Biogeosciences Discussions

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Comment on bg-2021-149

Anonymous Referee #2

Referee comment on "Biogeochemical controls on 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-RC2, 2021

General comments.

The authors present a study of ammonium cycling activity during an oceanographic cruise between Cape Town (South Africa) and the Marginal Ice Zone of the Southern Ocean. This is achieved through ship-based experiments. The authors present data that describes DIN concentration, N-assimilation and ammonium oxidation rate data. The manuscript is generally well written, citing appropriate refences in support of the arguments presented. The study addresses an area where there is genuinely very little information. The data is high quality and the insights are therefore valuable. I found the text expansive throughout; topics introduced and discussed are mostly relevant, although the level of detail frequently detracts from the focus of the study. In my view the manuscript would benefit from a considerably sharper focus of the main insights achieved, with less space/reliance dedicated to speculation and inference.

Specific comments.

L18-37 - Abstract, and the manuscript more broadly. My view is that the abstract should focus on the new information/data presented, rather than drawing upon additional data sets to support the conclusions. The data certainly offers support for the implication presented in conclusion of this manuscript (that the Southern Ocean is a net CO_2 source for half of the annual cycle). However, my view is that the authors over-reach the scope of their data to draw this conclusion.

[Introduction]

L38 - Introduction - very detailed scene setting, perhaps overly so. Topics introduced are relevant, but expansive. The introduction would benefit from a tighter focus.

L39-49 - In the first paragraph there are 11 references. 7 of these are not in the reference list. I didn't continue to check, but this needs to be done.

L150-152. While the stoichiometry of nutrient assimilation into organic material is relatively clear, I'm not sure that the stoichiometry of CO_2 release per NH_4^+ regenerated is as clear. Does it then follow that the SO may be heterotrophic for half the year? Maybe this hypothesis needs some additional support?

[Methods]

L164 – I am curious as to why the sampling regime differed between the two legs of this cruise - Southward leg involved only surface underway samples, while the Northern leg included CTD casts. How might this inconsistency between legs affect the study (I suspect this difference related to ships logistics; I may have missed this information).

L171 – rosette mounted oxygen sensors are notorious for drifting. Was this unit calibrated?

L177-178 – are the authors confident that this difference in sample collection methods did not influence the results presented? My concern would specifically be with using the ships underway system for ammonium measurements. The pipework in these systems are far from clean (by scientific standards) and offer an extensive surface for biofilm formation. Microbes growing in such films rapidly exchange resources with their surroundings (i.e. the surface sea water supply), potentially modifying nutrient and gas concentrations. Separately, physically pumping water is likely to disrupt biological processes associated with cells/aggregates in the pumped water (pressure pulses/turbulence). Was a direct comparison between sampling methods done on concentrations/processes?

L204 – was the DIC concentration measured to confirm this enrichment, or is it assumed?

L210 - collected from ships sea-water supply – depth of 7m

L215 – presumably these concentrations were measured post-cruise to allow accurate enrichment determination? Worth stating this.

L217 – If I understand this correctly, a constant $^{15}NH_4$ addition of 100nM was made to NH_4^+ oxidation vessels. In Fig 2, the ambient concentration was 0-0.7µM, representing an enrichment of 12-50%. If so, this could lead to a potential overestimation of nitrification rate.

L217 – why duplicate analysis (presumably logistics – I appreciate this is a lot of work)? This could limit the confidence in the statistical analysis of results.

L218 – I appreciate that carrier N needs to be added under certain circumstances in order to satisfy detection limits of analysis. However, this leads to a loss of sensitivity. Are the authors confident that their measurements were sufficiently separated from the inherent 'noise' of mass spec isotope analysis? Is the ambient NO2 data presented anywhere – I couldn't see it.

L231 – I suspect that surface seawater supply pipework is a higher contamination risk than sample invasion due to temperature gradients…..

L269 – Are the authors confident that system drift was not an issue during the analysis of so many samples? From personal experience, and that of colleagues who undertake isotope measurements, batch runs of as little as 15 samples, but generally no greater than 25 are used, punctuated by standards and filter blanks. I am not familiar with system described but would be surprised if any MS system was sufficiently stable for a run of this length.

L281 – again, was DIC measured directly? Perhaps I missed this….

L306 – evidence from elsewhere would support this assumption, but a short consideration of alternative NO2 sources would perhaps be useful. In a well-mixed environment with a sufficient NO3 supply, light transitions can lead to NO2 release from phytoplankton (as well documented). By undertaking bottle incubations, such transitions do not take place thankfully, so this mechanism shouldn't influence results (i.e. by diluting the enriched NO2

pool leading to an underestimation of NH4 oxidation rate).

L315-341 – AFC is a relatively standard analytical tool. The level of detail provided is unnecessary – refer to published methods.

L343 – 'potential heterotrophic activity *was* evaluated… '

L342-350 – I doubt this approach can yield useful information about the NH_4^+ regeneration rate. Cell abundance (any type of cell) is no indication of cellular activity. While particulate material is decomposed leading to the regeneration of inorganic nutrients, the more labile material is likely to be associated with the dissolved organic pool, especially the material actively released from living phytoplankton cells during growth. I appreciate that the authors wish to get a handle on this aspect in order to build a view of regional NH_4^+ cycling, however I think this stretch detracts from the dataset.

[Results]

L398 and onwards within results section – refs and associated text into discussion.

L1500 - Is it necessary to name the software package used to generate figures? I find the figures and their text on the small side, especially Fig 2, 4 and 5 (the latter has a great deal of information and appears rather cluttered), 7, 9.

L1532 – co plot of cell abundance with [NH4] – what's the rationale for the co-plot? Is there a link suggested or is it to provide context?

L462 – '….food source available to heterotrophs…' this statement is somewhat vague. Heterotrophs would include everything from heterotrophic protists to zooplankton and beyond. What is specifically referred to here?

[**Discussion]**

L470-496. While it is important to try and constrain the factors that are significant to NH_4^+ cycling, my view is that there is too much reach beyond the data. It is not robust to infer process rates from cell or detrital abundance data. The foundation of the paper is the observational data surrounding NH4 assimilation and oxidation, and the new insight this provides. My main criticism of this contribution is that it reaches well beyond this data, to inferred contributions and speculation, to build the view of NH4 cycling. While this view may ultimately be proven to be reasonable, I think a stronger case needs to be made through direct observations of the inferred processes and rates.

L505 – '…growth temperatures of temperate and…' attention.

L509 '….and west Indian…' West.

L589 – '…could dampen total…' Not sure what this means.

L639 – 'supporting role for iron…' This is speculation.

L659-662. Both NH4 and NO2 are intermediates in a number of microbial processes. It would be difficult to infer anything about how one process influences this balance.

L687 – The bacterial decomposition of DON leads to NH_4^+ regeneration. i.e. not just PON.

L692 – 'fresh' PON – specifically, do you mean labile material that can be readily decomposed?

L699 – this is speculation – what support is there for this statement?

L705-707 – what support is there for a link between the ratio of detrital to heterotrophic particles and the NH4 concentration?

L716 – ‘bacteria more efficient at lower temperatures..’ efficient at what? This is loose language.

L817 – I do not follow this. Is this specifically referring to grazing activity? Bacterial activity is predominantly heterotrophic and will most certainly be taking place here.

L831-856 – Is this section necessary? This aspect was not directly investigated (it needs dedicated spike experiments). This is an example of discussion and speculation that add little to the manuscript.

L857-886 – the manuscript now strays a considerable distance from the focus of the study. My view is that this section adds nothing to the discussion of the results.

L915 – Having read through the discussion I find it hard to pull out the headline from this study – there are steps forward, but they need to be stated more concisely, with less speculation and inference.

Technical corrections

L95 – ‘consumption’ and ‘assimilation’ are used interchangeably. I’d associated consumption with phagotrophy/mixotrophy/grazing. Assimilation is technically more appropriate here as the underlying process referred to is inorganic nutrient utilisation by

phototrophs (nutrient uptake, reduction where necessary and assimilation into organic molecules).