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Interactive comment on "A spatial investigation of the environmental controls over cryoconite aggregation on Longyearbreen glacier, Svalbard" *by* H. J. Langford et al.

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We would like to thank both reviewers for their positive and thought-provoking commentary on the article, the commendation for developing a useful experimental approach and their comments on the suitability and importance of the research. In our response below, we cite the comments of the reviewers and respond beneath each in turn. Reviewers comments are labelled as #R1 (Dr J. Cook) and #R2 (anonymous referee 2):

1. To generalists the significance of cryoconite aggregation might not be obvious, and it is not well explained in the manuscript. Some early illustration of the importance of this research would be useful. Some contextualisation of the findings of this study in

the discussion would also help to highlight the relevance to wider glaciological, microbiological and climatological research (#R1).

To supplement existing treatment of this topic (e.g. Page 3425, lines 6-9), we have further illustrated the biological significance of cryoconite and some discussion of its net ecosystem productivity, as well as incorporating some further references (Cook et al. 2010; Zarsky et al. 2013). Further contextualization within the discussion has also been added, at page 3446, line 26 and in the subsequent paragraph.

2. The degree to which the filament length measurement process was automated is not clear in the text (#R1).

We have amended the methods section on page 3432 to include a detailed description of this process.

3. Cryoconite holes and cryoconite debris show stark contrasts in their morphology and biology even between adjacent glaciers (Edwards, 2013), so a caveat about the representativeness of cryoconite sampled only on Longyearbreen is required (#R1).

In order to address this concern, the authors have added a caveat into the introduction at line 13, page 3425, referencing Edwards (2013).

4. How reasonable is the assumption that the chlorophyll a concentration vs. absorbance curve derived from spinach is applicable to cryoconite? If it is a standard procedure some supportive literature could be cited (#R1).

Chlorophyll assays using highly purified chlorophyll from spinach are standard procedure. To support this, the authors have cited one of the original and most highly cited journal articles relating to this procedure (Porra et al. 1989), which itself outlines the accurate assaying of chlorophyll a in a variety of solvents, including methanol and acetone.

5. The comparison of phycobiliprotein extraction techniques seems like a useful addition to the literature that should receive mention in the discussion. This paper indicates

the paucity of studies employing phycobiliprotein as a biomarker, and some further explanation might help to address this (#R1).

On page 3442, line 7, the authors have added: “Phycobiliproteins have received little research attention, largely due to difficulties in achieving an efficient extraction. With those difficulties now being overcome (Lawrenz et al. 2011), and with metagenomic data becoming available (Edwards et al. 2013), there is a real needed for further, specific, functional studies of cyanobacteria and their light-harvesting in natural aggregate systems.”

6. The term ‘photic zone’ requires definition early on in the manuscript, perhaps in reference to the spatial variability of IR receipt in section 3.1. It is not immediately clear that you are referring to areas of the glacier surface that contrast in their cumulative solar radiation receipt (#R1).

We have now referenced Irvine-Fynn & Edwards (2014) and briefly defined photic zone on page 3442 as the portion of a water/ice body receiving sufficient sunlight for photosynthesis.

7. Page 3425, line 20: Add reference Yallop et al, 2012 (#R1).

Reference added.

8. Page 3439, line 10: consider changing "As such, and given the patchy nature of phototrophic bloom activity, this activity is likely to preferentially raise carbohydrate concentrations in "hotspots" related to earlier bloom activity." to "As such, and given its patchy nature, phototrophic bloom activity likely preferentially raises carbohydrate concentrations in "hotspots" associated with earlier blooms." (#R1)

Changed as suggested.

9. Introduction & methods - I considered the paragraph discussing about the phycobiliprotein measurements is too extensive and not needed in this section. Introduction needs to focus on the main objectives of the study (#R2).

This suggestion is in direct conflict with that of the other reviewer; in fact, Dr Cook suggests further discussion of the paucity of phycobiliprotein data in the discussion. Mindful of this, the authors feel that the short, yet frank, outlining of the utility of, methods for, and usefulness of phycobiliprotein measurements sits well within the paper, is not too extensive, and hopefully only acts to promote further metabolomics measurement of bioaggregates, including all of their light-absorbing proteins. The authors feel that there is scope for further study into the broad-spectrum light-absorbance within bioaggregates and would hope that our frank discussion of the difficulties in measuring phycobiliproteins only further assists others in their research.

10. Results - In figure 2, the chlorophyll a pattern (increase down-glacier) is not as evident as the authors claim. Take care with the over interpretation of results. The same for phycobiliproteins. Indicate GT and SL in the figures (#R2).

The authors are very mindful of the important point raised here by the reviewer, as they do not wish to be seen to be over-interpreting the results. The pigment data was difficult to interpret and the authors agree that perhaps their viewpoint, that the data as a whole shows high heterogeneity, yet certainly the highest pigment concentration values are found towards the glacier terminus, has been slightly lost. As such, we have toned down some of the wording and interpretation within the results section in discussing heterogeneity of the spatial data concerning pigment concentrations, and the consequent lack of strong spatial trends, a fact backed up by the CCA data in Figure 3. We thank the reviewer for their precautionary advice. Further, GT and SL have been added onto Figure 2a.

11. I consider that CCR is just another way to show carbohydrate distribution and does not provide additional information (#R2).

We appreciate the reviewer’s opinion in that CCR does re-show carbohydrate distribution, however we believe it is important to compare the ratio between carbohydrate and chlorophyll to provide further, important details. As discussed in the results and

discussion sections, it visually highlights those areas of the glacier's ablation zone that are particularly dominated by carbohydrates whilst lacking chlorophyll. This is important, as outlined in the journal article, as it suggests allochthonous carbon inputs from the valley sides or could even suggest the possibility of stress-related carbohydrate production by photosynthetic microorganisms struggling to efficiently photosynthesise using chlorophyll, needing to scavenge nutrients and/or protect themselves from UV radiation and freeze-thaw. As such, and given the relatively small amount of text focused upon the CCR, we have retained the CCR data.

12. For easy comparison and interpretation, to include all correlations in the table (#R2).

The authors would point this reviewer to Table 2, which contains all of the correlations and is cross-referenced at page 3436, lines 8 and 14 and page 3437, line 4.

13. Move paragraph in page 3436, line 20 to discussion (#R2).

Since this paragraph presents primary data and some brief statistical analysis, in the form of correlation, and discussion is very limited, we feel it belongs in the results section rather than complicating the discussion section with new data.

14. Move paragraph in page 3437, line 25 to methods (#R2).

The authors agree with this suggestion, as this paragraph does cover further statistical methods used. However, in the context of its present setting, the authors feel that the paragraph acts as a barrier between two separate statistical datasets, both bridging the two and aiding readability by avoiding confusion. To address this, we have moved this paragraph into the methods and replaced it with a bridging sentence at the beginning of the paragraph currently atop page 3438.

15. Discussion – As authors indicate, contrary to previous works, they focused on explaining how some biochemical processes could control some physical characteristics of the cryoconite holes. However, in the first part of the discussion (pages 3438-3442)

they clearly exposed how their results indicate that biological parameters respond to stability and size of cryoconite. Considering the characteristics of the ice matrix and the ablation in glacier surfaces, I consider that this explanation is the most plausible. In spite of this, I agree with the idea that the evolution of microbial community can impact cryoconite stability and at the same time to conduct to a more mature community (e g. high-size filamentous microorganisms). In contrast, I can't figure how community evolution can influence the size of the aggregates. I am in more agreement with a model where feedbacks between some physical and biological parameters can explain the evolution of the cryoconite aggregations (#R2).

The authors thank the reviewer for this helpful summary. Upon reflection, although we specifically comment on the stability of the weathering crust environment and the physics of the glacier surface in driving certain biological parameters, such as photosynthesis, there is perhaps room for some misinterpretation here and we perhaps do not draw upon the feedbacks and inter-relationships between physical and biological parameters enough throughout the paper. As such, we have added text into the introduction, on pages 3425 and 3427, reinforcing the dynamic nature of the supraglacial environment and essentially asking the question 'how do biological forming factors, through feedbacks with the physical environment, control aggregate size and stability'. This has helped the observations and discussions between pages 3438-3442 sit much better within the article, and the authors thank the reviewer in helping us to achieve this. Furthermore, on page 3447, from line 6, this text has been altered to better convey the fact that interplay between biology and the physical environment is important. We feel that this addresses the concerns of the reviewer, whilst not detracting from the authors' intended focus of the paper - the idea that biochemistry is important to the size and stability of aggregates at the spatial scale.

16. It would be interesting to include measurements of total carbohydrate to support statement in page 3443 (line 25) about the increase of bound carbohydrates downglacier (#R2).

When organising the initial sampling procedure, sample volumes had to be kept small and these volumes were decided upon on the basis of assessed need. During the phase of laboratory analysis, total carbohydrate analysis was discussed. However, difficulty in extracting phycobiliproteins combined with these small sample volumes meant that the material for accurate total carbohydrate analysis was not present. After discussion with the co-authors, it was decided that any re-sampling would not be truly representative due to temporal changes in this very active nutrient pool.

17. Is there a significant correlation between carbohydrate concentration and community respiration? (#R2)

Table 2 indicates that, along the centre-line transect, there was no significant correlation between carbohydrate concentration and community respiration. The authors agree with the reviewer that there may be an identifiable correlation between carbohydrate concentration and community respiration, though suggest that a temporal study would be best placed to identify this, given that the net ecosystem productivity detected in our incubations was representative of the 24h period over which they were taken, yet carbohydrates can be accumulated over a longer period of time. As such, the authors feel that the approach taken in the research – using spatial measurement of key biochemical parameters as a ground-truthing exercise, before following this up with a transect study incorporating further biogeochemical data – is appropriate and provides for useful conclusions relating to both the spatial variability in biogeochemistry and the effects of these parameters upon aggregate stability and size.

Interactive comment on Biogeosciences Discuss., 11, 3423, 2014.