

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 #1

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GENERAL COMMENTS: Overall, the manuscript by Holland et al. provides some important, hard-fought observations in one of Earth's least studied biomes, and provide some of the first evidence of the biogeochemical role played by the large seasonal algal bloom that develops on Greenland's Ice Sheet, which has recently attracted attention due to its influence on albedo. These data are therefore timely given the projected future mass loss of the Greenland Ice Sheet, and the consequences that these fluxes may have on downstream environments. Lastly, these present data are unique in that they seem to represent a relatively large spatial and temporal extent, and analytically, the methods employed for the data generation appear excellent.

However, I have some concerns with the way the data are described, interpreted, and reported. Firstly, I feel like the authors could do better job in focusing what exactly this

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paper is about, as the abstract, introduction, and discussion all give slightly different objectives for the study (see detailed comments below). I think that this manuscript would benefit from clarifying and focusing the objectives and hypotheses, and making these consistent throughout the document.

The second issue is in reference to the biogeochemical cycles/transformations hypothesized to be taking place on the surface of the icesheet. Some of the language in this regard could be tightened for accuracy and consistency (or at least clarified, see below comments), and I have suggested that the authors could create a conceptual diagram (with all inputs, outputs, transformations, etc) to help in presenting the hypotheses and afterwards discuss the data.

Thus, in revising this article, I challenge the authors to focus this research by asking specific, testable questions, and clearly using the data to answer these questions throughout the different sections of the document, as well as to pay careful attention to the biogeochemical transformations taking place in this special environment. Some specific comments are outlined below by section and line number.

SPECIFIC COMMENTS Title: Is the paper really about nutrient 'cycling'? Maybe something like 'organic nutrients dominate supraglacial environments and correlate with algal cell density...' or similar would better represent the subject matter of this paper.

ABSTRACT Line 19: Probably should be nutrient 'abundance' rather than nutrient 'cycling' that is a constraint on algal abundance. Also, do we know if nutrients are indeed a constraint on these communities?

Line 20: This paper does not really investigate the conversion of dissolved inorganic nutrients to organic ones; it more just investigates the abundance of each. We can of course infer that conversion is the reason for one form of nutrient over another, but most certainly conversion itself was not assessed.

Lines 21-22: Where are these percentages coming from. . .are these from the entire

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dataset? There was a gradient of algal abundance sampled over, as well as cryoconite and supraglacial stream categories. . . . it might be appropriate to describe the sampling scheme briefly in the abstract, and state which of these data were used to calculate these numbers.

Line 23-24: Can maybe be more specific here to indicate the shift from inorganic to organic forms rather than 'phase shift'.

Line 24-25: Again, what supraglacial environments are we referring to with these ratios? There are three values given after DON:DOP and DOC:DOP. . . . why three - what do they correspond to? Also, why were these ratios reported and not DOC:DON? Perhaps more importantly, why are only the organic forms being reported and compared with Redfield Ratio as opposed to inorganic forms?

INTRODUCTION Line 40 and 56: Particles of what? Given the potential importance of these particles in providing nutrients, I think they can be described in a bit more detail here. Are these the same particles described in lines 41-44 as being LAI's?

Line 60: Redfield et al., 1963 is an interesting choice for a reference, especially since it is regarded as being specific only to marine plankton in the discussion. Could maybe find something more broad and recent. . . . maybe the Ecological Stoichiometry book by Sterner and Elser (2002) would work better?

Line 60: Why is carbon in ready supply on the ice sheet surface; where is it coming from? Why would this not also be the case for nitrogen and phosphorus. . . . where are these two coming from and in what forms? Perhaps this is intuitive to the authors who are specialists for this ecosystem type, but would be good to describe some of these inputs/outputs to non-specialist readers of the journal.

Line 63: Does the 'Stibal et al. 2017a' citation go with the cell concentration number? If so, it might be better to move it there. . . . I'm not sure that paper suggests that these habitats are nutrient rich (but I could be wrong).

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Line 69: If there are some more examples than the Telling et al. 2012 paper, you should cite them here.

Line 71: What was the detection limit in this study (i.e. Telling et al. 2012)? Should report before the citation in the same units as your paper.

Line 73-75: This is more or less what you found for DIN, no? However, for DON, the values were much greater. I think it would be nice to revisit these ideas in the discussion.

Line 76-77: This sentence is a little confusing to me.how do cycles of uptake and remineralization lead to accumulation of nutrients in biomass? Also, I think there are potentially a lot of systems with microbially-mediated nutrient cycles that can be used as an analogue here.Planktonic aquatic systems are nice ones, but I don't think this is somehow the pinnacle of nutrient cycling.

Line 78: Maybe rephrase this.'extremely active nutrient cycling' sounds strange and unspecific to me. Would be better to give a rate estimate.

Line 79: I think this is something that you need to expand a bit more on, since the whole paper is essentially centered on it. Why are dissolved nutrients concentrating in the organic form, and is this really a sign of 'active' nutrient cycling? Later in the text, the opposite rationale is essentially used to explain the same observation, which is that low mineralization rates are responsible for an accumulation of organic nutrients. I think the authors would do well to describe the major inputs, outputs, and transformations in this unique habitat. Perhaps a conceptual diagram could help here, not only explain the rationale for this nutrient survey, but also help define your hypotheses/predictions?

Line 80-82: Isn't there organic nutrient data in Telling et al. 2012? It is likely that there are not so many reports of organic nutrients from the dark zone of the GRIS (it's not so easy to get there, afterall), but what about elsewhere on the ice sheet, or on other glaciers around the world? I think this is something, in concert with my comment above,

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that needs to be expanded upon ultimately given the content of this paper, an in order to appreciate the finding of this paper later.

Line 84: Do you expect that the ice algae are 'recycling' the nutrients, or just taking them up?

Line 88: I think you would need uptake data, for example, to actually evaluate the 'importance' of different nutrient forms. Also, when you say 'microbial' recycling, are you only talking about the algae?

METHODS Line 98: This is an extremely big area. How were sites randomly sampled (line 103) over such a large patch? Is there any sense of the area covered/sampled over this time? Were some sites/areas resampled over the month of fieldwork?

Line 99: Was there any relationship with nutrient concentrations and date sampled? I can imagine that conditions on the ice could be a lot different on the 15 of July than they are on the 15 of August.

Line 100: This explains why you sampled the surface ice in low, medium, and high categories, but did you really sample the cryoconite and streams due to the spatial heterogeneity in ice algae distribution? Algae were not quantified for these two habitats, so this is probably not the case. If it is just as a comparison with the surface ice that is fine, but some justification is warranted.

Lines 109-110: Was there any special preparation for the glass stack, bottles and collection jars? Eg. Acid washing, furnacing, etc?

Line 131: What was the purpose in assessing the assemblage diversity (as opposed to just a number of cells)?

Line 140: What is TON....total oxidized nitrogen? Should probably spell this out the first time.

Line 143: This is a bit confusing as written...why not say that DON was estimated by

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subtracting DIN from TDN since you already defined DIN above? Or would be easier to say DON=TDN-DIN?

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Line 166: Why cite RStudio here...Wouldn't it better to cite R?

Lines 166-172: In general, I think that it would be better to be more specific about what analyses were conducted and why. For example, can say in order to test hypothesis 'x', we performed test 'y'.

Line 170: Similar to the comment above, why test DON and DOC, but not DOP? Why were these parameters chosen, and how to they help you to achieve your objectives? For example, why would you not look at inorganic species, or the ratio of organic to inorganic forms as a function of cell abundance? Would it help to include sample date and spatial coordinates as random variables?

General comment: Was there any attempt to quantify particulates on the surface ice? While biological activity is no doubt important to biogeochemical cycling, so too would be the density of particulates I would think, especially with regard to phosphorus, since it is usually sediment-bound. While this paper of course focuses on the dissolved fraction, the particulate fraction is likely also important, and I feel like this would also help answer a similarly important question: are the nutrients in the forms they are because of the biological actors, or because of what the biological actors are sitting upon? This may also play a role in why some patches are in 'high abundance', and others are in 'low abundance', and thus would be collinear with cell abundance. Also, if a given sample was below detection, were they included in the analyses? They seem to be included in the figures, but would be good to know if they were also included in calculations, and if so which ones and how they were treated?

RESULTS General comment: I think it would make more sense if the results section was more hypothesis-lead as well. Right now, it reads more like a list with some carefully chosen significant relationships scattered about, and are difficult to understand how they relate to the overall picture.

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Line 176: In some ways, I feel like this opening sentence is really only validating the obvious. Transects were chosen based on the abundance of stuff covering them, and the first result is that more stuff was found in these patches covered with more stuff. I think that it would be more helpful to report it in this way such that it is setting up your experimental design rather than a unique result in its own right.

Line 179-182: Why are correlations with DOC and DON reported here and not below? Why did you not compare with DOP? Also, while an interesting result, I feel like calling them 'highly significant' is a bit excessive, since the relationships (as far as I can tell anyway) seem to be based upon 3 comparisons apiece (averages of low, mid, high). Would Pearson correlations be the correct test here, or would it be better to test against the categories?

Line 184 and elsewhere: Noting the number of samples that were over the LOD is great, but out of how many samples? What then happens to these below detection numbers....are they included in calculations? Also, are some of these replicates or from the same patches? Are these also including cryoconites and supraglacial streams? The authors need to be more specific in their reporting of these data.

Line 186: This is interesting....why do you think that NH4 was the dominant component of the DIN? Could this be from microbial ammonification of DON? I think this could be potentially also highlighted in a conceptual diagram!

Line 194-200: Again, why do you not make comparisons with abundance and DOP? it seems central to what you are trying to find out, whether or not comparisons are 'significant' (in either case its interesting). It is also not clear which samples you are talking aboutare they all pooled values for the icesheet as a whole?

Line 196: 'The mean concentrations for the remaining 40 DIP concentrations [that were above the LOD] ranged from 0-0.7'....the lower limit should be 0.02, since that was the limit of detection, right?

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Line 198: 'DOP concentrations in cryoconite hole and supraglacial stream water fell below the LOD'....How do you mean this...that they fell below the LOD sometimes? In Figure 5, the average DOP for these two habitat types is around 7 uM. DON is a different story....Could it be that these two are being confused?

General comment: There are several mentions of nutrient ratios in the abstract and discussion. Why are these not discussed in the results? Also, where is figure 7?

DISCUSSION Lines 212-214: This information should be in the results, and it should be specified how they are calculated. For example, are these calculated for only surface ice environments? Furthermore, I think that the ratios of organic to inorganic nutrients would be potentially equally or more interesting to correlate with algal cell abundance than the absolute concentrations.

Line 215: Has this dominance been reported in other glacial systems?

Line 222: Does Tedstone et al. 2017 actually report the timing of this shift in Nitrogen? Actually, has anyone reported this shift in nitrogen?

Lines 223-225: Similarly, how does this Williamson et al. (2018) paper support the shift in nitrogen phase? I think this needs to be rephrased/recast.

Lines 226: But, these other impurities were not quantified, so its difficult to say this for certain....For all we know, all the impurities could be ice algae! However, I think there may be some other papers showing this these days that you can cite...

Line 227: There is a lot of talk of nutrients 'shifting' to the organic phase. But, it looks like to me that the concentration of DIN is basically the same for all the surface ice habitat types. Might the DON rather be accumulating through time from ice algae taking up DIN and subsequently 'leaking' DON into their habitat, rather than the DIN pool shifting? It would really be nice to see these relationships over time.

Line 228: Furthermore, the big differences in organic/inorganic nutrients with algal biomass seems to only apply to nitrogen, and I think it is important that this distinction

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is made. Why would this not apply to phosphorus? This should be discussed in detail, and the authors should be more specific whether they are talking about 'nitrogen' or indeed 'nutrients' (ie nitrogen + phosphorus) elsewhere in the manuscript.

Line 230: Do the data really suggest 'efficient' conversion? I think at best there is a correlation between cell counts and organic nutrients, but no data that points directly to conversion, and definitely no data that would suggest that the process is efficient (for example, the DIN concentration seems unchanged with increasing cell abundance). Furthermore, why do you think the same would not be seen for DOP?

Line 232-233: I think this information belongs in the results section. Furthermore, Figure 7 is mentioned for the first time here. Maybe would it be better to put this in supplementary information if it is not going to be used to support the main results? Individual data points could also be superimposed onto bar figures (e.g. 'jittered' points in ggplot2) to illustrate variability between categories, if that is the goal.

Line 239: 'Demonstrate' is strong in this case. . . . perhaps 'suggests'?

Line 240-241: Are ice algae assemblages the main producers of dissolved organic nutrients stocks in freshwater and marine ecosystems? Recast this text.

Line 242: Do the ice algae really 'rapidly' take up inorganic nutrients? If there are some numbers to back this statement that is great, but I think this cannot be said without some support.

Line 243: I still think that it would help to somehow organize these sources in a diagram to help guide your thinking and the readers comprehension. What forms of inorganic nitrogen is deposited on the ice sheet and how? How about organic forms? Phosphorus?

Line 245: Can also be breakage, leakage, or lysis, for example. . . . what about extracellular processes?

Line 248: Does bacterial carbon production equate to nutrient-transformation pro-

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cesses like ammonification? If bacteria are really that sparse, I think you could alternatively think that they are really efficient, since they seem to be producing measurable ammonium in excess of uptake.

Line 251-254: 'Reduced capacity' is interesting wording. . . . were they at higher capacity at some point? I think the production of ON is just outpacing mineralization

Lines 257-259: This is interesting that all of these different habitats types studied by Stibal et al. (2008) also had the organic forms dominate. Why do you think this was not the case for Nitrogen in the supraglacial streams and cryoconites from this study, while it also it seems to hold true for phosphorus?

Line 271: Are ice algae producing EPS? Has anyone tried to quantify this?

Line 279-280: is it possible that DON and DOP are also 'over-wintering' on top of the icesheet? Could any of this be 'leftovers' from a previous season?

Line 280: This sentence is vague. . . . what exactly about the export of dark zone DOM is unknown. . . . the character. . . . the quantity?

Line 285: The Redfield Ratios was certainly generated using data from marine systems, but I think its utility over the last decades has been in providing a point of comparison. However, I think it also deserves clarification that the Redfield Ratio is the average molar ratio of biomass under balanced growth. Do we know the elemental composition of ice algae under balanced growth, and how it compares to Redfield Ratio? I'm also not sure that I understand the purpose of the text that follows. While there is certainly a lot of variability across aquatic habitats in dissolved N:P ratios from cold regions around the world (and elsewhere), I'm not sure how useful it is to bring up these numbers here. Furthermore, it is not clear if the ratios from the cited studies are also using the organic fractions-only as done in this study (my guess is that this is not the case). If the purpose of this text was to (presumably) link the reported N:P ratios discussed in the paragraph below to the literature, this makes comparisons difficult,

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and calls into question the need for this text, or at least would suggest that it needs to be revised to fit the authors' purpose.

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Line 295: This is the first time DOC:DON:DOP ratios have been reported besides in the abstract. . . . I did not see it in the introduction, methods, or results that you planned to look at these ratios.

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Lines 298-300: Why are you making nutrient ratios for the organic form of these nutrients? Wouldn't you expect that algae would be taking up the inorganic forms primarily (especially NH₄)? I feel like these ratios might not be accurately approximating availability for algae, and thus I'm not sure that, based on comparing these ratios with the Redfield Ratio alone, that we can say that the system is P-limited. I think it needs to be carefully explained in the text why this would be the case.

Lines 301-304: Would it be possible to more rigorously investigate this statement of different slopes of CP and CN over algal abundance? I think that this could be interesting if better developed, but as written it seems more of an afterthought.

Line 313: Is cryoconite the same as the particles talked about in the introduction?

Lines 326: In order to be able to say 'rapid uptake of dissolved nutrients', you need to have data on the uptake rates to compare. You also do not report rates of organic production.

Lines 328-329: These production data are also assumed to hold true here, as production wasn't investigated in this work. Also, why would it be inefficient. . . . because there are leftover organic nutrients?

Line 332: Was this the case for phosphorus? Also, I think that the notion of this retention being due to EPS is too speculative to say it this way.

Line 334: This is vague and repeated from line 280.

TECHNICAL CORRECTIONS Line 23: Comma after 'nitrogen' not necessary

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Line 30: Should there be spaces between values and “Gt”?

Line 36: Similarly, should there be a space between “30” and “km”? This should be fixed throughout.

Line 160: HCl

Line 214: comma after “To date”

Line 241: here and elsewhere, references should be ordered.

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

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