Biogeosciences Discuss., 8, C4672–C4674, 2011
www.biogeosciences-discuss.net/8/C4672/2011/

Biogeosciences Discussions

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

Anonymous Referee #1

Received and published: 5 December 2011

The topic of the manuscript is very intriguing and exciting! The authors are right that we know virtually nothing about N- cycling in Gletcher-ice and in this respect the present contribution is very welcomed. However after reading the manuscript I was a bit disappointed about the actually progress that the study has provided. The Introduction summarizes the aim of the paper: 1) Test that there is active microbial N cycling on the GrIS by measuring nitrogen fixation (comment: In reality there also would be N –cycling without N fixation!) 2) Quantify the relative nifH-gene abundance and nitrogen chemistry over a 79 km long transect 3) Estimate the relative importance of N-fixation to the total N input to GrIS and to what extent this fraction support the net microbial growth. The present manuscript heavily rely og NEP values (and other information) from Stibal et al 2011. I suspect that the novel aspect (measured N2 fixation rates and maybe measured DIN levels) of the present manuscript could easily have been incor-

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porated in the context of Stibal 2011 omitting the very speculative budget calculations . Whereas as I find 1) and 2) justified and covered by the manuscript. I remain very sceptical towards some of the calculations (see below) and in my opinion the procedure must be better justified (or removed) before publication. I find that Fig 4, 5 & 8 justifies publication (but they could most likely have been included in Stibal et al 2011?)

Specific comments: Page 10428 line 25 – That another study has documented a linear development, does not mean that this also was the case at your settings! Page 10430 line 6 – define NEP- first time used! Page 10430 line 10 – Do you mean DIN (aq) or DIN (s) under point b? Please specify Page 10430 line 26 - Cryoconite area must be extremely difficult to estimate – how trust worthy are these results? Page 10430 line 26 - Further, I do not find it appropriate to use an average value for the transect 2 to79 km, with the argument that they not were statistically different. You have to use the actual measurements at each site multiplying it on TNicemeltopen otherwise you will bias the data to become more similar along the transect than actually justified by the data – Please correct that. Page 10431 Eq 3 – I find this calculation so imprecise that it is hardly worth doing. The key variable (NEP) is from another manuscript) – and it is very difficult to evaluate the robustness of these data. I don't mean statistically (SD's given in Table 2), but there must be a range of constant used to convert measured values to ugC g-1 d1 – (growth efficiency, carbon content average, cell size etc etc). Each of these vary with environmental controls and could potentially be very different along the transect. How well constrained are these NEP values? Did you validate the various "constants" that are required for the calculation? At least you ought to evaluate how much the NEP realistically could vary at a given site given a reasonable range of each "constant" – beside the given SD which presumably reflect SD on true replications. Further – is it really reasonable using a Redfield ratio given that it was developed for marine phytoplankton? The Redfield ratio is not a universal value, but may vary extensively in different environments. Again, I do not find it fair using the same average value of masscryoconite along the transect. To reflect the real variability along the transect you must use the values measured at the respective sites! In my

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opinion this must be fixed before potential publication! This also goes for eq 4!. Page 10431 line 23 – But have you also done that for the NEP assessments? Page 10431-10432 line 24 – Is it fair to use one value for all ice samples? See argument above. It is simply not fair using an overall SD for all measurements for evaluating the statistical robustness and then afterwards discuss if there is any variations or trends along the transect! Page 10432 line 20, the linear relation in ablation – is another assumption, that adds to the very loose "back on the envelope calculation" for the N budget/balance. Table 2 (Page 10432 Line 28)- were the "coverage" from Stibal et al 2011 – or primary data of this publication. That is unclear reading Line 28 at page 10432? Page 10433 (Fig 3) it is a bit confusing using DIN and No3- as synonyms – please use NO3- in figure legend if all other N- species were below detection limit. Page 10434 line 6 – Where does the TOC data comes from ? They were not described previously – or did I miss something? Section 3.4 I remain very sceptical on what we can learn from this calculation – the potential of N limitation is very poorly constrained. Further calling this very crude and simple calculation procedure for "modelling" is a bit much. I would suggest calling the calculations what they are! Page 10436 line 8 – This is in reality not in situ nitrogenase activity! Section 4.1 & 4.3 you may be right? But I do not find that the budget calculation provide any solid ground for your argumentation. It all remains very speculative. In conclusion I find that the manuscript could be published after major revision. This would include elimination of the very speculative budget calculations that basically are based on a "snap-shot" transect and a long list of very poorly constrained factors. I like the basic assessment of N2 fixation and its relative importance as an N source and do find that this merits publication.

Interactive comment on Biogeosciences Discuss., 8, 10423, 2011.