

Biogeosciences Discuss., referee comment RC1  
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## Comment on bg-2021-149

Anonymous Referee #1

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Referee comment on "Biogeochemical controls on wintertime ammonium accumulation in the surface layer of the Southern Ocean" by Shantelle Smith et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-149-RC1>, 2021

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Review of Smith et al: **Biogeochemical controls on wintertime ammonium accumulation in the surface layer of the Southern Ocean (Biogeosciences)**

The authors present an interesting and thought-provoking study on the dynamics of ammonium cycling in the southern ocean. There are some nice new observations reported here building upon previous work by the UCT group, with the authors presenting a novel scenario for the seasonal accumulation and utilization of ammonium in surface waters. The paper however can be made more succinct, particularly in the discussion, which at times speculates far beyond what can be demonstrated from the presented data or confidently gleaned from the literature.

Comments:

Introduction – Nicely sets the premise of the study but could be shortened and modified to avoid overly descriptive presentation of key concepts. Are Lines 107-123 really required in the intro? L107-152 could perhaps be more succinct but do clearly present the working premise behind the study

Methods – Some care and clarification needed in presentation of methodological details.

L177-178: Can you add approximate irradiance depths here?

L184-185: How many replicate samples were collected?

L204: Please quantify <sup>13</sup>C addition and (presumably) your working assumption of ambient DIC concentration.

L213: Were (replicate) 2L bottles used as for NPP incubations? If not please add description

Section 3.2.1 – Please clarify the method, particularly the time period samples were left frozen for prior to analysis. L236 Clarify if the same sample was measured 3 times or whether you measured 3 replicates once each.

L233 – Please clarify procedures. How long were samples frozen?/ How long after sampling were samples analysed? What impact could this have had on NH<sub>4</sub> concentrations? Freezing NH<sub>4</sub> samples is not ideal as it may lead to problems. See Degobbis (1973) (On the storage of seawater samples for ammonia determination ), though note there is no

Uptake rates – Based on L205/216 the incubations for N uptake were <6 hrs in length. However, by converting the short duration incubation length to day fraction in equation 1 the authors are extrapolating their results. Implicit in this approach is extrapolation to 24 hours from <6 hourly uptake expts. How confident are the authors that the measured rates of NPP and N uptake remain constant over the day/night cycle? Is it realistic to assume NPP or N uptake continues at the same rate around the clock? How could diel variations in N uptake impact the results? Could this extrapolation be one reason why the NPP rates are higher than reported previously (L397-399?)

L682-683: The authors conclude that NH<sub>4</sub> production must be high, which in turn indicates that the NH<sub>4</sub> uptake rates are likely biased by isotope dilution within the incubation and that final rates are thus low. This is not corrected for (nor apparently can it be given the experimental work undertaken) but needs to be formally acknowledged somewhere as it is a significant caveat for the overall discussion.

Units: Throughout the paper the authors report uptake rates as nM d<sup>-1</sup>. One assumes shorthand for nmol N L<sup>-1</sup> d<sup>-1</sup>, but please clarify volumetric basis somewhere in methods section.

L286: Why not also present specific uptake rates for all nutrients individually?

Eq3 : Reliance upon specific uptake rates to calculate the f-ratio (rather than actual uptake rates) can lead to errors and/or inconsistencies with other studies. F-ratio values are very occasionally stated in the text and not in Table 1. Please include an indication in table 1

L294: An error of 4-8% when urea is included/excluded seems very low. Usually this would be higher – see Wafar et al 1995 (Wafar, M.V.M., P. Lecorre and S. L'helguen (1995). *f*-ratios calculated with and without urea uptake in nitrogen uptake by phytoplankton. Deep-Sea Research Part I 42(9), 1669-1674.)

L300: Correct/clarify oxidation rate units (i.e. nM d<sup>-1</sup> = nmol N L<sup>-1</sup> d<sup>-1</sup>)

L306: As above, does extrapolation from short term incubations to daily rates have an implication for the results? I.e any diel variability to consider?

L311: Is a 20 ml subsample a sufficiently large enough volume to accurately enumerate from? Why not settle and analyse the whole 250 ml sample?

L353: Any difference in the SST gradient between Leg S and Leg N?

Results- Generally clear and understandable but the section does have a tendency to

seway into discussion. Present only the facts, leave the discussion to the Discussion section.

Discussion - The discussion is extremely long and should be shortened. Large parts of the discussion read like a literature review, presented solely for the purpose of supporting the seasonal scenario outlined by the authors and not necessarily to contextualise the observations presented here. Fundamentally, this paper presents NH<sub>4</sub> uptake and NH<sub>4</sub> oxidation rates from a single cruise, whereas the discussion incorporates additional cruise data and relies heavily upon literature observations to establish a new, somewhat speculative, view of NH<sub>4</sub> cycling in the southern ocean, many aspects of which are not supported by data but only by assumption of their significance. Some sections e.g. section 5.3 can probably be deleted in their entirety as they go way beyond a discussion of the data presented here and stray into unnecessary speculation. The discussion section could probably be a standalone opinion paper or the premise for a proposal in its own right. Much is unsubstantiated (though not necessarily wrong) and some references used derive from rather different subtropical environments where the relevance of NH<sub>4</sub> and urea can differ markedly.

L520: Based on Fig 4c alone it is not possible to say how long d15N conditions lasted

L726-733: Without supporting observations this is speculative and unsupported.

L737-747: Relevance? Without any supporting measurements its just not possible to ascertain validity of the statements presented here

L748-762: Relevance? No measurements of NH<sub>4</sub> input are reported here so the connection is unclear.

Summary – L900 – Measurements of heterotrophic NH<sub>4</sub> production rates are required to support the speculative scenario outlined in the discussion, not your conclusions

L911: Use of references in a 'summary' section. Not something I agree with personally. The statement being made here should be moved to the discussion.

Figures – Generally clear and readable

Fig 5: Please clarify what the black box in the legend represents (dark > 2.7  $\mu\text{m}$ ) or remove.

Fig 5b: Difficult to read, may need resizing. Please also clarify % uptake by the 0.3-2.7

um size fraction (as per Fig 5a)

Fig 6 & Fig 7: Just getting to be too small to clearly read fonts etc. Please resize

Fig 8: If authors retain the long and extensive discussion then perhaps addition of approximate rates and pool sizes to Fig 8 will help.