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Interactive comment on "Seasonal changes of ice surface characteristics and productivity in the ablation zone of the Greenland Ice Sheet" *by* D. M. Chandler et al.

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The paper is generally well written and addresses the concern about autochthonous biomass change and its feedback to albedo on the GIS. It is quite right to point out that previous work on the subject has not adequately constrained spatial and temporal variability, making this a potentially important study. However, the combination of the AVHRR image work and the plot scale biological process work is almost meaningless and spoils this paper. Significantly, the authors did not measure albedo on the ground during the field surveys. However, they do present a good (almost complete) seasonal data set showing biological production, and the calculation of the areal carbon balance

from this. In spite of being conducted at the plot scale, it is worthy of publication in my opinion. I am not sure what to suggest about the remotely sensed albedo work though – it does provide some useful background information to the study and so would perhaps be best used to describe the seasonal changes in the general area and assess if the summer was indeed any different to previous ones (as is suggested).

The seasonal dataset the group acquired from the inland ice site is a good achievement, which could be really useful if tied more closely to the ice surface characteristics (which are presently only described qualitatively for the different periods). If available, I would welcome observations on the depths/thicknesses of cryoconite holes, superimposed ice and the "weathering crust" in order to better understand these links. Cryoconite hole depths greatly influence the incident radiation receipt by photosynthesising microorganisms and it is about time that we started to measure how the depths are controlled by factors that are both extrinsic (e.g. the balance of sensible heat and incident radiation) and intrinsic (optical transparency of superimposed ice and weathering crust ice; debris albedo etc) in their nature. Specific points:

1. P1341: comments on surface roughness. These comments are interesting but irrelevant. You could have included roughness estimates derived from using the microtopographic method in your plot though. Roughness induced by cryoconite holes is an insignificant melt/dust capture feedback I suspect, but hummock-induced roughness is probably important and might influence the spatial pattern of cryoconite hole formation. Make up your mind whether you want to write about roughness or not, and consider its wider implications.

2. P1345 and Table 2. I think you should almost certainly consider superimposed ice as a surface type. It is conspicuous by its absence and something that I was looking for in your seasonal study. See also below where I ask whether superimposed ice melt out is related to debris mobilisation or not. Did you record the cryoconite hole depths and their seasonal change?

3. P1346. Incubations were placed in “a hole”. What depth? It is very tempting to sample cryoconite from a range of different holes (with different depths) and then incubate them all in a single large pool. In fact this can be a useful experiment in its own right, but I wanted to know more about the holes you sampled (their depths and debris cover thicknesses) and the incubation environment because this will contribute to the scatter (or lack of relationship) seen in Figure 11. You also state that you could not estimate debris thickness reliably, which is a shame. You can image the hole (giving area) and it would have been sufficient to derive a thickness estimate after carefully sampling and estimating the volume. However, I guess this could have been too destructive? I think Telling et al (2012) shows that it is really important we consider cryoconite debris layer thickness due to self-shading effects. Further, if cryoconite holes are full of discrete aggregates, then knowing if the debris layer has arranged itself into a single layer following thermodynamic equilibration in the manner suggested by Cook et al (2010) is also important.

4. The stoichiometric O2/C ratio issue was also discussed for GIS cryoconite holes by Hodson et al (2010), who provided the first evidence that the respiratory quotient is close to unity. This (and Telling's more detailed data set) justifies your reliance upon O2 measurements for biological production, although I would recommend the use of both DIC and O2 change to avoid some of the data problems encountered in your study.

5. P1354. I appreciate your point, but photography still captures the hole – even if the debris is not visible. We use field notes for this sort of problem to support the image analysis later on.

6. Figure 10 and bottom of p 1357. Some readers will wonder why all your initial O2 measurements appear to be undersaturated. Was that always the case and is it worth speculating why?

7. P 1355, bottom. It is well known that holes persist from one year into the next but the reference here is for the lidded Dry Valley holes rather than more representative

Arctic glacier holes. It's all to do with the relative importance of sensible heat – if it is low at the end of summer then deep holes will form and persist if the surface receives sufficient solar radiation. Later on p1356 you suggest that “most” holes survive over winter. I didn't think you should say this unless you surveyed the pre-winter cryoconite holes.

8. Despite the minor issue above, I liked this discussion because it shows the importance of being present on the ice making careful observations about its changing surface. Our experience in maritime environments is that when the superimposed ice is completely burned off by ablation, then you can expect an initial mobilisation event. This is certainly the case in Svalbard, where sensible heat is significant and combines with low solar angles at the end of summer to minimise hole depths prior to winter. It would therefore be interesting to know how the loss of superimposed ice contributed to your mobilisation event on p1350.

9. On p1358 you talk about the incubation of dirty ice and make the point about the incubation environment being rather different to reality. In the spirit of the interactive nature of this journal (rather than to harshly criticise your work) I would suggest the development of chambers for this measurement. We have had success in Svalbard where a large skirt around the chamber can be used to seal the environment. You can them employ CO2 measurements rather than O2-based wet incubations.

10. Section 4.2 is too speculative (last sentence – albedo retrieval and cryoconite holes)

11. P1360: I would mention that yours is a seasonal study, whilst Hodson et al (2010) was an end of season study when ice lids were forming and the transmission of light through the glacier ice walls toward any cryoconite was reduced by snow cover. It is a shame therefore that your incubations did not continue after the early snowfall of 31st August to see if the system became net heterotrophic.

12. I didn't think that Section 4.4 was informative – not least because most of the focus

is upon dirty ice. Do you think that the cloudy weather (and warm air advection) are sufficient to destroy cryoconite holes and form the dirty ice? If so, then not being able to detect this with areal albedo estimation from space would be frustrating.

13. You use the term “distinct periods”. The second P1 was certainly distinct from the first, so why use the same label?

Interactive comment on The Cryosphere Discuss., 8, 1337, 2014.