

Interactive comment on “Stable isotopic evidence for high microbial nitrate throughput in a High Arctic glacial catchment” by A. H. Ansari

Anonymous Referee #1

Received and published: 10 May 2016

General comments

This manuscript uses mass balance and isotopic analyses to examine the role of microorganisms in transforming nitrogen in proglacial streams running from a glacier in the High Arctic, Svalbard. As such, it is within the remit for Cryosphere. As seen by the similarity in titles, it is very much a follow up paper to A. Ansari's previous Biogeochemistry paper from 2013 'Stable isotopic evidence for nitrification and denitrification in a high Arctic glacial ecosystem'. The key question is whether this new paper adds sufficient new insight into glacial nutrient transformations to enable publishing in Cryosphere.

In summary, the data extends the dataset of the 2013 paper by:

1. Studying the identical suite of analyses (geochemistry , NO₃- isotopic values and

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discharge) of snow and proglacial streams in another year (2010 compared to 2009) 2. Studying further sample sites downstream from the first proglacial sampling points (in contrast, the 2013 paper also studied upstream supraglacial sites).

The interpretation extends the 2013 paper by:

1. Tabulating the amount of excess/depleted NO₃⁻ in proglacial waters relative to snow-pack NO₃⁻, by normalizing against the conservative tracer Cl⁻ (in contrast, the 2013 paper discussed this briefly in the text, finding a small xs rather than small decrease in NO₃⁻)
2. Using a more detailed isotopic model to attempt to quantify the relative rates of microbial assimilation and nitrification (in both papers the % NO₃⁻ produced along the transects is produced).

Overall, I do not find the findings of the new manuscript sufficiently strong or original to merit publication in Cryosphere in its present form, although the data, with additional analysis and interpretation, certainly can be published, although where will depend on the strength of new analysis and combination with previous papers (particularly the 2013 paper mentioned above). Here are my principal reservations/questions and suggestions:

Specific comments

1. The principal new conclusion that the new paper makes (based on isotopic mass balance calculations) is that there is (according to e.g. the abstract) 'fast in-stream recycling of assimilated NO₃-N' with 'overwhelming amounts of NO₃-N production and assimilation reveals a hitherto unknown level of microbial processing in the Arctic glacial ecosystem'. I have the following issues with these conclusions:

a) In the prior 2013 paper, the author (I think correctly) states that "...until our understanding of isotopic fractionation and exchange improves (a requirement for laboratory investigation that is beyond the scope of the present study), some caution is required

when interpreting Fig. 5 (analogous to part of Fig 6 in the new paper). Yet in the new paper, this caution is ignored with the calculation of exact relative rates of assimilation and nitrification, with errors based on the precision of measurements (Appendix) rather than the almost certainly far larger yet unknown errors for the different fractionation steps themselves. Without adequate knowledge of the isotopic fractionation steps in microbial glacial systems, I don't see how such precise estimates can be made. How does the author justify this change in approach, given no further insight into these issues since his previous paper?

b) Fast in stream recycling of $\text{NO}_3\text{-N}$ (presumably through NH_4^+ or organic N back to $\text{NO}_3\text{-}$) will have different isotopic effects for the N and O isotopes. For O, the signature will be derived from either water or O_2 , and hence is not a closed system. For N, he is assuming (e.g. for complete recycling) a closed system. In this case, he will need to take into account Rayleigh fractionation in a closed system rather than the more simple mass isotopic approach he has taken.

c) Before recycling of $\text{NO}_3\text{-}$ can be assumed (as in abstract), the possibility of independent $\text{NO}_3\text{-}$ sources need to be taken into account, in particular from dissolved NH_4 , and DON (and particulate N, which was not measured). Rather than just focus on an excess or depletion of $\text{NO}_3\text{-}$, a dissolved N mass balance should be carried out and included in Table 1. Is there an actual increase or decrease in total dissolved nitrogen? Can all the excess $\text{NO}_3\text{-}$ be from e.g. oxidation of DON or NH_4^+ rather than recycling?

d) It appears to be assumed throughout the manuscript that all microbial induced changes take place within the stream itself, by which I assume that the author means the water column. What about the pore water in underlying sediment? What about the potential for any lateral shallow groundwater transport? In line 116 you state that you 'believe that . . . discharge. . . remains unchanged from MLW3 to MLW1. . .'. Without having measured comparable discharge at downstream sites, is it possible to disregard the possibility of shallow groundwater flow influencing geochemical and isotopic data?

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2. There is insufficient comparison to the similar 2013 paper. This starts in the introduction, with the statement ‘it has been generally considered that due to low temperature biotic impacts on nitrogen cycling in these streams have low quantitative significance’ – this in direct opposition to his own 2013 paper where he states the remarkably similar conclusion that microbial processes produce up to 95% of the nitrate in the same streams. While I realise that the author is trying to differentiate between the studies, a stronger approach would be to directly combine and compare the results. The same overlapping datasets exist in 2 seasons, but apparently show some subtle seasonal differences e.g. the 2009 data shows an apparent NO₃⁻ excess, while the 2010 new data shows slight decrease. The 2009 data shows additional data for denitrification, the 2010 does not. Why? From reading both papers again, the largest isotopic changes may occur close to the glacier (from supraglacial waters to proglacial, rather than proglacial downstream changes documented in the submitted manuscript), again why? Comparing datasets and looking at these subtle differences might potentially produce some more robust novel insight into N cycling.

3. The author uses isotopic values for snow as comparison to the proglacial streams, but presumably ice melt will have an increasing contribution as the season progresses. Were the isotopic values and concentrations of ice melt measured, and how would these affect the interpretations?

Technical comments

1. The detail given for the denitrifier method is not necessary, a similar approach should be used to the 2013 paper where basic details are given with citation.

2. The abstract needs substantial reworking. For example, it does not give the site or country where fieldwork took place.

3. I found the graphs in Figure 3+4 hard to read, with too much information. It would be clearer to e.g. separate the isotopic from compositional data and put them side by side. They should also have error bars on, or where they are within the scale of the

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symbol it should be stated. Compositional and isotopic values for snowmelt (and ice melt if available) should be plotted on the y axis, as this will clearly show the proportion of changes that occur in supraglacial to proglacial environments.

4. The 'S' columns in Table 1 should be immediately adjacent to their respective proglacial analyte to help readability. A new column should be also added for total nitrogen (sum of NO₃⁻, NH₄⁺ and DON) to enable a better mass balance estimate to be made.

5. Data from 2013 and new data should be combined in Figure 5 to aid seasonal and spatial interpretations.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-59, 2016.

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