Overall response: We would like to thank the reviewer for the helpful and constructive review. We have made extensive changes to the text, particularly the Discussion, in line with the commentary below and that of the other Reviewer. We feel that the manuscript has been significantly improved as a consequence.

Reviewer comments and responses.

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 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.

- **Response:** We would like to thank the reviewer for their commentary regarding the clarification of the manuscript. We have significantly revised the manuscript and feel that this revision better follows the three main aims and objectives set out in the last paragraph of the introduction. A major restructuring of the results section has been carried out, to make the section more hypothesis driven, as well as to link more clearly to the objectives in the introduction and the subsections of the discussion. The discussion has also been rewritten to clarify meaning and refocus on the objectives of the manuscript.

The second issue is in reference to the biogeochemical cycles/transformations hypothesized to be taking place on the surface of the ice sheet. 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.

- **Response:** We hope that we have removed terms and phrases that could be misleading to the reviewer about what was actually quantified in the present study. A simple conceptual diagram has been included.

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.

- **General response:** We thank the reviewer for this challenge and hope that the revision now passes muster. Our responses to each individual question are given below.

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.

- **Response:** We agree and have changed the title.

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?

- **Response:** the text has been revised. Nutrient abundance in the Dark Zone has not been investigated in detail enough to definitively determine if it is a constraining factor on the bloom, which is why this manuscript investigates a limiting nutrient.

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.

- **Response:** Text changed from 'conversion of dissolved inorganic nutrients…' to 'abundance of dissolved organic nutrients….'.

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

- **Response:** The authors have added text briefly describing the five supraglacial environments sampled in the study. Please see lines 20-21 of updated manuscript. Text has been added explaining that the percentages have been calculated from across all of the ice surface samples containing low, medium and high visible impurity loadings. Please see lines 21-22 of updated manuscript.

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

- **Response:** This line has been deleted in updated manuscript.

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?

- **Response:** This line has been deleted in updated manuscript.

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?

- **Response:** The particles being referred to are considered to be dust, dated to the late Holocene by Wientjes et al., 2012, melting out of ancient meteoric ice. However, these particles are one example of mineralogic LAIs that could comprise the visible impurities seen in the Dark Zone, which is why the authors have also included a list of other mineralogic LAIs in Lines 53-54 of the

updated manuscript. In line 52 of the updated manuscript 'ancient Holocene dust' has been added as a descriptor of the particles being described.

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?

- **Response:** We have largely removed reference to the Redfield ratio, and note that information on the C:N:P ratio of glacier algae is sparse. We have added Hessen et al., 2013 as a additional reference.

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.

- **Response:** Carbon is in ready supply on the ice sheet surface for two main reasons, the first is that it is scavenged from the atmosphere during snow crystal formation and then is released to the surface ice environments when the snow pack ablates. The second is due to the surface ice environments constant interaction with the atmosphere. Due to the air-water interface during the main ablation season, gas exchange can occur which allows for carbon to be readily available. Both of these forms of carbon are in the dissolved inorganic phase, which includes aqueous CO_2, HCO_3 (bicarbonate), and H_2CO_3 (carbonic acid). Nitrogen is dominantly released to supraglacial environments via snow melt as nitrogen is also scavenged from the atmosphere during snow formation, with a lesser input from ice ablation. N_2 is also a potential source due to the air-water interactions occurring as mentioned before, however it is not very bioavailable and most photosynthetic organisms are not able to fix it from the atmosphere (Falkowski and Raven, 1997). Furthermore, Telling et al., 2012 reported that the overall importance of nitrogen fixation for microbial growth decreases with distance from the margin of the GrIS. Phosphorus is a rock dervived and is therefore only released by physical and chemical weathering of rock derived particles. Typically why it is the limiting nutrient in supraglacial environments. Lines 69-78 have been updated to include a more detailed explination.

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).

- **Response:** This line has been deleted in updated manuscript.

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

- **Response:** References to Telling et al., 2012 and Wadham et al., 2016 has been added to the end of lines 85-86 of updated manuscript as these presently are the only two studies to have quantified nitrogen concentrations in the Dark Zone of the GrIS.

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.

- **Response:** The LoD for the Telling et al., 2012 study was 0.33µM and has been added to line 89 of the updated manuscript.

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.

- **Response:** Section 4.1 of the discussion revisits these values and discusses the difference between the DIN and DON concentrations for this study.

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.

- **Response:** Uptake and remineralization describes what occurs in the microbial loop. As these microorganisms are utilizing and recycling the available nutrients, they become incorporated and accumulate into their cellular biomass as well as to being released back into the meltwater, which leads to nutrients not only existing in the environment in the inorganic phase but also in the organic phase in the form of biotic mass and dissolved organic matter that the cells produce. The authors chose to use planktonic aquatic systems as a comparison as it is similar to the aqueous ice surface environments.

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

- **Response:** Line 95 of the updated manuscript now cites an NPP rate from Williamson et al., 2018.

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?

- **Response:** We argue that DIN and DIP uptake by glacier algae and the production of EPS and other degradation products is the source of DOP and DON. This is the most consistent explanation from the data sets we present. A simple conceptual diagram is now included in the manuscript.

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

- **Response:** Text has been added that now cites Telling et al., 2012 and Wadham et al., 2016 as reporting TN for the Dark Zone, and clarifying that non has been reported for ice populated by

Streptophyte ice algae, lines 111-112. Lines 104-107 cite other sources reporting dissolved organic nutrient concentrations in other Arctic environments and the Antarctic.

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

- **Response:** This line has been deleted due to edits in the updated manuscript. We do believe that recycling is occurring due to the fact that heterotrophs are present. If heterotrophs are present then they are utilizing dissolved organic matter and therefore remineralization is occurring, albeit at an inefficient rate. Lines 102-118 begin to describe this conclusion.

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?

- **Response:** Text changed from 'importance' to 'relative abundance' in response to reviewer's comment, line 129 in updated manuscript. 'Microbial' recycling refers to the microbial loop and therefore both the algae and bacteria.

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?

- **Response:** Within each category of low, medium and high visible impurity loadings the sample location was chosen randomly by eye. There was no quantification of visible impurity loading before sampling however, as seen in Figure 2 the differences between the three ice surfaces are very apparent. Figure 3 reinforces this by the significant difference in algal abundance between the three ice surfaces. GPS points were collected at each sampling location within the 500 X 500 m sampling site, however no plots have been made to visualize the total area covered. Sampling areas were destructively sampled by the use of a hand saw to remove the top 2cm of the surface ice as described in the methods, therefore areas were not resampled.

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.

- **Response:** There was no clear temporal trend in the data. We believe that this is due to the extremely dynamic and heterogenic nature of these environments making trends over long time series difficult.

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.

- **Response:** Lines 144-145 in the updated manuscript adds text for clarification about supraglacial stream and cryoconite hole sampling.

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

- **Response:** Lines 158-160 in the updated manuscript adds text explaining the sample jar preparation.

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

- **Response:** Reference to assemblage diversity has been removed.

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

- **Response:** Line 185 does this.

Line 143: This is a bit confusing as written. . .why not say that DON was estimated by subtracting DIN from TDN since you already defined DIN above? Or would be easier to say DON=TDN-DIN?

- **Response:** Text has been changed for clarification, line 193.

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

- **Response:** RStudio is considered an IDE, integrated development environment which cannot be run without R. However, R is an independent program, which can be run without RStudio.

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'.

- **Response:** We feel the text does say this. Please push back if you still feel that it doesn't.

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?

- **Response:** Line 250 of the updated manuscript now state: 'Comparison of DOP surface ice concentrations and algal cell counts were not significant.'. Only significant relationships were reported in the manuscript. The authors chose to compare the average algal abundance to the average DOC and DON concentrations for the three ice surfaces as a way of illustrating a relationship between the glacier algae abundance and the concentration of DOC/DON. We feel that this helps achieve the objective of showing that algae are the main producers of DOC and DON in the ice surface environments.

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?

- **Response:** We agree with the reviewer about the potential importance of particulate nutrients in supraglacial environments, however, it was simply outside the scope of this study as we aimed toward understanding the dissolved phase. There is a large companion paper that investigates the mineralogy of the particles that comprise the surface impurities which is about to be submitted for review, and we will make reference to this following its submission and this second review of our manuscript. Investigation into the potential phosphorus input from particles in cryoconite holes has been investigated by Stibal et al. 2008. Text has been added in line 213 that states "Samples resulting below the LoD were considered 0 μM.".

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.

- **Response:** The text in the results section has been completely reorganized with new sub-headings in order to make the reasoning and hypotheses clearer. Please refer to the results section in the updated manuscript (section 3) for the reworked text.

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.

- **Response:** We believe that we need to make this is an important distinction. No quantification of the particulate content of the visible impurities was made. It is therefore important to state that not only did the algal cell abundance increase with the amount of visible impurities but that the differences in abundance were statistically significant. This also provides justification for the sampling method we employed.

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?

- **Response:** Since the reorganization of the results this correlation is now under the "Links between algal abundance and dissolved organic nutrients" subheading: please refer to section 3.2 of update manuscript. Please refer to above comment regarding the lack of DOP comparison. The Pearson correlation test was used as an initial test to illustrate a relationship between DOC/DON concentration and algal abundance. The term 'highly significant' has been removed.

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.

- **Response:** Text has been added at the start of results section 3.1 noting the total sample number for each nutrient for all five supraglacial environments sampled. Please see above comment with regards to values assigned to samples below the LoD. As stated earlier, sample locations were destructively sampled so the same patch was never sampled again. Two different samples each of the low, medium and high visible impurity ice were collected each sampling day, they are treated as individual samples in the data set.

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!

- **Response:** Ammonification was not quantified, and so we are unable to definitively say the cause of the ammonium dominance. Telling et al. 2012 noted that the presence of NH4 in cryoconite hole samples might be an indication of active organic matter remineralization.

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 about . . .are they all pooled values for the ice sheet as a whole?

- **Response:** Please refer to above comment regarding the lack of DOP comparison. Section 3.1 of the results has been rewritten and text has been added explaining which samples are being referred to.

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?

- **Response:** Samples that fell below the LoD were considered to be 0 μM, which is why our sample range begins at 0 μM.

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?

- **Response:** The text has been changed and the comment moved to line 262.

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?

- **Response:** Nutrient ratios have now been added to the result section 3.2 in response to the reviewer's comment. Figure 7 from the original version has been removed from the manuscript in response to reviewer 2 comments. Figure 7 is now the conceptual diagram.

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.

- **Response:** Percentages have been removed from the discussion and added to results section 3.1 with text added to explain which samples the percentages are referring to. See lines 230-231 in updated manuscript. The revised text discusses the increase of DON:DOP and DOC:DOP ratios with increasing visible impurities, lines 351-354 of discussion.

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

- **Response:** Yes, dissolved organic dominance is commonly reported for cryoconite hole environments (Stibal et al., 2008; Telling et al., 2014). Lines 305-309 in updated manuscript describe dominance of dissolved organic nutrients in other glacial systems and its relation to microbial activity in the environment.

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

- **Response:** Text revised.

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.

- **Response:** The authors have reworded the sentence. Please refer to lines 286-287 in updated manuscript.

Lines 226: But, these other impurities were not quantified, so it's 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. . .

- **Response:** Text changed in response to reviewer's comment. Yallop et al., 2012 has now been quoted as reporting a particle: cell ratio of 3:2 in the dark zone of the GrIS. Please refer to lines 292-293 in updated manuscript.

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.

- **Response:** The authors would like to clarify that the use of the term 'shift'. With regards to the nitrogen nutrient pool the author's use of 'shift' has to do with the snow and ice core data that show a dominance of DIN with little to undetectable levels of DON. Yet, as the season progresses the dissolved nutrient pool is dominated by the dissolved organic phase, showing that something is occurring at the ice surface to cause the nitrogen pool to change. The authors agree that it is very likely the ice algae up taking the DIN, utilizing it and producing DON, which is the main argument of this paper: ice algae are the drivers in this conversion of nutrients.

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 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.

- **Response:** The revised text hopefully makes this clear.

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?

- **Response:** Text changed in response to reviewer's comment and the term conversion has been removed. Please refer to line 268 in updated manuscript. Discussion of changes in DOC:DON:DOP ratios can be found in Section 4.4.

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.

- **Response:** Figure 7 from the original version and linear regression relationships have been removed from the manuscript in response to reviewer 2 comments.

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

- **Response:** This sentence has been deleted in revised text.

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

- **Response:** This sentence has been deleted in revised 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.

- **Response:** This sentence has been deleted in revised text.

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?

- **Response:** The authors have produced a conceptual diagram in response to reviewer's comment. The diagram depicts likely nutrient inputs to supraglacial environments, ice algae producing dissolved organic N, P and C and inefficient remineralization by heterotrophs. The diagram is simple due to the fact that many aspects of nutrient input, cycling and export in the Dark Zone of the GrIS still remain unknown and was one of the main objectives of this paper, to produce a preliminary dataset of dissolved inorganic and organic nutrients for this region. The authors fear that by making this diagram overly detailed it could be misleading as not enough research has been done in the Dark Zone. Please see Figure 7 for conceptual diagram.

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

- **Response:** The authors included ‘decomposition of the ice algae’ to account for the breakage, leakage or lysis input of dissolved organic nutrients. Extracellular processes such as the production of EPS is addressed in section 4.3 of the discussion.

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

- **Response:** It is possible that depletion of nitrate and higher levels of ammonium could suggest ammonification, but it would only be speculation within the constraints of this manuscript. Furthermore, the authors would also like to clarify that the manuscript comments on bacterial production rates in comparison to net primary production, not bacterial abundance. Nicholes et al. (2019) reports bacterial abundance as $3.3 \pm 0.3 \times 10^5$ for surface ice samples taken during the same field campaign as the present study. This shows that bacteria are abundant, but not active.

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

- **Response:** This sentence has been deleted in revised text**.**

Lines 257-259: This is interesting that all of these different habitat 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?

- **Response:** The authors believe that there could be differences between the two studies due to retention by surface ice microbial communities. One conclusion of this manuscript is a retention ability by the microbes in the surface ice to hold dissolved organic nutrients at the surface via the production of EPS. As EPS contains N, it is likely that N is being retained at the ice surface as opposed to being transported through the water table. DOP is also exuded in the form of EPS, but actually the difference in DOP and DIP in supraglacial streams is not statistically different. The concentrations of DIN/DON and DIP/DOP in cryoconite hole water are not statistically different either.

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

- **Response:** To the authors knowledge, quantification of ice algal production of EPS has not been conducted, but Yallop et al. 2012 identified EPS in surface ice samples dominated by ice algae.

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?

- **Response:** We believe that some DON and DOP can remain in the ice surface at the end of the ablation season and remain frozen until the next season. We use Musilova et al. 2017 to provide an example that this has already been shown for DOC. Please refer to lines 332-340 of updated manuscript.

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

- **Response:** This sentence has been deleted in revised text.

Line 285: The Redfield Ratio 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, 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.

- **Response:** We agree that the text following the Redfield Ratio in the original manuscript may cause confusion for the readers and it has been removed. We know of no published literature on the elemental composition of ice algal under balanced growth.

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.

- **Response:** Text has been added to the updated manuscript to include ratios in the results section 3.2. Line 130 of the introduction now describe our intent to investigate nutrient ratios in the manuscript. We do not believe that it is appropriate to include the ratios in the methods section.

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.

- **Response:** Please see revised Discussion Section 4.4.

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.

- **Response:** Please see revised Discussion Section 4.4.

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

- **Response:** Cryoconite is part of the LAIs discussed in the introduction. Cryoconite holes can melt out or be flushed out throughout the season which causes the cryoconite debris to be washed over the surface. The particles referred to in the introduction describe dust ablating out from meteoric ice as reported by Wientjes et al. 2011.

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.

- **Response:** "Rapid" has since been removed from the sentence. We use the term "high production of dissolved organic production" to refer to the high concentrations of C, N and P produced not to imply any rate at which the production is occurring.

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?

- **Response:** An efficient microbial loop has similar rates of NEP and secondary production, which results in more balanced concentrations of dissolved organic and inorganic nutrients. The fact that there is such a dominance of dissolved organic nutrients implies that remineralization rates are low/inefficient. Nicholes et al. (2019) is cited here because they determined a 30:1 ratio for the same surface ice samples reported in this study, as this manuscript focuses on the geochemistry.

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.

- **Response:** We believe that the retention of dissolved organic nutrients via the production of EPS is a viable hypothesis. Please see revised Discussion Section 4.4.

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

- **Response:** This sentence has been deleted in revised text.

TECHNICAL CORRECTIONS

Line 23: Comma after 'nitrogen' not necessary

- **Response:** Text changed in response to reviewer's comment.

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

- **Response:** Spaces have been added between values and units throughout the updated manuscript.

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

- **Response:** Please refer to above comment.

Line 160: HCl

- **Response:** Text changed in response to reviewer's comment.

Line 214: comma after "To date"

- **Response:** Text changed in response to reviewer's comment.

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

- **Response:** Text changed in response to reviewer's comment.