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.

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

- **Response**: The authors would like the thank the reviewer for their in-depth assessment of our manuscript and for providing beneficial comments for the restructuring of the manuscript. We direct them to our responses to each individual question below.

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

- **Response**: We now include the sample sizes of each habitat.

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)?

- **Response:** Sample locations were determined visually as the difference in the impurity loadings was quite apparent, as the authors tried to show in Figure 2. There was no quantitative process conducted on the ice surface prior to choosing the sample location, however Figure 3 reinforces the validity behind choosing sights visually as there was a significant increase in algal abundance between the low, medium and high visual impurity ice. There was no further analysis of the impurities beyond Yallop et al., 2012, who quantified a 3:2 particle: cell ratio for their samples collected in the Dark Zone. Furthermore, a companion paper is being produced that investigates the mineralogy of the impurities collected.

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.

- **Response**: Only Pearson correlation is reported in the revised manuscript, and only significant relationships were reported. DOP did not correlate significantly with algal abundance. ANOVA analysis is included to provide information about the differences in nutrient concentrations between 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?

- **Response**: LoD for DON is 0.87 µM and is included in the revised text. Lines 229-230 have been added for clarification about the number of samples for each test. Values below the LoD were considered to be 0 µM, line 214 has been added for clarification.

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.

- **Response**: LoD for DOP is 0.02 µM and is included in the revised text, as are the correct number of samples.

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

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

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.

- **Response:** We would like to thank the reviewer for pointing out this oversight on our part. The plot and linear regression analysis have been removed.

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.

- **Response**: We agree and have further elaborated on weathering crust dynamics have been included.

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?

- **Response:** We thank the reviewer for this commentary. We have made substantial revision to the Discussion and decided to concentrate mostly on differences between macronutrient concentrations in the melting surface ice environments. We felt that a discussion of processes in cryoconite holes detracted from the main message of the paper, and so have not included these types of ideas here. However, we fully agree with the reviewer that this is a very interesting idea. It is likely that the melting surface ice does fix more carbon by dint of the greater surface area, but this is not the main thrust of this paper.

Line 277: I don’t find the argument for a “large pulse of dissolved organic nutrients” particularly 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.

- **Response:** We agree and have downplayed this idea in the revised manuscript.

Editorial Suggestions

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

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

Line 101: add “of” after “amounts”

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

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.

- **Response:** Text changed in response to reviewer’s comment, line 186 now reads: “NO_2^- and total oxidized nitrogen (TON) (NO_2^- + NO_3^-)…”.

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.

- **Response:** Figure 7 has been removed from the manuscript after review from the above comment and has been replaced with a conceptual diagram.

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

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

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

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