

Interactive comment on “Nutrient cycling in supraglacial environments of the Dark Zone of the Greenland Ice Sheet” by Alexandra T. Holland et al.

Anonymous Referee #2

Received and published: 29 April 2019

This paper provides novel information on the chemistry of supraglacial ecosystems. The main finding is that most of the dissolved N and P in these environments is in organic rather than inorganic forms. The authors use their chemical data in concert with measurements of algal cell abundance to make inferences about the role of microbes in supraglacial nutrient cycling. The paper is generally well written and would be of interest to biogeochemists, and to a lesser extent, hydrologists and glaciologists, working in ice-covered ecosystems. There are several sections of the paper that I felt were overly speculative, especially with regard to rates and mechanisms of nutrient retention. In addition, I believe that the authors could better reconcile their findings with previous literature on OM production in supraglacial environments. As a result, I think the paper needs some important revisions before it should be considered for

[Printer-friendly version](#)

[Discussion paper](#)



publication in Biogeosciences Discussions. I have provided comments and editorial suggestions below that I hope will be helpful for revising the paper.

General Comments

Line 99: It would be appropriate to report the number of samples collected for each habitat type somewhere in this section.

Line 101: How were sample locations classified into low, medium, and high impurity categories? The figure gives a sense of the density of impurities but there is no indication of whether there was some quantitative aspect to the process (i.e. number of impurities per unit area) or whether the process was wholly subjective. Also, the nature of the impurities is not well described – are they mineral, biological, or a mixture of both (such as the material found in cryoconite holes)?

Lines 179-181: The comparisons between algal cell abundance and organic nutrients are inconsistent. Algal cells and DOC are compared by regression, algal cells and DOC and DON are compared by pearson correlation, and algal cells are not compared at all to DOP. Moreover, these tests do not provide any information about the differences in the relationship between different habitats.

Line 184: What was the LoD for DON? Are the sample numbers you report (54 DON samples, 41 DIN samples) out of the 70 samples you included in the data for Figure 4? Also, what value did you use for all of the samples that were below the LoD – half of the LoD or some other value?

Line 194: What was the LoD for DOP? There were 74 DOP samples above the LoD, however in the legend for Figure 5 it appears that only 70 DOP samples were included in the figure.

Line 230: How do you get information about conversion rates from the concentrations you measured?

Lines 232-234: This regression plot is not an effective way to analyze the relationship

(or lack thereof) between DOC and algal abundance. The fact that there is any positive relationship is based on the single outlier in the upper right hand corner of the graph. If you removed that outlier, it appears that there would be a negative relationship between DOC and algal cell abundance (or, at best, no relationship). If there is, in fact, no relationship between algal cell abundance and DOC, that does not seem to support your statement that you “interpret these data to demonstrate that ice algal assemblages are the main producers of dissolved organic nutrient stocks within the melts surface ice...” (line 239). This may well be true but it is not what these data show. There are other possible explanations for the lack of relationship between DOC and algal cell counts including that you are comparing data collected across a full month and the relationship may change over the melt season.

Lines 234-237: Similar to the comment above, this explanation for the lack of a relationship between algal cell abundance and DOC would be more convincing if it detailed more specifically how these variables could become decoupled rather than just invoking the “highly dynamic nature of the environment” where solutes and gases move around.

Lines 267-274: It is surprising that cryoconite holes have low stocks of dissolved organic nutrients compared to surface ice. Past research has focused on cryoconite holes as hotspots of C fixation in autotrophic supraglacial environments (e.g. Anesio et al., 2009, Global Change Biology). If this were the case, it seems that the abundant production in cryoconite holes would be reflected in dissolved organic nutrient concentrations, but that is not what these data show. Does this suggest that surface ice habitats are potentially more important for autotrophic production or is there another explanation? Also, if you invoke EPS, which is known to occur in cryoconite holes, as the mechanism by which nutrients are retained in surface ice, wouldn't this also be true for cryoconite holes and drive up dissolved organic nutrient concentrations in the same way in those habitats?

Line 277: I don't find the argument for a “large pulse of dissolved organic nutrients” par-

[Printer-friendly version](#)[Discussion paper](#)

ticularly convincing. Particulate organic nutrients are hardly mentioned in this paper. It seems like a more parsimonious explanation for the loss of organic nutrients produced in supraglacial habitats is that they are exported downstream, at least partially, in particulate forms.

Editorial Suggestions

Line 77: change “to accumulate” to “accumulation”

Line 101: add “of” after “amounts”

Line 140: It would be helpful to define the acronym TON. I presume that it represents total oxidized nitrogen here but this acronym is commonly used to refer to total organic nitrogen (dissolved + particulate ON) so you should be clear about how it is being used.

Lines 179-180: This sentence refers to data shown in Fig. 7 (currently referenced on line 233), which should be renumbered to Fig. 4 and cited here.

Lines 189: “increase” should be “increased” to be consistent with the rest of the results which are in the past tense.

Line 234: Suggest changing “counts were” to “abundance was” since DOC is not plural.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2019-69>, 2019.

Printer-friendly version

Discussion paper

