recognition of error sources (L576) The abstract states: “comparisons with conventional labeling methods are discussed” (L28) and this is too vague. Similarly, the Conclusions could be stronger and more direct.

The ms would benefit from adhering more strongly to a clear separation of methods, results, discussion. The paragraph starting L395 is a good example where this needs to be done. It might help to shorten the ms too.

Finally, although I greatly appreciate the enrichment assay, it appears to be done once. I cannot be sure based on the description given, but it appears unreplicated and that does limit the interpretation the authors can make. Starting L314, more detail is needed including how many incubations, and whether they were replicates or uniquely treated. Having the high and low nutrient assay immediately next to each other in the methods

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would also lend better comparison. As is, it looks like these assays are unreplicated and water was collected at different depths, etc.

Specific Comments Line 50, explain what is meant by the 'inventory method' Line 57 "mainly" L105, 210 – be specific about what 'new method' means L106 is STELLA a model or a method? What is meant by "The method was also validated using the STELLA model". L150 omit 'basically' L210 what is the 'incubation system'? L243 "approximately" L416 the depth of water collection for the experiment is unclear. Here it says 25 m, while elsewhere it states .3 m. L419 the final enrichment value should be given. Methods – What were the dissolved oxygen levels? Is the assumption that this is a well-oxygenated system and loss of ^{15}N as gas is irrelevant? L482 'result' Though I could read eqns 5, 6, 7, they are reprinted poorly due to some 'translation' issue. L552 – Is there good evidence for light inhibition? Many studies find high rates of nitrification with normal light. The authors do not need to comment so much on inhibitors – which they did not use. Table 1 caption – explain 'different $r\text{NH}_4^+$ variation" . What seems to be meant here is that the authors are manipulating the values of $r\text{NH}_4^+$ to mimic the effects of isotope dilution as a consequence of regeneration. Same for Table 2 caption. Table 3 – Provide a citation for the Traditional Rate Calculation (Dugdale and Wilkerson?) and cite the equation numbers used for each.

Suppl fig 1 and 2 are STELLA figs which can be confusing without equations. I did not get much out of these figs, other than the recognition that the authors used this model structure.

Throughout the ms, the authors need to check that chemical terminology is reprinted accurately. Similarly, when subscripts are used.

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