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Biogeosciences Discussions

Interactive comment on "Microbial nitrogen cycling on the Greenland Ice Sheet" *by* J. Telling et al.

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We would like to thank the anonymous reviewer for the constructive comments. We agree with the reviewer that the manuscript would be strengthened by omitting the more speculative 24 hr nitrogen flux mass balance estimates and focusing more on the more concrete geochemical and microbiological data, and on the relative inputs of nitrogen to the surface of the GrIS. While we stand by the basic approach of the 24hr mass balance calculations, on reflection we find that the large uncertainties in some of the parameters (in particular for ablation, mass of cryoconite and cryoconite coverage) likely do not justify the amount of space given to the calculations in the discussion. These uncertainties largely stem from the very limited amount of time available for sufficient physical field measurements at each site. This is the reason why

we decided to use the average cryoconite coverage and mass over the entire bare ice zone, however we take the reviewers point that this approach weakens the robustness of the spatial trends. Plus with hindsight it would have been far better to take several absolute manual measurements of ablation at each site using ablation poles rather than rely on the automatic weather station network for which unfortunately only very limited data was available for the period of the transect measurements, leading to the need for the linear regression of limited ablation data.

Where we disagree with the reviewer is that the calculation of NNEP (via Eq 3) is "so imprecise that it is hardly worth doing", although we believe this may stem in large part through a misunderstanding of how NEP was actually measured. The NEP (net ecosystem production) data was derived from direct measurements of CO2 uptake in closed bottle incubations incubated within the cryoconite holes (see Stibal et al., 2011). The data is normalised to units of μg C uptake g-1 dry sediment, using the measured mass of cryoconite in the bottles, and having carefully replicated the in situ thickness of cryoconite sediment of each hole within the bottles. As such, the NEP measurements are a direct estimate of net carbon uptake by the cryoconite hole communities that already takes into account losses by respiration. We do not therefore have to correct for growth efficiency [i.e. microbial production/(microbial production+microbial respiration)] as we already take into account the carbon lost by respiration. Nor do we have to include estimates of carbon content or cell size, since they are again superfluous for this type of geochemical flux measurement. Perhaps the reviewer is confusing the NEP data with bacterial production data (e.g. by the leucine method) for which this additional information would clearly be needed? In the revised version we shall add an additional sentence to make the definition of NEP clear, and also define it properly the first time it is used in the text.

We do agree with the reviewer that a remaining uncertainty in the NNEP calculation (Eq. 3) is the ratio of C:N uptake within cryoconite holes. In the absence of further data, we chose to use the Redfield ratio. As the reviewer points out there may be

significant departures from the Redfield ratio depending on the type of organism and environmental conditions. In the revised version we will add a short paragraph outlining the caveats of using the Redfield ratio adding some key references (Reiners, 1986; Stal, 2000), although in the absence of other data we still stand by its use as a reasonable best estimate of the C:N of cryoconite microorganisms. Indeed, the Redfield ratio has been commonly used as a reference for C:N ratios in previous supraglacial studies (e.g. Barrett et al., 2007, Stibal et al., 2008).

We propose to delete Figure 5 (the 24 hr mass balance estimates) and references to it (Section 2.5, 3.4 and sections in the discussion) from the manuscript. Instead, we will include a heavier focus on annual estimates of N fluxes on the GrIS surface. We propose to extend the calculations shown in Fig. 8 as follows. First, we will divide the transect measurements into the three ecological zones proposed by Stibal et al 2011 namely: 1) Marginal zone 2) Bare ice zone and 3) Slush zone. Second, for these three zones we will calculate the total estimated annual nitrogen fluxes from, a) precipitation b) icemelt (using annual ablation data from 2010 for the transect shown in Fig 2a (Figures 2b and 2c will be deleted), plus additional annual ablation data from 2009, and the mean total nitrogen content of ice from each zone) c) Nitrogen fixation d) net N uptake by cryoconite ecosystems (NNEP). All units will be normalised to kg N km-2 y-1 to allow easy comparison with the previous studies of nitrogen fluxes on Svalbard valley glaciers by Hodson et al 2005 and Telling et al 2011. To increase the robustness of the dataset we will also include previous GrIS NEP data from Hodson et al 2010 and previously unpublished NEP and N2 fixation data from the 2km adjacent site on the GrIS earlier (June/July) in the 2010 season. Using this approach, the relative sizes of the estimated annual nitrogen fluxes on the GrIS will be more clearly demonstrated.

Response to other minor comments not covered by above:

Page 10428 line 25 – we will add a caveat here that the N fluxes may be under or overestimated if the 24 hr measurements are non linear 10430 line 6 – we will define NEP 10433 (Fig 3) – we will rename DIN to NO3- Page 10434 line 6 – TOC comes

from Stibal et al. (2010), as shown in Table 2. We will also state this in the main text in the revised version.

Additional references Barrett, J.E., Virginia, R.A., Lyons, W.B., McKnight, D.M., Priscu, J.C., Doran, P.T., Fountain, A.G., Wall, D.H., and Moorhead, D.L.: Biogeochemical stoichiometry of Antarctic Dry Valley ecosystems, J. Geophys. Res, 112, G01010, doi:10.1029/2005JG000141, 2007. Reiners, W. A.: Complementary models for ecosystems, Am. Nat., 127, 59–73, 1986. Stal L.J.: Cyanobacterial mats and stromatolites, The Ecology of Cyanobacteria (Whitton BA, Potts M, eds), pp. 61-120. Kluwer Academic Publisher, Dordrecht, 2000. Stibal, M., Tranter, M., Telling, J., and Benning, L.G.: Speciation, phase association and potential bioavailability of phosphorus on a Svalbard glacier, Biogeochemistry 90, 1–13, 2008.

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