

Interactive comment on “Sources, cycling and export of nitrogen on the Greenland Ice Sheet” by J. L. Wadham et al.

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This review was jointly written by Amy Ritchie, Jenny Davidson, Sally Jenzen, Peter Foster and Cameron Campbell, students at the University of Edinburgh, with some input and supervision from Dr Ed Mitchard.

— The authors present a novel dataset – we believe it is the first time that nitrogen export from a glacier in Greenland has been quantified. We believe this is an important topic, with relevance for the wider environment especially in the context of rapid climate change in this region. The methodology followed appears sound, and the dataset presented is certainly useful and provides a step forward in information on the Greenland Ice Sheet. However, the extrapolation to the whole Ice Sheet may be questionable, and we have some five major specific concerns that we would like the authors to address,

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detailed below. Further, we have some specific, more minor comments.

1) Introduction: While the introduction to this paper is adequate, it is very short and we have identified some potential improvements that could be made. We feel the introduction is somewhat brief and lacks explanation for why the study site was chosen nor the background behind the study and the methods used.

Specifically, you state that the paper will examine the sources and cycling of nitrogen on the Greenland Ice Sheet. However, while you state that your study site will be the Leverett Glacier in southwest Greenland, and refer to it throughout, there is no evidence as to why this particular area is representative of the whole of the Ice Sheet. It would be beneficial to include some evidence of the Leverett Glacier's importance in the context of nutrient cycling in Greenland, and why this site was chosen. It would be useful to put a sentence in the Introduction or Study Site Description giving the proportion of the Greenland Ice Sheet that is drained by this specific glacier.

Not only this, but it would be beneficial to provide more context and relevance of the general issues explored in the paper. You state “there is comparatively little data on nitrogen sources and cycling on the Greenland Ice Sheet, which is likely to be important as a nutrient source to downstream fjord and marine ecosystems.” While this provides some background to the specifics of the Greenland ecosystem, this should be set in terms of the global importance of nutrient cycles, with reference to climate change. In this context, it would be beneficial to refer to studies that have looked at nitrogen transport more widely, and to paint the broad context as to what could happen to nitrogen flows into the ocean under rapid climate change.

We feel further that the specific aims of the paper are not clearly set out. An additional paragraph at the end of the introduction setting out the rationale and aims of the paper, and setting in a global context, would be useful.

2) Sampling, Extrapolation and Confidence Intervals: Many of the results (from “mean nitrate concentrations” in section 3.1 to final TN fluxes from the GrIS in 3.3) lack un-

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certainty values and/or confidence intervals. There is also a general lack of statistical analysis throughout the results section, in particular related to supposed trends seen in e.g. Figure 5. As this study involves extrapolation from a single year of measurement from a single, small, site, the confidence intervals will inevitably be large: this makes it all the more critical they are estimated.

3) Discharge volumes: for the calculations and extrapolations to be valid it is essential we understand the volume of glacial runoff over the melt season. The paper omits raw measurements of water volume, which would enable a better understanding of how nitrogen flux values were derived. Only a singular value of water volume arising from Leverett Glacier is provided in the paper (Table 2). This is reported as an annual measurement for 2012, despite field work occurring over the summer period only. A more comprehensive account of the measurements taken to reach the reported figure of 2.2km³ a⁻¹, and whether this is a total actually measured or a value extrapolated from limited measurements, would hence be beneficial. Again confidence intervals would be useful here, and an idea of how much this might vary from year to year could be included in the discussion.

The paper would also benefit from expanding and enlarging the bulk discharge (output) time series (Figure 6) and presenting the continuous stage-discharge (moulin meltwater) time series (SI Figure 4) in the main article. This would enhance the interpretation of results arising from each flow measurement. Finally, the number of discharge measurements and water samples taken in the study should also be included, to improve our understanding of the methodology.

4) Sampling sites: Whilst section 2.1 adequately introduces the Leverett Glacier and its relationship with the Søndre Strømfjord, the references to Figure 1 are not well matched (NOTE: the fjord is incorrectly spelt as “Søndre Strømfjord” in text and in Figure 1). In Figure 1, the Danish name for the fjord “Søndre Strømfjord” has been replaced with “Kangerlussuaq Fjord”, yet is still referred to as Søndre Strømfjord in the figure caption. Furthermore, the figure caption references “the two other major

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runoff sources to the fjord...”, which omits the Watson River. Also, the sampling site indicators are not well described: “white dot” for LG could be confused with the River locations; “red dot” refers to a red star.

We suggest that this may be resolved by changing the caption to: “Figure 1. Map showing the study area, including the location of the Leverett Glacier runoff sampling station (yellow star) and the surface sampling site (red star), together with the Watson River and the two other major runoff sources to the Søndre Strømfjord (Umivik River and Sarfartoq River).” This would require altering the map itself to rename the fjord and add the yellow star for the LG sampling station.

On a similar note, whilst the locations of the surface and bulk meltwater sampling stations are included in Figure 1, the location of the basal ice sampling is not. Not only should this be included, but more detail on the sampling site should be given in section 2.2.3. It is assumed that the margin of the glacier is used due to difficulties in accessing more pristine inner basal ice, but this is not directly stated. Also, there is no mention of the possible variation in margin basal ice and interior basal ice. These factors should be briefly discussed to assure the reader that no significant confounding factors exist – and again if they do perhaps an analysis of uncertainty could be included with confidence intervals placed on the resulting estimates.

5) Figure 5 is unclear and we believe, as presented, very difficult to interpret. We suggest several changes:

- We believe some kind of smoothing is needed, as there is clearly a lot of noise in the datasets and seeing trends through time is difficult. For example there appears to be a significant outlier in the NH₄⁺ trend in live anaerobic – either this is real and should be commented on, or more likely it is noise and interferes with seeing the relationships. We suggest either fitting smoothed lines and making the individual points on the graphs smaller, or binning the x axis into three time ranges (say 0-300, 300-600, >600) and showing the results as means (with CI's or box/whiskers) over those periods.

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- The y axes on all four sub-figures should be the same – we believe this would make the comparisons clearer
- We still believe, with these corrections, the figure would be hard to interpret. We wonder whether the use of ratios between the points would be helpful?
- The colours used here are indistinguishable in black and white. We recommend the use of symbols, e.g. triangles, circles, crosses and squares, to allow the figure to be understood in a black and white printout.

Further more minor points:

We feel that although the applicability to the whole of Greenland is reasonably well justified, it may be overstated. It is strange that in the conclusion, the “Greenland Ice Sheet” is continuously mentioned when the findings are specific to the Leverett Glacier. We would prefer if in many points through the text Greenland Ice Sheet is replaced with ‘Leverett Glacier’, as it is to this that the findings can be known to conclusively refer.

Figure 2 – the legend to this figure does not state what the grey periods on the left hand graphs refer to. Further, the datapoints are too large and obscure the underlying lines, e.g. in 2a)

Figure 3 – the graphs are too small, if the size could be increased (and data points shrunk) more information could be garnered from this figure. We also wonder if it might benefit from a logged x-axis – possibly both could be displayed side by side? This might allow the cluster of lower values to be more clearly observed.

Figure 4 – the line fitted here does not seem to match that in the text (which describes the intercept as 2 ± 0.6). The intercept in the graph appears to be approximately 2.5. Also given the errors on both axes we wonder if RMA/SMA regression would be more appropriate here anyway.

Page 5 lines 12-16: moulins should be defined, cryoconite debris better defined (“summer ice” not sufficient), and cryoconite itself could be better explained. A figure with

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pictures could be helpful here?

Page 6 line 7 – the dye technique is not well described – we believe this is a standard technique but a reference should be provided.

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